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INDEX TO THE ECONOMIC JOURNAL

VOLUME LXIX

ARTICLES	PAGE
Aschheim, J., Open-market Operations <i>versus</i> Reserve Requirement Variation ...	697
Bauer, P. T., International Economic Development ...	105
Bellerby, J. R., National and Agricultural Income 1851 ...	95
Bhagwati, J. N., Protection, Real Wages and Real Incomes ...	733
Black, J., A Savings and Investment Approach to Devaluation ...	267
Clower, R. W., Some Theory of an Ignorant Monopolist ...	705
Corry, B. A., Malthus and Keynes—A Reconsideration ...	717
Dell, S., Economic Integration and the American Example ...	39
Dosser, D., The Formulation of Development Plans in the British Colonies ...	255
Farrell, M. J., The New Theories of the Consumption Function ...	678
Fogarty, M. P., The White-collar Pay Structure in Britain ...	55
Gottlieb, M., Price and Value in Industrial Markets ...	22
Grey, A. L., and Brockie, M. D., The Rate of Interest, Marginal Efficiency of Capital and Investment Programming: A Rejoinder ...	333
Hahn, F. H., The Theory of Selling Costs ...	293
Hall, Margaret (Lady), and Winsten, C. B., The Ambiguous Notion of Efficiency ...	71
Hall, Sir Robert, Reflections on the Practical Application of Economics ...	639
Hallett, G., The Economic Position of British Agriculture ...	522
Harrod, Sir Roy, Domar and Dynamic Economics ...	451
Klein, L. R., and Ball, R. J., Some Econometrics of the Determination of Absolute Prices and Wages ...	465
Lundberg, E., The Profitability of Investment ...	653
Matthews, R. C. O., Duesenberry on Growth and Fluctuations ...	749
Meyer, F. V., The Terms of Trade of Manufactures ...	507
Morag, A., Is the "Economic Efficiency" of Taxation Important? ...	87
Oshima, H. T., Income Originating in the Models of Harrod and Domar ...	443
Patel, S. J., Export Prospects and Economic Growth: India ...	490
Pearce, I. F., A Further Note on Factor-Commodity Price Relationships ...	725
Penrose, Edith, Profit Sharing Between Producing Countries and Oil Companies in the Middle East ...	238
Prest, A. R., The Expenditure Tax and Saving ...	483
Richardson, G. B., Equilibrium, Expectations and Information ...	223
Robinson, E. A. G., Comment on Mr. Hallett's Article on the Economic Position of British Agriculture ...	540

ARTICLES (*continued*)

PAGE

Robinson, Joan, Accumulation and the Production Function	433
Rose, H., The Possibility of Warranted Growth	313
Spraos, J., Speculation, Arbitrage and Sterling	1
Stuvel, G., Asset Revaluation and Terms-of-Trade Effects in the Framework of the National Accounts	275

REVIEWS

Bauer, P. T., <i>Economic Analysis and Policy in Underdeveloped Countries</i> . By G. M. Meier ...	347
Bauer, P. T., and Yamey, B. S., <i>The Economics of Underdeveloped Countries</i> . By G. M. Meier	347
Baum, W. C., <i>The French Economy and the State</i> . By P. Bareau	555
Bernal, J. D., <i>World Without War</i> . By B. R. Williams	560
Black, D., <i>The Theory of Committees and Elections</i> . By A. D. Roy	367
Bonné, A., <i>Studies in Economic Development</i> . By A. K. Sen	134
Bowman, M. J. (Ed.), <i>Expectations, Uncertainty, and Business Behavior</i> . By F. H. Hahn	777
Brittain, Sir Herbert, <i>The British Budgetary System</i> . By A. T. Peacock	772
Brown, A. J., <i>Introduction to the World Economy</i> . By E. V. Morgan	551
Buchanan, J. M., <i>Public Principles of Public Debt</i> . By A. R. Prest	358
Burn, D. L. (Ed.), <i>The Structure of British Industry</i> . By J. Downie	144
Cartier, C. F., and Williams, B. R., <i>Investment in Innovation</i> . By G. C. Allen	558
<i>The Challenge of Development</i> . By S. Adler	782
Child, F. C., <i>The Theory and Practice of Exchange Control in Germany</i> . By H. H. Liesner	141
Churchman, C. W., Ackoff, R. L. and Arnoff, E. L., <i>Introduction to Operations Research</i> . By M. R. Fisher	785
Coleman, D. C., <i>The British Paper Industry, 1495-1860</i> . By A. Beacham	149
<i>Commercial Policy 1957</i> . By Sir Roy Harrod	553
Cook, P. L., and Cohen, R., <i>Effects of Mergers</i> . By G. C. Allen	788
Corlett, W. J., <i>The Economic Development of Detergents</i> . By B. R. Williams	373
Davies, R. W., <i>The Development of the Soviet Budgetary System</i> . By M. H. Dobb	162
Dore, R. P., <i>Land Reform in Japan</i> . By W. J. Macpherson	792
Downie, J., <i>The Competitive Process</i> . By P. L. Cook	360
Duesenberry, J. S., <i>Business Cycles and Economic Growth</i> . By R. C. O. Matthews	749
Dunning, J. H., <i>American Investment in British Manufacturing Industry</i> . By R. W. Evelyn ...	147
Easterbrook, W. T., and Aitken, H. G. J., <i>Canadian Economic History</i> . By F. Thistlethwaite	160
<i>Economic Survey of Europe in 1957</i> . By A. Nove	165
Edey, H. C., <i>Business Budgets and Accounts</i> . By B. Tew	783
Ehrmann, H. W., <i>Organised Business in France</i> . By P. Bareau	555
Faxén, K. O., <i>Monetary and Fiscal Policy under Uncertainty</i> . By G. L. S. Shackle	130
Firestone, O. J., <i>Canada's Economic Development, 1867-1953</i> . By J. B. Heath	377
Friedman, M. (Ed.), <i>Studies in the Quantity Theory of Money</i> . By A. J. Brown	773
Gårdlund, T., <i>The Life of Knut Wicksell</i> . By B. Thomas	369
Gillman, J. M., <i>The Falling Rate of Profit</i> . By R. L. Meek	132
Goldsmith, R. W., <i>Financial Intermediaries in the American Economy since 1900</i> . By R. F. Henderson	155

REVIEWS (*continued*)

PAGE

Haas, E. H., <i>The Uniting of Europe</i> . By F. V. Meyer	139
Hagenbuch, W., <i>Social Economics</i> . By E. V. Morgan	569
Harrod, R. F., <i>Policy Against Inflation</i> . By Sir Ralph Hawtrey	356
Henderson, J. M., <i>The Efficiency of the Coal Industry</i> . By M. J. Farrell	151
Henderson, J. M., and Quandt, R. E., <i>Micro-economic Theory: A Mathematical Approach</i> . By F. H. Hahn	779
Higgins, B., <i>Indonesia's Economic Stabilization and Development</i> . By T. H. Silcock	578
Hirsch, W. Z., <i>Introduction to Modern Statistics</i> . By C. A. Blyth	152
Hornby, W., <i>Factories and Plant</i> . By D. Jay	566
Imlah, A. H., <i>Economic Elements in the Pax Britannica</i> . By P. M. Deane	562
Inman, P., <i>Labour in the Munitions Factories</i> . By S. R. Dennison	564
Kurihara, K. K., <i>The Keynesian Theory of Economic Development</i> . By J. Robinson	352
Leibenstein, H., <i>Economic Backwardness and Economic Growth</i> . By J. R. Hicks	344
Little, I. M. D., <i>A Critique of Welfare Economics</i> . Second Edition. By J. E. Meade	124
Mansergh, N., and others. <i>Commonwealth Perspectives</i> . By E. E. Rich	573
Marshall, J. D., <i>Furness and the Industrial Revolution</i> . By W. A. Cole	794
Martin, A., <i>Economics and Agriculture</i> . By E. H. Whetham	149
Mason, E. S., <i>Economic Concentration and the Monopoly Problem</i> . By G. C. Allen	156
Mathias, P., <i>History of the Brewing Industry in England, 1700-1830</i> . By Sir Hugh Beaver	796
Maxcy, G., and Silberston, A., <i>The Motor Industry</i> . By A. K. Cairncross	786
Meier, G. M., and Baldwin, R. E., <i>Economic Development</i> . By A. K. Sen	134
Mills, L. A., <i>Malaya: A Political and Economic Appraisal</i> . By B. H. Farmer	790
Mintz, I., <i>Trade Balances during Business Cycles: U.S. and Britain since 1880</i> . By R. C. O. Matthews	775
von Mises, L., <i>Theory and History</i> . By A. Briggs	770
Moser, C. A., <i>Survey Methods in Social Investigation</i> . By D. Cole	153
Musgrave, R. A., <i>The Theory of Public Finance</i> . By A. R. Prest	766
Musgrave, R. A., and Peacock, A. T. (Eds.), <i>Classics in The Theory of Public Finance</i> . By A. R. Prest	138
Myers, C. A., <i>Labor Problems in the Industrialization of India</i> . By D. T. Lakdawala	375
Myrdal, G., <i>Development and Under-development</i> . By P. T. Bauer	105
Myrdal, G., <i>Economic Theory and Under-developed Regions</i> . By P. T. Bauer	105
Myrdal, G., <i>An International Economy</i> . By P. T. Bauer	105
Myrdal, G., <i>Value in Social Theory</i> . By A. Briggs	365
Niculescu, B., <i>Colonial Planning: A Comparative Study</i> . By P. Ady	576
Nussbaum, A., <i>A History of the Dollar</i> . By Sir Roy Harrod	143
Oort, C. J., <i>Decreasing Costs as a Problem of Welfare Economics</i> . By E. J. Mishan	544
Parker, H. M. D., <i>Manpower</i> . By S. R. Dennison	564
Peacock, A. T., and Dosser, D. M., <i>The National Income of Tanganyika, 1952-54</i> . By P. M. Deane	575
Rangnekar, D. K., <i>Poverty and Capital Development in India</i> . By A. K. Sen	134
Robinson, E. A. G., <i>The Structure of Competitive Industry</i> . Revised Edition. By J. Downie	571
<i>Royal Commission on Canada's Economic Prospects: Final Report</i> . By J. B. Heath	377

REVIEWS (*continued*)

PAGE

Ryan, W. J. L., <i>Price Theory</i> . By J. K. Eastham	362
Shackle, G. L. S. (Ed.), <i>A New Prospect of Economics</i> . By R. G. Lipsey	549
Sigsworth, E. M., <i>Black Dyke Mills</i> . By P. L. Cook	571
Simon, H. A., <i>Models of Man</i> . By G. L. S. Shackle	547
Stapleton, G. B., <i>The Wealth of Nigeria</i> . By I. G. Stewart	164
Tawney, R. H., <i>Business and Politics under James I: Lionel Cranfield as Merchant and Minister</i> . By C. Wilson	380
Thomas, B. (Ed.), <i>Economics of International Migration</i> . By F. Thistlethwaite	158
Tinbergen, J., <i>The Design of Development</i> . By A. K. Sen	134
Tinbergen, J., <i>Economic Policy: Principles and Design</i> . By J. C. R. Dow	353
Titmuss, R. M., <i>Essays on the Welfare State</i> . By C. W. Guillebaud	364
<i>Trends in International Trade</i> . By Sir Roy Harrod	553
Tsuru, S., <i>Essays on Japanese Economy</i> . By W. J. Macpherson	792
Viner, J., <i>The Long and the Short</i> . By Sir Dennis Robertson	543
Wicksell, K., <i>Selected Papers on Economic Theory</i> . By B. Thomas	369
Wilson, J. S. G., <i>French Banking Structure and Credit Policy</i> . By P. Bareau	555
Youngson, A. J., <i>Possibilities of Economic Progress</i> . By W. W. Rostow	349

NOTES AND MEMORANDA

Allen, J. E., Railways or Roads?	180
Bauer, P. T., and Yamey, B. S., A Case Study of Response to Price in an Under-developed Economy	800
Farrell, M. J., Mr. Lancaster on Welfare and Choice	588
Frank, A. G., Industrial Capital Stocks and Energy Consumption	170
Galbraith, J. K., Mr. Hunter on Countervailing Power: A Comment	168
Green, H. A. J., Mr. Worswick on the Production Possibility Function	177
Hahn, F. H., The Theory of Selling Costs: A Correction	826
Healey, D. T., Note on the Relation between Share of Wages in National Income and Terms of Trade	823
Hirschleifer, J., Marginal Efficiency and Capital: A Comment	592
Hunter, A., Galbraith on Countervailing Power: A Reply	822
Jasay, A. E., Speculation, Arbitrage and Sterling: A Comment	590
Knapp, J., Capital Exports and Growth for Argentina: A Comment	593
Lancaster, K., Welfare and Expanded Choice—Proof of the General Case	805
Mishan, E. J., Mr. Lancaster's Welfare Definitions: A Comment	395
Morgan, D. J., Rejoinder on Jamaican Land Valuation and Land Taxation	391
Newman, P., Mr. Lancaster on Welfare and Choice	588
Nicholson, J. L., Import Duties and the Gross Domestic Product at Market Prices: A Reply	388
Oliver, F. R., Industrial Capital Stocks and Energy Consumption: A Comment	809
Patinkin, D., Keynesian Economics Rehabilitated: A Rejoinder to Professor Hicks	582
Robertson, Sir Dennis, Some Marshallian Concepts	382
Salera, V., A Note on Profit Sharing in Middle East Oil	812

NOTES AND MEMORANDA (*continued*)

PAGE

Sammons, R. L., A Note on the Treatment of Import Duties in the Measurement of Gross Domestic Product... ..	384
Silcock, T. H., Complementarity and Future Time: A Note on the Marginal Efficiency of Capital	816
Singer, H. W., Differential Population Growth as a Factor in International Economic Development	820
Solow, R., Is Factor Substitution a Crime, and if so, How Bad?: Reply to Professor Eisner	597
Soper, C. S., The Marginal Efficiency of Capital: A Further Note	174
Tobin, J., Reply to Professor Eisner	599
Wolfe, J. N., Some Marshallian Concepts: A Comment	807
Wright, J. F., The Marginal Efficiency of Capital	813

NOTES ON CURRENT TOPICS 183, 395, 599, 827

RECENT PERIODICALS AND NEW BOOKS 185, 401, 604, 830

REVIEWERS

Adler, S., 782	Jay, D., 566
Ady, P., 576	Lakdawala, D. T., 375
Allen, G. C., 156, 558, 788	Licsner, H. H., 141
Bareau, P., 555	Lipsey, R. G., 549
Bauer, P. T., 105	Macpherson, W. J., 792
Beacham, A., 149	Matthews, R. C. O., 749, 775
Beaver, Sir H., 796	Meade, J. E., 124
Blyth, C. A., 152	Meck, R. L., 132
Briggs, A., 365, 770	Meier, G. M., 347
Brown, A. J., 773	Meyer, F. V., 139
Cairncross, A. K., 786	Mishan, E. J., 544
Cole, D., 153	Morgan, E. V., 551, 569
Cole, W. A., 794	Nove, A., 165
Cook, P. L., 360, 571	Peacock, A. T., 772
Deane, P. M., 562, 575	Prest, A. R., 138, 358, 766
Dennison, S. R., 564	Rich, E. E., 573
Dobb, M. H., 162	Robertson, Sir D., 543
Dow, J. C. R., 353	Robinson, J., 352
Downie, J., 144, 571	Rostow, W. W., 349
Eastham, J. K., 362	Roy, A. D., 367
Evely, R. W., 147	Sen, A. K., 134
Farmer, B. H., 790	Shackle, G. L. S., 130, 547
Farrell, M. J., 151	Silcock, T. H., 578
Fisher, M. R., 785	Stewart, I. G., 164
Guillebaud, C. W., 354	Tew, B., 783
Hahn, F. H., 777, 779	Thistlethwaite, F., 158, 160
Harrod, Sir R., 143, 553	Thomas, B., 369
Hawtrey, Sir R., 356	Whetham, E. H., 149
Heath, J. B., 377	Williams, B. R., 373, 560
Henderson, R. F., 155	Wilson, C., 380
Hicks, J. R., 344	



THE ECONOMIC JOURNAL

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SPECULATION, ARBITRAGE AND STERLING¹

It has come to be taken as axiomatic that a speculative attack on sterling must cause a heavy loss of gold and dollars. But the principal argument of this article is that rational speculation (in the sense of speculation at least possible cost) is innocent of any adverse effect on the reserves, and that, apart from irrational speculation, interest arbitrage must be held responsible for the drain of gold and dollars commonly attributed to speculation. To be sure, it may well be, and in the 1957 crisis it certainly was, the case that speculation, by pushing forward sterling to a heavy discount, is ultimately responsible for the interest arbitrage margin in favour of foreign centres which gives rise to the adverse arbitrating operations and the consequent loss of reserves. Nevertheless, it is important to insist that the loss of reserves should be attributed to arbitrage and not speculation—not on semantic or taxonomic grounds, but for the intensely practical reason that adverse arbitrage is far more tractable than adverse speculation.

The thesis advanced here is implicit, *inter alia*, in Dr. Einzig's standard work on forward exchanges,² and in Keynes' plan for managed forward rates,³ but has been so overlooked in recent years that, as it is hoped to show, wrong conclusions have been drawn on both the theoretical and policy levels.⁴

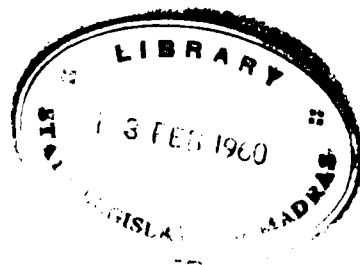
Besides the main thesis and its implications for theory and policy, this article contains: (a) an analysis of the three principal types of interest arbitrage; (b) a criticism, along welfare economics lines, of the policy of allowing the emergence of a discount on forward sterling; and (c) an appraisal of the practicability of the above-mentioned Keynes plan under present-day conditions.

¹ This article has greatly benefited from long correspondence with Mr. Harrod, who is not, however, to be held responsible for, or assumed to share, the views advanced here.

² P. Einzig, *The Theory of Forward Exchange* (London: Macmillan, 1937).

³ J. M. Keynes, *A Treatise on Money* (London: Macmillan, 1930), Vol. II, pp. 319-27.

⁴ Since this was first written at the beginning of 1958 there has been some public discussion of the problem. See A. E. Jasay, "Bank Rate or Forward Exchange Policy," *Banca Nazionale del lavoro*, March 1958, pp. 3-17; J. Spraos, A. E. Jasay and editorial writer, "Exchange Policy in the Forward Market," *The Banker*, April 1958, pp. 225-35; A. E. Jasay, "Forward Exchange: the Case for Intervention," *Lloyds Bank Review*, October 1958, pp. 35-45.



THREE TYPES OF INTEREST ARBITRAGE

To begin with, a description of the various types of interest arbitrage needs to be given. This is essential for a thorough understanding of the argument and has to be undertaken because all but one type are unfamiliar.

There is, first, *pure arbitrage*. This is the familiar type and is said to be "pure" because it is not associated either with trading or speculative activities. It involves the movement of short-term funds between financial centres in search of the highest return. The repatriation of such funds must be covered against the exchange risk, otherwise there would be an element of speculation which is excluded by definition. Therefore the relative attractiveness of two centres depends, on the one hand, on the short-term interest differential between them and, on the other, on the margin between the spot exchange rate (at which funds must be converted for transfer) and the current forward rate (at which they must be covered).

The *neutrality condition*, under which there is no incentive for interest arbitrage in either direction, is invariably stated¹ in the form

$$\text{Discount rate in London} - \text{Same in New York} = \frac{\text{Price of sterling spot} - \text{Same forward}}{(\text{expressed as a percentage per annum of spot price})},$$

and there is said to be an arbitrage margin in favour of London, attracting funds to the City, if the left-hand side is greater; and in favour of New York if it is smaller.

Actually this is a biased formulation. But its correct equivalent, being a little more complicated, is perhaps better left to a footnote,² for it is not

¹ See, for example, J. M. Keynes, *A Tract on Monetary Reform* (London: Macmillan, 1923), p. 124, and P. Einzig, *op. cit.*, pp. 165-7.

² If K is the volume of funds measured in dollars; R the short-term interest rate in New York expressed as a percentage per (say) three months and r the same in London, E the pound-dollar exchange rate in the spot market (the dollar being used as the *numéraire*) and e the same three months forward, there is no incentive to move funds in either direction when

$$K(1+R) = K \frac{1}{E} (1+r)e \quad \text{or} \quad \frac{R-r}{1+r} = \frac{e-E}{E}$$

If the left-hand side is algebraically greater there is an interest arbitrage margin in favour of New York and funds are under an incentive to move in that direction; conversely, if the right-hand side is algebraically greater the arbitrage margin is in favour of London. Observe that only if $r=0$ does the above condition reduce to the traditional proposition that there is no arbitrage incentive either way when the interest-rate differential and the percentage forward (or "swap") margin are equal. Moreover, the bias in the traditional formulation is in one direction if we are working with interest rates, and in the opposite direction if we are working with discount rates. Denoting by D the discount rate per three months in New York and by d the same in London, we have the following identities:

$$R = \frac{D}{1-D} \quad \text{and} \quad r = \frac{d}{1-d}$$

Whence, by substitution into the previous equality, we get

$$\frac{D-d}{1-D} = \frac{e-E}{E}$$

The traditional formulation would now be strictly true if $D=0$, and the reversing of the bias in that formation can be seen from the change in the sign in the denominator of the left-hand side.

essential to the logic of the argument at the intuitive level, nor need the biased formulation be misleading as to the facts if proper allowance is made for the bias. In fact, it will be seen that in examining the sterling crisis of 1957 the biased formula was used for calculating the daily arbitrage margin between London and New York. To avoid misunderstanding this is made plain in each case by referring to the arbitrage margin so calculated as "crude." In the period between July 1, 1957, and February 28, 1958, the crude arbitrage margin between London and New York was biased on occasions by as much as $\frac{1}{2}\%$ per annum. This, whilst not large, is not negligible when pure arbitrage is said to be set in motion by a margin of as little as $\frac{1}{2}\%$ per annum.

Another bias arises from the habit (of writers, not operators) of calculating the percentage forward margin from the mid-point of dealers' buying and selling quotations. The mid-point represents better terms than can be attained by either buyer or seller (though dealers themselves, when acting for their banks, try to beat the quoted range of other dealers). It should be understood that the crude arbitrage margin in Table I includes the bias from this source also. The magnitude of this bias is of the order of $\frac{1}{2}\%$ per annum, but is offset in part by the other bias.

Under present exchange control regulations residents of the Sterling Area are precluded from exporting capital for interest arbitrage.¹ The banks, however, constitute a partial exception to this. They are allowed to hold a limited volume of foreign balances as cover for forward commitments undertaken in the course of business with clients, and this gives them room for manoeuvre. Thus if, say, a British importer wants to buy forward dollars, his bank will oblige by selling them to him. But it is not part of a bank's business to carry the burden of the exchange risk attached. It can relieve itself of this burden either by matching its forward dollar sales to clients with its forward dollar purchases from clients, or, in so far as it is left with a net balance of forward dollar sales, by purchasing spot dollars to hold as cover. It is entirely under the bank's control whether it will be left with a net forward commitment at the end of the day or not. For it can always vary its forward quotation in such a way as to balance supply and demand. If it chooses to accumulate a net forward balance (to be held under spot cover) it must be because it pays it to do so, since banks are not philanthropic institutions. And whether it pays it or not is governed by the arbitrage margin.

Thus, in so far as the banks "marry" their forward purchases and sales of foreign currencies, they act like any other broker, bringing buyers and

¹ Non-resident sterling balances can be withdrawn freely, subject to restrictions on dollar convertibility if the balances in question are not held by residents of the "American Account Area." Central Banks of Sterling Area member countries are entitled to full convertibility for their sterling balances. But they are not known to engage (nor would it be in the spirit of the Area's conventions if they did) in either arbitrage or speculation adverse to sterling.

sellers together and playing no independent part. In so far as they let their net forward commitments accumulate, they act, in effect, as pure arbitrageurs. Their arbitrage could perhaps be called *passive*, in that it emerges in the course of the banks' work for their clients. But it is deliberate none the less, since its emergence could be checked by adjustment of the forward quotation to the point at which forward demand and supply of clients are equated.¹

The second type of interest arbitrage may be called *trader-arbitrage*. The name is designed to indicate that such arbitrage is associated with the financing of international trade. The emphasis on the trading aspect is also intended to exclude any element of speculation—exchange risks always covered. If trade is financed in the country from which the goods originate, the importer or his bank carry a commitment to pay a sum in the exporting country's currency x days after the shipment of goods, and this has to be covered by a forward purchase of that currency. If, on the other hand, the financing takes place in the country of destination, the remittance across the exchanges takes place immediately by a spot purchase, and there is no exchange risk for the importer to cover.² There is thus the choice between: (i) paying the rate of interest ruling in the exporting country and buying its currency forward, and (ii) paying the rate of interest ruling in the importing country and buying the foreign exchange spot. In other words, which centre is the least costly for trade financing depends on exactly the same factors as those determining which centre yields the highest return in the pure arbitrage case. But since we are now dealing with the borrowing of funds, the signs are, so to speak, reversed. An arbitrage margin in favour of London attracts pure arbitrageurs but repels trader-arbitrageurs, and vice-versa.

The neutrality condition (under which there would be no incentive for trader-arbitrage in either direction) is exactly the same as in the pure arbitrage case. But two practical differences may be noted. First, whereas for pure arbitrage the most relevant interest rates are probably

¹ Much confusion over the role of the banks in this field has arisen from the traditional dealer practice of covering immediately by a spot transaction each forward commitment entered into in the course of business. But this is generally only a stop-gap measure necessitated by the fact that the forward market is rather imperfect (particularly for odd maturities) and would therefore be overstrained if dealers attempted to "marry" immediately each forward sale by a purchase, or vice versa. The ultimate aim is to arrange such "marriages," and when this is achieved the stop-gap spot cover is reversed. If persistent failure to "marry" forward commitments is encountered it means that supply and demand are not in balance. In such an event the forward quotation would be adjusted, except in so far as it pays the banks to accumulate net forward commitments to be held under spot cover.

² Financing in a third country is another possibility, which, however, complicates greatly the exposition without adding anything of substance. In any case, since British exchange control prohibits at present the granting of credits for trade in which neither party is a Sterling Area resident, London, which was pre-war the principal centre for third-country financing, is out of this business, and the remaining facilities in the world for such financing are very limited.

those on Treasury Bills,¹ for trader-arbitrage it would seem that the interest rates on commercial paper are the more appropriate. Secondly, the minimum margin required to induce trader arbitrage is greater than in the pure arbitrage case, because, naturally, financial institutions are the most responsive in this respect, and their arbitraging operations are mostly of the pure type.² Trader-arbitrage would not be expected to occur for as little as $\frac{1}{2}\%$ per annum, which can sometimes suffice for pure arbitrage. At $1\frac{1}{2}$ –2%, however, traders in primary products might well begin to stir, as such a percentage would make a substantial difference to the profitability of their activities, in view of the surprisingly low gross profit margins on which they operate.³

Trader-arbitrage derives its importance from the existence of exchange controls. These controls limit the scope for pure arbitrage to such an extent that other forms of arbitrage which can be conducted within the rules acquire a special significance. Since 1953, when the present writer laid emphasis on trader-arbitrage,⁴ exchange controls (as distinct from import controls) have not significantly diminished.

The third type of arbitrage may be referred to as *speculator-arbitrage*. As the name implies, it is associated with speculation; with uncovered exchange risks. Nevertheless, the activity to be described is arbitrage in the fullest sense of the term, for it involves taking advantage of a strictly calculable and entirely non-speculative margin—the margin between costs of alternative means to the same speculative end. Two sub-species may be distinguished. The first is tied up with trade in that the opportunity to engage in one of the speculative alternatives arises from the operator's capacity as a trader; the second is tied up with investment for an analogous reason. As an illustration of the first, take an American importer of British goods. He can speculate against the pound by either: (a) getting his trade financed in London, thus contriving to owe sterling, or (b) financing his trade in New York and simultaneously opening a speculative position by a sale of forward sterling. Since under alternative (a) he would be paying the appropriate London interest rate, whilst under (b) he would be paying the corresponding New York interest rate, and the remittance to the British

¹ This is only valid as between countries which have organised markets in Treasury Bills, and even then the Treasury Bill is far from being the only arbitrage medium. Occasionally market imperfections give rise to arbitrage profit in one direction between Treasury Bills and in the opposite direction between (say) bank deposit accounts.

² To be sure, there is still some scope for banks to engage on their own account, and unknown to the traders themselves, in what is here called trader-arbitrage—they do so whenever they set out to answer the question: to refinance abroad or not to refinance? (Refinancing means, in effect, the shifting of credit from the bank which has nominally granted it to a trader on to the shoulders of some other bank.) In that event, the inducement margin for trader-arbitrage need be no greater than for pure arbitrage. But exchange regulations do not at present allow refinancing in London, and this cuts out the principal source of refinance credits in the world.

³ See J. Spraos, "The Theory of Forward Exchange and Recent Practice," *Manchester School*, May 1953, pp. 87–117, especially pp. 109–10.

⁴ *Ibid.*, pp. 92–6.

exporter would be made by a spot purchase of sterling, the same four factors come into play: the interest rates in the two centres and the spot and forward exchange rates. If we are to compare like with like, the size of the open position should, of course, be identical under both alternatives, and in that case the point for cost indifference between them is given by the same neutrality condition as for the other types of arbitrage.

As an illustration of the second sub-species, take an American financial institution with funds available for short-term employment and anxious to back an up-valuation of the Swiss franc. It can either: (a) buy spot francs and employ its funds in Zürich, or (b) employ its funds in New York and purchase forward francs. Clearly, once again the relative merits of the two alternatives depend on the same four factors, and the position of indifference between the alternatives is indicated by the same neutrality condition as before.

The difference between a speculator-arbitrageur and, what we might call, a *pure speculator* is that the latter has neither spare liquid funds to invest nor trade to finance, and therefore has no choice in his method of speculation: he must necessarily buy or sell forward. A speculator-arbitrageur need not, of course, limit his open exchange position to the point which his resources or trading activities permit him to reach by either of the methods described. But in so far as he exceeds this point he acts as a pure speculator.¹

Finally, the minimum margin required to channel speculator-arbitrage in one direction rather than another should be no greater than for pure arbitrage—i.e., $\frac{1}{2}\%$ per annum—when the speculator-arbitrage is associated with investment; and when associated with trade it should be no greater than for trader-arbitrage. There can, of course, be no speculator-arbitrage at all, no matter how big the margin, unless people wish to engage in currency speculation. But from the point of view of sterling, which nowadays is frequently the object of speculation, speculator-arbitrage is very important, and much may depend on understanding it fully and turning it to good account.

EFFECT ON RESERVES

The full story of the effects of interest arbitrage on a country's gold and dollar reserves, under all possible contingencies, is as interesting as it is complicated, and has yet to be written.² But here the objective is more

¹ Pure speculation, be it noted, is not open to a Sterling Area resident because (except by special approval) he is allowed to buy or sell currency forward only for the purpose of covering a trade contract in foreign currency. For the same reason speculator-arbitrage is not open to him, since one of the alternatives under each sub-species involves entering the forward market, but not for cover.

² For recent discussions of parts of the field see M. N. Trued, "Interest Arbitrage, Exchange Rates and Dollar Reserves," *Journal of Political Economy*, October 1957, pp. 403-11; the present writer's letters to *The Economist* (February 8, 1958, p. 479 and March 15, 1958, p. 931) and that *Journal's* Business Leader of March 1, 1958 (pp. 775-6); also the literature quoted in footnote 4, on p. 1.

limited: to show in the light of the previous section that, in principle, gold and dollars are lost not through speculation but through arbitrage. For this purpose the various ways in which a bear attack against sterling is said to denude the reserves will be considered, and the logic of each examined in turn.

Take first the case which can be disposed of most easily: the repatriation of foreign-owned funds from London. If these were covered by a forward deal in the first place they were carrying no exchange risk. They are therefore withdrawn either for some extraneous reason, which is irrelevant, or because the interest they can now earn in London *minus* the cost of forward cover is less than they could earn in their country of origin. If the latter, it is a case of pure arbitrage, not speculation. If, on the other hand, the funds in question were not covered to begin with, their owners can always turn to the forward market when they find the exchange risk too heavy to carry any longer. If they choose instead to withdraw them it must be that they can get better value by so doing—again a case of pure arbitrage.

Secondly, take the case of foreign-owned funds which in the sterling crisis of 1957 were said to be under double incentive to move from London to West Germany: the expectation of sterling devaluation or D-mark up-valuation. If, say, an American who keeps funds in London wants to back an up-valuation of the mark, he can do so by buying forward marks; he need not transfer his funds to West Germany. If he rationally chooses the latter it must be because he can thereby attain his speculative end on better terms—a case of speculator-arbitrage.¹

Thirdly, turn to the adverse "leads" and "lags" in commercial payments. Are these the result of speculation pure and simple or of speculator-arbitrage? Take the case of an American importer of British goods. He has been financing his trade in New York,² and could therefore discharge his sterling obligation immediately on shipment of the goods. Now he suspects that sterling might be devalued and—so the argument goes—wishing to be in a position to benefit from such an eventuality he delays ("lags") his sterling remittance. But this means that he now switches to London for the financing of his trade, and why should he do that unless it is cheaper than the alternative of leaving the pattern of his trade payments undisturbed and speculating against sterling by selling it forward? Or take the Sterling Area importer who, not wishing to be caught with a foreign-currency obligation in the event of sterling devaluation, is said to accelerate ("lead") his remittance. This, too, implies a switch of finance from abroad to London, and what, if not a relative cost advantage, is the

¹ It has been suggested to the writer that German exchange controls prevent the short-term employment of non-resident marks. This is equivalent to a zero short-term interest rate.

² If he employs his own capital for such financing it makes no difference, since this would have an opportunity cost determined by New York interest rates.

rational inducement for this in preference to covering forward the foreign-currency commitment? In short, assuming rational behaviour, it is not speculation, as such, that causes the damaging "leads" and "lags" in payments, but the particular form it is induced to take by arbitrage considerations.¹

All this is doubtless obvious once stated. Its importance derives from the fact that the authorities, by intervening to keep the arbitrage incentive in the right direction, can induce all bear speculation against the pound to take the form of forward sales of sterling, thereby rendering it harmless to the gold and dollar reserves.

If the forward market is not subject to official intervention a bear attack will cause an immediate loss of dollars irrespective of whether it consists of spot or forward sales of sterling. If it consists of spot sales the loss is direct. If it consists of forward sales the loss is indirect but no less immediate. For the forward sale must be absorbed, and this will be accomplished through a fall in the forward rate sufficient to induce some arbitrage-motivated purchase of forward sterling. This would transmit the impact to the spot market. If, for example, pure arbitrage were involved, the arbitrageur would combine his forward purchase of sterling with a spot sale of it. Thus, the forward sterling sale of the speculator would be converted to a dollar-absorbing spot sale per medium of the arbitrage margin which the initial action of the bear speculator opened up against London.²

At a time of acute scepticism over the future of the sterling parity the arbitrage margin against London is persistent. The volume of arbitraging operations which is forthcoming is not sufficient to close it, because once the forward sterling discount narrows to a certain point there is a rush of

¹ In a footnote on p. 115 of the present writer's "The Theory of Forward Exchange and Recent Practice" (*op. cit.*), the following passage was quoted from the United Nations Report on *Measures for International Economic Stability* (November 1951): "A rumour of currency devaluation that advanced exchange transactions relating to imports by an average of one month, and delayed transactions relating to exports and other receipts by one month, would lead to a drain on the reserves equivalent to two months imports." The distinguished authors of the Report were then criticised for failing to appreciate that a devaluation rumour need not have any acceleratable effect on a country's import payments. At the same time it was granted that it would inevitably have a delaying effect on export receipts. But this was a mistake, and should not have been granted, for this, too, depends on arbitrage considerations.

² Cf. A. I. Bloomfield, *Capital Imports and the American Balance of Payments 1934-39* (Chicago: The University of Chicago Press, 1950), pp. 20-1. There is another argument which purports also to reach the conclusion that in a market free from official intervention a forward sale of sterling would deplete the reserves just as much as a spot one. It rests on the banks' practice of obtaining immediate spot cover for each forward commitment undertaken towards clients. But this argument is either fallacious or merges into the one outlined in the text. It is fallacious in so far as it overlooks that the spot cover is generally a stop-gap measure, to be reversed when the "marriage" which dealers actively seek between their forward purchases and sales of sterling is achieved. It merges into the argument of the text in so far as the banks find it profitable, thanks to the arbitrage margin, to accumulate net forward commitments under spot cover, thus acting in effect as pure arbitrageurs. (The background to this is discussed in the section entitled "Types of Interest Arbitrage.")

pure speculators offering forward sterling. (They cannot offer spot sterling, because, by definition, they do not possess any.) In other words, in those circumstances the supply of forward sterling becomes highly elastic, because the potential cost of unsuccessful speculation becomes low relative to the potential rewards of success. Thus, on account of pure speculation, an arbitrage margin against London can persist for a long time, acting as a standing invitation to dollar-absorbing arbitrage.

If, however, the authorities intervened in support of the forward rate the adverse arbitrage margin could be turned into a favourable one and the drain on the dollar reserve eliminated. The bear sales would be deflected by the favourable arbitrage margin to the forward market, where they would be absorbed by the support purchases of the authorities until such time as they came to be reversed by the bears themselves. For it is to be expected that they would be reversed. In the case of the pure speculator (who, by virtue of exchange controls, cannot be a Sterling Area resident) there is in fact no choice. He does not possess the sterling which he sold forward, so he must inevitably cancel his open position by a purchase of sterling on or before the maturity of his initial deal. He can, of course, renew his open position, but sooner or later he must close it. The foreign speculator, on the other hand, who has sold forward sterling for dollars and at the same time owns sterling balances can, if he wishes, ask for the dollars on the maturity of his deal (or, for that matter, earlier). But why should he thus transfer his capital if the arbitrage margin continues to be in favour of London? There are, of course, balances being withdrawn all the time because they are needed for other purposes, but so long as London retains an arbitrage advantage, other foreign funds, becoming simultaneously free, will take their place. (The question of reversibility is considered again in the section entitled "Arguments and Counter-Arguments.")

Thus the authorities' purchases in support of the forward rate would not involve them in any loss of dollars. Such purchases would amount only to a book-keeping commitment to pay dollars x days ahead—a commitment destined to be cancelled before it matures.

The conclusion then is that a speculative attack is not a sufficient condition for a loss of dollars. Such a loss will occur only if the speculative attack is allowed to depress the forward rate and thus create an adverse arbitrage margin which will shift the burden of the attack on the spot market. In fact, it can be fairly said that arbitrage, not speculation, is the source of drain on the reserves. For should the authorities intervene and support forward sterling at a point which retains an interest arbitrage margin in favour of London, speculation, whether pure or impure, will remain but (in so far as it is rational) the loss of reserves will cease.

London-New York Interest Differential, Forwa

Date	1	2	3	4	5	6	7	8	9	10	11	12	13
July 1957:													
Yield differential between U.K. and U.S. treasury bills . . .	0.75	0.67	0.67		0.70			0.87	0.87	0.87	0.87	0.81	
Forward dollar premium as percentage per annum . . .	1.30	1.30	1.30	1.30	1.30			1.30	1.30	1.30	1.21	1.21	
Crude arbitrage margin . . .	0.55	0.63	0.63		0.60			0.43	0.43	0.43	0.34	0.40	
August 1957:													
Yield differential between U.K. and U.S. treasury bills . . .	0.53	0.60				0.58	0.60	0.59	0.63				
Forward dollar premium as percentage per annum . . .	1.57	1.57				1.57	1.80	1.84	1.71			0.56	0.4
Crude arbitrage margin . . .	1.04	0.97				0.99	1.20	1.25	1.08			2.07	2.2
September 1957:												1.51	1.7
Yield differential between U.K. and U.S. treasury bills . . .			0.57	0.59	0.60	0.74				0.76	0.72	0.70	0.7
Forward dollar premium as percentage per annum . . .		3.82	4.40	4.22	3.87	4.04				4.04	3.95	3.86	4.2
Crude arbitrage margin . . .			3.83	3.63	3.24	3.30				3.28	3.23	3.16	3.5
October 1957:													
Yield differential between U.K. and U.S. treasury bills . . .	3.11	3.11	3.12	3.16			3.16	3.10	3.10	3.10	3.10		
Forward dollar premium as percentage per annum . . .	4.47	4.52	4.65	4.65			4.29	3.93	3.30	3.39	3.48		
Crude arbitrage margin . . .	1.36	1.41	1.43	1.39			1.13	0.83	0.20	0.29	0.38		
November 1957:													
Yield differential between U.K. and U.S. treasury bills . . .	3.06					3.13	3.16	3.19				3.20	3.2
Forward dollar premium as percentage per annum . . .	3.39					3.48	3.66	3.44	3.17			3.26	3.2
Crude arbitrage margin . . .	0.33					0.42	0.53	0.28	0.02			0.06	0.0
December 1957:													
Yield differential between U.K. and U.S. treasury bills . . .		3.44	3.45	3.46	3.46	3.46				3.55	3.55	3.52	3.4
Forward dollar premium as percentage per annum . . .		3.25	3.17	3.07	3.12	3.07				3.07	2.94	2.90	2.9
Crude arbitrage margin . . .		-0.19	-0.28	-0.39	-0.34	-0.39				-0.48	-0.61	-0.65	-0.5
January 1958:													
Yield differential between U.K. and U.S. treasury bills . . .		3.62	3.59			3.59	3.54	3.60	3.63	3.76			3.7
Forward dollar premium as percentage per annum . . .	3.12	2.94	3.02			3.02	3.07	3.07	2.98	2.94			3.0
Crude arbitrage margin . . .		-0.68	-0.57			-0.57	-0.47	-0.53	-0.65	-0.82			-0.7
February 1958:													
Yield differential between U.K. and U.S. treasury bills . . .			4.66	4.56	4.53	4.52	4.44				4.45	4.39	4.3
Forward dollar premium as percentage per annum . . .			3.46	3.55	3.55	3.60	3.68				3.68	3.64	3.6
Crude arbitrage margin . . .			-1.20	-1.01	-0.98	-0.92	-0.76				-0.77	-0.75	-0.7

Notes:

YIELD DIFFERENTIAL BETWEEN U.K. AND U.S. TREASURY BILLS. Based on market yields for Treasury Bills of the respective country with maturities as near to three months as possible. U.S. Treasury Bills are issued only on Thursdays, hence on other days of the week there are no maturities of exactly three months. The "ask" New York yield, as quoted by the *Wall Street Journal*, has been used throughout.

FORWARD DOLLAR PREMIUM AS PERCENTAGE PER ANNUM. The mid-point between dealers' buying and selling rates for the three months forward dollar quoted in London at the close of daily business has been taken and, after being converted to a per annum basis it has been expressed as a percentage of the spot rate. Source: *Samuel Montagu's Review of Foreign Exchanges*.

IMPLICATIONS FOR THEORY AND POLICY

On the plane of theory the above is principally relevant to the debate on pegged (but adjustable) versus floating exchange rates. The main argument against pegged exchanges advanced by the advocates of floating rates is that by providing a "one-way option" for speculators, that is by giving them the opportunity to gain from devaluation without the risk of

Exchange Premium, and Arbitrage Margin

14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
		0.87	0.87	0.87	0.92	0.97		0.87	0.70	0.68	0.68	0.66			0.61	0.52	0.52
		1.21	1.21	1.21	1.21	1.21		1.30	1.39	1.39	1.48	1.57			1.66	1.57	1.57
		0.34	0.34	0.34	0.29	0.24		0.43	0.69	0.71	0.80	0.91			1.05	1.05	1.05
0.46	0.56	0.73			0.78	0.78	0.83	0.85	0.73			0.72	0.59	0.58	0.58	0.65	
2.52	2.87	3.59			4.04	5.39	5.03	5.25	5.12			3.32	2.34	2.16	3.41	3.77	
2.06	2.31	2.86			3.26	4.61	4.20	4.40	4.39			2.60	1.75	1.58	2.83	3.12	
			0.71	0.69	0.69		3.11		3.20	3.16	3.16	3.16	3.20			3.21	
			4.31	5.21	5.75	5.93	4.67		4.67	4.02	4.21	4.66	5.20			4.29	
			3.60	4.52	5.06		1.56		1.47	1.86	1.05	1.50	2.00			1.08	
3.06	2.99	2.98	3.00	3.05			3.07	3.04	3.04	3.07	3.05			3.05	3.05	3.05	3.06
3.53	3.61	3.53	3.79	3.84			3.61	3.57	3.57	3.61	3.48			3.44	3.39	3.22	3.17
0.47	0.62	0.55	0.79	0.79			0.54	0.53	0.53	0.54	0.43			0.39	0.34	0.17	0.11
3.21	3.51			3.55	3.55	3.55	3.54	3.41			3.40	3.38		3.38	3.39		
3.26	3.44			3.35	3.20	3.35	3.44	3.26			3.17	3.17	3.38	3.30	3.30		
0.05	-0.07			-0.20	-0.35	-0.20	-0.10	-0.15			-0.23	-0.21		-0.08	-0.09		
			3.44	3.42	3.42	3.38	3.38		3.38	3.42			3.37			3.77	3.64
			2.81	2.81	2.90	2.90	2.90		2.90	2.90			2.90			2.94	3.12
			-0.63	-0.61	-0.62	-0.48	-0.48		-0.48	-0.52			-0.47			-0.83	-0.52
3.81	3.79	3.84	3.80			3.82	3.88	3.96	4.13	4.13			4.18	4.22	4.33	4.56	4.68
3.15	3.15	3.15	3.24			3.33	3.24	3.24	3.24	3.24			3.24	3.33	3.20	3.29	3.46
-0.66	-0.64	-0.69	-0.56			-0.49	-0.64	-0.72	-0.89	-0.89			-0.94	-0.89	-1.13	-1.27	-1.22
4.37			4.40	4.38	4.38	4.51	4.65		4.90	4.81	4.83	4.83					
3.73			3.73	3.60	3.46	3.55	3.55		3.60	3.64	3.56	3.74					
-0.64			-0.67	-0.78	-0.92	-0.96	-1.10		-1.30	-1.17	-1.27	-1.09					

CRUDE ARBITRAGE MARGIN. Equals the difference between two preceding rows. A positive figure indicates that the arbitrage margin is in favour of New York and a negative one that it is in favour of London. Differs from the true arbitrage margin (for exchange see above under "Three Types of Interest Arbitrage") and usually exaggerates it by at least 1%.

Blank cells are accounted for by Saturdays, Sundays and American and English holidays. In addition, the relevant U.S. data for our dates could not be traced in London, whilst for September 19—the date of the Bank Rate rise from 5 to 7%—yields of U.K. Treasury bills were somewhat nominal.

loss from up-valuation, it encourages large speculative attacks on a suspected currency, which impose an intolerable strain on the reserves.¹ But whilst

¹ Of the large number of possible references in this field suffice to mention Professor J. E. Meade ("The Case for Variable Exchange Rates," *Three Banks Review*, September 1955, pp. 3-27, especially p. 13), who is the best known advocate of floating exchanges in the United Kingdom, and Mr. W. M. Scammel (*International Monetary Policy* (London: Macmillan, 1957), pp. 98-9), who, after giving an exhaustive list of pros and cons, declares the above argument to be the most telling.

our analysis does not affect the "one-way option" argument it does invalidate the conclusion that a speculative attack encouraged by such an option must necessarily cause a loss of reserves. In principle, it will do no such thing if the arbitrage incentive is held at a position favourable to the suspected currency.

On the plane of policy the principal conclusion to be drawn from the above analysis is that the system instituted in December 1951, and operated ever since, whereby forward sterling is allowed to float whilst the spot rate is pegged within narrow limits, suffers from a serious flaw. A speculative attack pushes forward sterling to a heavy discount, and this provides the incentive for adverse interest arbitrage in the absence of which speculation against sterling would have left the reserves unscathed (except in so far as it is irrational, about which more below). Bank Rate has then to be called upon to compensate for the wide forward discount—the two largest jumps in Bank Rate since the reactivation of monetary management in 1951 (March 1952, $1\frac{1}{2}\%$ rise; September 1957, 2% rise) were preceded by a sharp widening of the forward sterling discount—irrespective of the internal health of the economy.

It is instructive to survey the picture of recent months portrayed in Table I. The table begins three weeks or so before sterling first came under suspicion. By the end of July the crude arbitrage margin had widened sufficiently to induce some run-down of the London balances of American and other foreign banks. For the alternative course of keeping them in London covered against the exchange risk would have left them worse-off by more than $\frac{1}{2}\%$ per annum, which is the figure at which banks are said to become sensitive. In the next fortnight the position deteriorated rapidly, and, in the week beginning August 19, London's relative disadvantage had grown to 4% per annum. At that point, in addition to pure arbitrageurs, every conceivable bear, who had the choice, was offered a strong inducement to act in a way detrimental to the reserves. The American importer of Sterling Area goods, for example, was doubtless motivated by speculation when he lagged his payment, but if he was a rational man the reason for his choosing this method instead of a forward sale of sterling (the adverse effect of which could be neutralised) was that the latter was now so much more expensive. With a difference of 4% per annum, only those who did not own or could not borrow sterling would have been expected, in principle, to speculate by selling it forward—and that because they had no alternative.

In the following week, the last in August, the crude arbitrage margin narrowed considerably, thanks, doubtless, to official intervention.¹ On a couple of days (27th and 28th) it narrowed sufficiently to eliminate for all but the most alert arbitrageurs the inducement to act in a way harmful to the reserves. Thereafter, however, whilst official intervention continued,

¹ See *The Economist*, August 31, 1957, p. 700.

was less ambitious. Could it be because the authorities were (unnecessarily) alarmed at the rate at which their forward commitments were owing when the forward sterling discount was kept low? In any event, in the first fortnight of September the true arbitrage margin between London and New York stood against the former in the region of 3% per annum, which was high enough to encourage a heavy pressure on the reserves. And the position between London and West Germany was far worse, the forward sterling discount against marks being more than double the discount against dollars. It is the contention of this article that herein lies the principal explanation for the gold and dollar losses of August and September 1957 (see Table II).

TABLE II
Changes in the Gold and Dollar Reserves

	Actual change in reserves, \$ million.	True dollar balance in period,* \$ million.
1957:		
July	— 14	— 32
August	—225	—300
September	—292	—296
October	+243	+141
November	+ 92	+ 71
December	+ 88	+ 86
1958:		
January	+131	+166
February	+135	+207

* Adjusting for one month lag in settlement of E.P.U. balance, for known special payments (not regular or quarterly ones) and credits from I.M.F., Export-Import Bank, etc.; no adjustment is made for Germany's deposit of £75 million in the summer months. Source: *The Economist*, February 8, 1958, p. 508, and March 8, 1958, p. 867.

And so to September 19, when Bank Rate was raised from 5 to 7%. So far as the arbitrage margin is concerned there is no difference between increasing the interest differential and narrowing the forward sterling discount, so the arbitrage margin shrank, though it remained somewhat against London for another fortnight or so. Then, about October 8, 1957, the arbitrage margin moved into the neutral belt, where there is no substantial arbitrage incentive in either direction, and remained there (with the possible exception of one or two days) until January 22, 1958. Nevertheless, after the severe pruning undergone by the working balances of foreign banks, when holding them covered in London became so expensive, there would be some replenishment of them even under a neutral arbitrage margin, and the reserves would stand to gain from it. Finally, the fall in the rate of discount on U.S. Treasury Bills, which had started in mid-November 1957, gained momentum in January 1958, and by the end of that month a clear arbitrage margin in favour of London had emerged and was maintained

over the early part of February. Naturally the reserves could not fail to benefit from it. The wheel had now turned full circle, and the authorities acknowledged this fact on March 20, 1958, when they reduced Bank Rate from 7 to 6% and followed this up by further reductions later.

The combined story told by Tables I and II is doubtless capable of more than one interpretation. All that can be said for the proposition that the arbitrage margin lies at the back of a very substantial part of the recorded changes in the gold and dollar reserves is that it is consistent with that story. But if the logic is consistent with the facts there is something to be said for sticking to the logic. The moral as far as policy is concerned is that it was a serious mistake to let forward sterling go to a heavy discount in the two months of crisis, August and September 1957.

ARGUMENTS AND COUNTER-ARGUMENTS

The first argument against supporting the forward rate is that it increases the scale of attack against sterling by reducing the cost of failure to the speculator, and in the limit, when the forward rate is held at the spot parity, by eliminating such cost altogether.¹ For in that case the speculator is able to reverse at any time his forward commitment on the same terms as those under which he made the commitment in the first place. But so what? As previously indicated, speculative forward deals are mere book-keeping entries destined to be rubbed off. This is manifestly so for the pure speculator who cannot deliver the sterling he has sold forward because he does not have any. It is equally so for the chap who does have the sterling but has no incentive to deliver it because, when the arbitrage margin is in favour of London, he can do better for himself by reversing his forward commitment, instead of letting it run its course and foregoing, in consequence, the superior return which he can obtain for his funds in London.

There is thus no limit to the resistance potential of the authorities to a speculative attack of this kind, except the size of the books in which they enter their forward transactions. There is admittedly the danger that they might lose their nerve if, as a result of their support of the forward rate, they see their forward dollar commitments mounting beyond the total reserves.² But such a loss of nerve would be groundless. For the only people who can call upon the authorities to honour their forward dollar commitments are those who either possess sterling which is convertible (*de jure* or *de facto*) or who can borrow it; people, in other words, to whom the authorities have a standing commitment anyway. All the other forward dollar commitments of the authorities would be to operators who have sold sterling "short," and who must therefore inevitably enter into a reversing

¹ See, for example, *The Economist*, August 31, 1957, p. 700.
² Cf. P. Einzig, *op. cit.*, p. 392.

transaction on or before the maturity of their forward deals, thereby cancelling the authorities' dollar commitment.

A second argument against official support of the forward rate is that though it benefits the published reserves, it is tantamount to "cooking" them,¹ inasmuch as the benefit in question is gained by pushing the seamy side out of sight into the forward account. Dr. Einzig tells us that when he was writing his classic book it had become "an accepted rule that if the foreign currencies held by the Central Bank are sold forward they cannot be included in their metallic reserves."² He later showed this for the nonsense it was,³ and, fortunately, post-war practice in the United Kingdom has taken no notice of this "rule." Is it not, in fact, the failure to divert a speculative attack out of sight into the forward account that makes for "cooking" of the true position? Take an American importer of British goods who delays payment, instead of having his speculative fling by means of a forward sterling sale. He makes a hole in the reserves; but does he alter for the worse the Sterling Area's real balance of total assets and liabilities?

Another argument, sometimes met in conjunction with the previous one, is that the diversion of speculators to the forward market does not eliminate the loss of reserves, it merely postpones it. According to this argument, the authorities, instead of paying gold now, undertake through their support operations to pay gold in (say) three months' time, and when the undertaking matures they are no better off. But this is not so. As already indicated, operators who had sold sterling "short" are in the nature of things compelled to reverse their initial deals. Whilst those who possessed nonconvertible sterling but chose to cover it by a forward transaction instead of converting it via the spot market, would also be expected to reverse their initial deals if they are rational. For just as in the first place the authorities' support of the forward rate made spot conversion less desirable than forward cover, so their continuing support would make conversion more costly than reversal of the initial deal combined (if desired) with renewal of forward cover.

A fourth argument would grant that forward commitments arising from the activities of speculators would cause the authorities no loss of reserves if they succeeded in maintaining the exchange parity, but would point to the cost of those commitments in the event of the speculative attack forcing a devaluation. The answer to that is that if a speculative attack is diverted to the forward account it need never force a devaluation, since it would cause no loss of reserves. It may well be that a devaluation is deemed

¹ The word is Mr. Trued's (*op. cit.*, p. 409, n.), though, to do him justice, the inverted commas are also his.

² P. Einzig, *op. cit.*, p. 126. A League of Nations pronouncement on the Austrian Bank is said to have created the precedent.

³ *Ibid.*, p. 346.

expedient for other reasons, but in that case its promulgation need not be associated with heavy forward commitments in the authorities' books.

A fifth argument is that a forward exchange support policy could not deal with capital fleeing from suspected confiscation or restriction on its freedom of movement, because a forward deal is subject to the risk of retrospective annulment, and is therefore no substitute, however attractive its terms, for a change of domicile. This would be a valid argument if the flight from sterling was due to such suspicions and not just to the risk of devaluation.

Lastly, an argument which may be anticipated is that the case advanced here depends crucially on the prevalence of rationality. But what if the American bank with funds in London decides to protect them against devaluation by withdrawing them, even though it stands to lose less by covering them forward? Or what if the American importer of British goods takes it into his head to lag his remittance, even though he could speculate at less cost to himself by paying as promptly as before and selling forward sterling instead? And so on.

In so far as there exists full knowledge of the alternatives, such perverse actions are clearly not to be expected from banks whose business it is to make money from small turns, or from traders who, by virtue of their occupation, have a more highly developed sense of profit and loss than the rest of the community. In so far as there is some ignorance over such matters, the authorities should not find it too difficult to spread enlightenment. Instead of ineffective affirmations that sterling will be held at its current parity and that it is not, therefore, worth anybody's while to speculate against it, they can let it be known through all the unobtrusive but highly effective channels at their command that it is to the bears' own advantage to operate in the forward market. The educational effort needed may well be small. Keynes, referring to the situation in 1922, remarked that, unlike professional speculators, amateurs confined themselves to the spot market.¹ But Dr. Einzig assures us that by the middle thirties the "general public" had "also acquired the habit of operating in Forward Exchange" whenever there was "a substantial or lasting speculative movement."²

For what it is worth, history provides no confirmation of the "irrationality" hypothesis. Since the unpegging of forward sterling in December 1951 the three periods of severe strain on the reserves can be explained by the widening of the forward discount. The disastrous effect of the speculative attack of 1949, and of that part of the 1951 attack which preceded the freeing of the forward rate, can also be attributed to their being diverted from the forward market. This time not through the medium of the forward discount, but on account of exchange controls which kept the forward

¹ J. M. Keynes, *A Tract on Monetary Reform*, *op. cit.*, p. 132.

² P. Einzig, *op. cit.*, p. 145.

market closed to all speculative activities. The 1947 convertibility crisis was of a different breed, not relevant for present purposes. And as for the "hot money" movements of the thirties, Dr. Bloomfield's admirable account of the mechanism by which they affected the reserves is fully consistent with the thesis of this article.¹

THE FORWARD MARGIN AS A SOURCE OF INEFFICIENT ALLOCATION

So far the proposition which, it is hoped, has been established is that forward sterling should be supported on balance-of-payments grounds, the object of such support being to discourage adverse arbitrage operations. This object does not necessarily call for support at the spot parity. In this section, however, it will be argued that departure of the forward rate from spot parity (in either direction) involves a misallocation of resources. This is true (for different reasons) from the point of view of both the single country and the international economy as a whole. But to limit the discussion, the matter will be considered from the point of view of the former only.²

Take the case of a discount on forward sterling. The London bank which finances the export of, say, a British car charges a rate of interest of $x\%$. But the foreign importer of that car is, by virtue of this London credit, able to discharge his obligation by a purchase of forward sterling maturing on the expiry of the credit. Thanks to the forward sterling discount, the effective cost to him of the London finance is less than $x\%$. This is not reflected in the return obtained by the bank, but in less favourable terms of trade than would otherwise obtain. In other words, there is in this instance, in Professor Pigou's useful though somewhat *démodé* terminology, a divergence between marginal private net product and marginal social net product.³

And if one makes the assumption of a perfect market in credit for international trade (which in this kind of abstract argument one might have to make—see footnote 2 on this page) there is not even the consolation that the above arrangement operates as a hidden subsidy for Sterling Area exports and that it may, in consequence, benefit the Area's balance of trade. Because, under the perfect market assumption, the cost of trade financing would be equalised throughout the world and Sterling Area exports would not enjoy any exclusive advantage.⁴

¹ See A. I. Bloomfield, *op. cit.*, especially pp. 18–24.

² It is now well understood that partial welfare economics is strictly valid only if all other prices in the system conform with the rules for allocative efficiency, which is manifestly untrue, or if the sector under examination approximates to a closed circuit, which is rare. With this in mind, let the reader make his own reservations regarding the argument of the text.

³ See A. C. Pigou, *The Economics of Welfare* (London: Macmillan, 4th ed. 1932), Part II, Chapter IX.

⁴ Incidentally, the perfect market assumption is not necessarily incompatible with exchange controls prohibiting the financing of trade between third countries. But it is incompatible with a persistent arbitrage margin in favour (or against) any financial centre.

THE KEYNES PLAN

Keynes saw in the margin between spot and forward exchange rates an instrument for insulating domestic monetary management from short-term international capital movements: short-term interest rates were to be changed in response to domestic requirements, whilst an offsetting change in the forward margin was to be engineered by the authorities to neutralise the interest arbitrage margin that would otherwise emerge.¹ The scheme was conceived as symmetrical. That is to say, it would work just as well when domestic short-term interest rates were lower than those ruling abroad, only in that event the forward exchange rate would have to be supported at a premium over the spot rate. The symmetry of the scheme, however, depended on a symmetry of uncertainty: taking a longish period, expectations were not supposed to be predominantly one way, whether in the direction of depreciation or appreciation. But symmetry of uncertainty is lacking to-day, and for this reason the full Keynes plan is not practicable under present conditions.

Keynes emphasised the importance of uncertainty for the scheme on the ground that, if the spot rate could be relied upon to enable repatriation of funds without loss, arbitrageurs would not have to obtain forward cover for their operations, and thus the forward margin would not enter their calculations.² Oddly enough, it is not in this connection that the one-way uncertainty under which sterling has had to labour most of the time since the War gets the plan into difficulties. For suppose that spot sterling stands at its upper limit against the dollar—\$2.82 to the pound—and that short-term interest rates in New York stand above those in London. American capital would still have no incentive to withdraw from London if there is a forward sterling premium which yields a gain on the “swap”—the spot purchase of sterling and its simultaneous sale forward—that compensates for the lower interest in London. But British capital would be under an incentive to move to New York. For the interest obtainable there is higher and there is, at the same time, no need to incur a loss on the “swap,” since, by the assumption of one-way uncertainty, appreciation of sterling is ruled out, and there is therefore no need for forward cover against the exchange risk. But such “uncovered” interest arbitrage by British capital is not permitted by exchange control,³ and in this respect, therefore, the one-way uncertainty attached to sterling does not affect the applicability of the Keynes plan.

¹ See J. M. Keynes, *A Treatise on Money*, *op. cit.*, Vol. II, p. 325–7.

² *Ibid.*, p. 324.

³ This accounts for the fact that the case of uncovered interest arbitrage was not considered when in the first section of this article the various types of interest arbitrage were analysed. There are, of course, no controls prohibiting uncovered arbitrage by American capital in the opposite direction, when sterling touches the lower limit of \$2.78 to the pound. But since the War a hovering of sterling in that neighbourhood has always been accompanied by intense suspicion of devaluation, which effectively ruled out uncovered arbitrage.

What does affect it is the cost of supporting the forward rate at a premium under one-way uncertainty. For suppose again that the spot rate stands at \$2.82 to the pound and that everybody is confident that it will not appreciate above that point. And suppose further that three months sterling is maintained by the authorities at, say, two cents premium, that is at \$2.84 to the pound. Under those conditions everybody (except Sterling Area residents, who would not be permitted to do it by exchange controls) would sell forward unlimited quantities of sterling and buy it back on maturity, making in the process at least two cents on every pound at the expense of the authorities. Of course, the uncertainty is not likely to be as one way as all that, but if it is as predominantly one way as it has been in post-war years the authorities would suffer heavy losses, one suspects, if they were to support forward sterling at a premium.

Different people would draw different conclusions from this. Some would doubtless say that the logic of the Keynes plan leads us to floating exchange rates (either entirely without official limits or with such limits very widely apart). For we have now stumbled on the element of truth in the one-way-option-for-speculators argument against pegged exchanges, which we criticised earlier.¹ For pegged exchanges tend to be associated with one-way uncertainty. And if the home currency is under such uncertainty and at the same time short-term interest rates are lower at home than abroad, the cost of deflecting, by means of the arbitrage margin, a speculative attack to the forward account, where its adverse effect on the reserves can be neutralised, is prohibitive.

Others would probably conclude that so long as the uncertainty attached to sterling relates nearly always to whether it will go down, and only rarely to whether it will go up, short-term interest rates in London should be held above those of the principal financial centres abroad. The Keynes plan could then be operated without difficulty.

Still others would want to retain the stabilising influence of a pegged spot exchange rate, but at the same time would not want to surrender the freedom of lowering short-term interest rates below those ruling abroad. They would therefore look for alternatives to high interest rates. There is in fact a very handy alternative: the stamp duty on drafts under documentary credits. This is levied at the rate of one shilling per £100 on most instruments by which financing of international (and some internal) trade is undertaken. It originated, presumably, as a revenue duty, but it can doubtless be used to discourage adverse “leads” and “lags” in commercial payments, which cannot occur without the extension of London credit facilities. (It would, of course, have to be varied according to circumstances.) To be sure, this would still leave foreign-owned London balances with an incentive to engage in either pure- or speculator-arbitrage adverse

¹ See above, opening paragraph of section headed “Implications for Theory and Policy.”

to sterling when short-term interest rates become higher abroad.¹ But without the aid of "leads" and "lags" in commercial payments the withdrawal of sterling balances held outside the Sterling Area cannot break the bank. These balances are not a match for the reserves in either volume or composition.²

Of the three alternative conclusions which can be drawn from the asymmetry of the Keynes plan under present conditions of one-way uncertainty, the writer prefers the last. But notice that because of the one-way uncertainty there is an effective upper limit to the forward exchange rate—it cannot exceed the upper limit of the spot rate. There is, however, no lower forward limit. It seems to the writer that, on balance, it would be advantageous if there was one—close to the lower spot limit—to which the authorities were publicly committed. For the following reasons. First, because it would set a limit to the possible divergence between social and private net product (see previous section). If allocative efficiency clashes with the defence of the reserves it should be sacrificed. But in this instance there is no clash. Secondly, a fixed floor under the forward rate would reduce the burden of discretion carried by officials. This burden can become so heavy at times that it makes the expedient look preferable to the right. In discussing the time series of Table I it was observed that the authorities intervened boldly towards the end of August 1957 in support of forward sterling, but failed to keep this up for more than a week. Did they lose their nerve? If so, this would be a case in point.

Thirdly, the arrangement would introduce the right type of automaticity. Suppose, for example, that initially the spot exchange stands at \$2.80 to the pound and the three months forward rate at \$2.78, which we shall assume to be the floor for both spot and forward sterling. Now a speculative attack begins, and there is pressure on the spot rate because, let us assume, under the two cent forward discount there is not sufficient incentive to deflect all the attack on to the forward market. The spot rate therefore drops, but not the forward, which stood at its floor from the beginning. In consequence, the forward discount narrows, and will continue narrowing until all the speculative attack is diverted to the forward account and its harmful effect on the reserves eliminated. It may well be, of course, that, given the relationship of short-term interest rates between financial centres, this position could not be reached unless forward sterling appreciated to a premium, which it cannot do. In that event there will have to be recourse to increases of stamp duty on instruments of international trade financing and/or other devices to supplement the narrowing of the forward discount.

¹ Compare, however, the First World War expedient (to which Dr. Pressnell has drawn the writer's attention) of discriminatory interest rates for foreign-owned funds. (See E. V. Morgan, *Studies in British Financial Policy, 1914-25* (London: Macmillan, 1952), p. 334.)

² See *Economic Trends*, May 1958, pp. vii-xii.

SUMMARY AND CONCLUSION

The foundation of the whole paper is a careful analysis of interest arbitrage. From this analysis it follows without difficulty that the withdrawal of foreign-owned funds from London and the adverse "leads" and "lags" in commercial payments, which denude the reserves, are due to arbitrage and not, as commonly attributed, to speculation. More specifically, a speculative attack on sterling is directed against the reserves if the arbitrage margin is against London; it can be deflected into self-cancelling forward deals if the arbitrage margin is held at a point favouring London. If the forward exchange rate is not officially supported, a speculative attack will push it to a heavy discount, thereby turning the arbitrage margin against London, and thus exposing the reserves to all the speculative fury. But if official support prevents a widening of the forward discount, and thereby succeeds in maintaining an arbitrage margin in favour of London, speculation may rage, but there is no reason why it should touch the reserves. This conclusion is inescapable, and if it appears startling it is because of the conditioning we have all undergone since the War.

The objections to official support for the forward rate, which have been discussed and which to the present writer appear unconvincing anyway, can hardly be said to be very weighty compared with the balance-of-payments advantage to be gained by determined support. Besides, we noted that a forward exchange discount is inconsistent with allocative efficiency.

The Keynes plan for "managed" forward exchanges was not designed to meet speculative attacks. But the logic behind it is analogous. For this reason the last section of the paper was devoted to an examination of the plan. The chief point emerging from that examination was that under present conditions the plan would be asymmetrical—workable in one direction but not in the other. The different lessons that different people could choose to learn from this fact were outlined. This led to the final argument, which went farther than the rest of the paper in advocating that the forward rate should not only be supported, as a defence against speculative attack, but should be actually pegged.

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PRICE AND VALUE IN INDUSTRIAL MARKETS

"The difference is rather in tone and emphasis than in analysis. My generation, brought up on the view that everything in the garden was lovely, concentrated attention on the weeds. A generation to whom our weed manuals were the orthodox text-books react by pointing out that there are after all many splendid blooms to be seen."

Joan Robinson

I. INTRODUCTION

IN this paper we intend to canvass from a new perspective some of the basic themes of "value theory" with reference chiefly to industrial markets. The immediate stimulus for our presentation is the twofold conflict ranging in our literature between marginalist and full-cost theories of pricing and the Marshallian theory of value and the modern "theory of the firm." We believe that this conflict can be satisfactorily resolved along lines which have been variously indicated in recent writings.

We are concerned about this conflict only in the case of pricing by industrial enterprise of products sold at "set" prices to informed buyers in competitive industrial markets with few restrictions on entry but with oligopolistic and "sticky" operational characteristics. We are thus dealing essentially with the case of "workable competition" (to use Clark's term) or competition which is "free" (to use Scitovsky's).¹ We are ignoring the case of isolated sellers, of oligopolistic markets with restricted access; and we exclude the case of industrial markets dealing with uninformed retail buyers. It is this latter case with its crucial categories of product differentiation and sales outlay on which Chamberlin concentrated in his famous treatise. The underlying presupposition for these categories was the "uninformed" or "inexpert" buyer.² We will not contend that this case is a minor one, since it applies in varying degrees to all consumer goods and services. The case applies only in "varying degrees," since the degree of inexpertness varies widely—and with it susceptibility to quality differentiation and sales persuasion—and distributive channels play a variable role. Galbraith has recently helped to make us aware of the important role played in industrial markets by the big retail distributor.³ On the other hand, we observe the rise of untraditional and complex consumer goods products, the

general decline in consumer technical skills and knowledge, the general uprootedness of modern urban life and the rise of mass informational media.¹ The relationship between avocational consumers with their plastic wants, retail distributors, manufacturer-sellers and informational media is complex and generates significant feedbacks on quality design, pricing and scale of operations. As Sherrard has indicated, this case raises issues and involves complications which require special consideration.² Hence we omit consideration of this case in this paper.

This restriction in the scope of our analysis means that we can neglect the role of quality and sales outlays in the demand equation and thus revert to the "classical" type of demand equation: with income, price and quantity variables only. This certainly does not mean that in the desiderated industrial markets sales persuasion or marketing effort will not play a role. They will. But the marketing effort will be needed primarily to make the price and product offer effective, *i.e.*, to bring information about the offer to potential buyers and to develop the necessary personal relations contact with them. And product variation or "quality" changes will involve innovations undertaken to improve serviceability or to meet emerging or neglected user-needs. In our desiderated industrial markets a given basic need will be met not by a single but by a family of differentiated products innovated to respond to different user interests or capabilities. As Heflebower has emphasised, this differentiation is not arbitrary but is service-oriented to meet the "niches" of a product market.³ In these circumstances product change may be considered not as a species of "differentiation" of a static "variable," but as an "innovation" which is independent of the demand equation, since it sets the frame in which this equation takes on meaning.

We commence our presentation with an argument that it is analytically valid to aggregate a family of related products into a standardised "commodity" produced by an "industrial market group" for which supply and demand schedules may be developed. This right of aggregation is now in question, particularly where supply schedules are concerned. At this point we intend to innovate and to supplement the conventional cost-derived supply schedule with a true offer-schedule, for which A. W. Marget contended so strongly in his neglected *Theory of Prices*, and which has in disguised form been resurrected among us as the revenue curve in "break-even analysis."

The second stage of our analysis (Part III of the paper) will be concerned with analysis under static conditions of the relationships between the offer,

¹ See Scitovsky, *Welfare and Competition* (1951), Chapters II and XVII.

² See Scitovsky, pp. 16 ff.; Stigler, *Five Lectures on Economic Problems*, p. 19.

³ Galbraith perhaps only generalised an idea which had been earlier formulated by C. Wilcox and R. Heflebower. For a competent treatment and references, see Heflebower, "Mass Distribution: A Phase of Bilateral Oligopoly or of Competition," *American Economic Review Proceedings*, May 1957, pp. 274-85.

¹ For emphasis see Scitovsky's illuminating discussion, Chapter XVIII.

² See A. Sherrard, "Advertising, Product Variation and the Limits of Economics," *Journal of Political Economy*, April 1951.

³ See R. B. Heflebower, "Toward a Theory of Industrial Markets and Prices," *American Economic Review Proceedings*, May 1954, p. 124 ff.

the supply and the demand schedules. We shall define an equilibrium level of value in terms of "market clearing" developed in the work of Hicks, Klein and Samuelson. We shall consider the tendency for the offer-schedule—and hence the realised market price—to become located at that equilibrium level or within a definite range of values above it. We shall carry away certain insights and conclusions from static analysis. Nevertheless, we shall point out the weakness of statically assuming that price is adjusted to prevailing and experienced conditions. In industrial markets price-determination is inherently dynamic. Prices are never designed afresh, but are in principle always carried over from a pre-existing period for either the same or the most comparable product. Hence the problem is never one of *determining* a new price but of *altering* an old one—or of changing the level of the old offer-schedule. As soon as the problem is stated in these terms—and it is Scitovsky to whom we owe the formulation—we become aware of the deep bias for price stability or for changing price in a consistent time-trend. It is for this reason that an offer-schedule will be fixed—or a price-offer set—at a level designed to accommodate the market needs not of the current but of a projected future and its anticipated conditions of demand and supply.

This analysis leads to Part IV of our paper, which is concerned with the means and workable short-cuts evolved out of business experience to achieve the necessary accommodation to future market conditions in an uncertain world. It is at this level that we move beyond the bounds of short-run cost and demand projections and turn to levels of Marshallian normal value with their standard costs and "normal" profits of a "representative" firm. We shall see that the "representative firm" is only a way of conceiving of a potential competitor when entry is open or feasible. We shall then attempt to show that most of the thinking behind full-cost pricing formulas in fact represents an effort by business-men to adjust price trends to Marshallian levels of "normal value" which itself will involve a forecasted sustainable pathway or level of equilibrium values emergent under anticipated trend values of supply and demand.

Obviously my subject cannot exhaustively be developed with rigorous proof. That would require the dimensions of a monograph, and presuppose capacities and interests which I do not have. We are rather attempting to sketch the outlines of the argument without any effort to make the presentation conclusive. Nor could we hope to pursue through references the necessary debate with other writers to whom I am indebted or to whom reference is intended.¹ We will cite only basic works which are particularly needed in exposition or for emphasis.

¹ But I am obviously indebted to many: J. Robinson, A. W. Marget, R. Harrod, P. W. S. Andrews, M. Reder, R. B. Heflebower, Scitovsky and others too numerous to mention. Unfortunately, I became acquainted with P. Wiles, *Price, Cost and Output* (1956) after the paper was completed so that I was not able to draw upon and profit from this effort to develop a comprehensive theoretical frame for industrial economics.

II. THE "INDUSTRY," ITS STANDARDISED "COMMODITY" AND SUPPLY AND OFFER-SCHEDULES

Throughout our analysis we shall deal with an "industry" which allegedly produces and markets a "commodity" under conditions which may be summarised in demand, supply and offer-schedules. Since this procedure flies in the face of current theoretical practice, it should be justified in rather self-conscious terms. We commence with the easiest case, that of the "industry," and then move on to the case of the standardised "commodity" and finally to the validity of an aggregated supply and offer-schedule. The issues involved at all three levels are interrelated; hence the analysis must be judged as a whole and not in its separate presentations.

The case for the "industry" is the easiest to make. We have all learned much since the first theorists of the new ("monopolistic competition") dispensation taught us to eschew competition and adopt monopoly or an isolated seller as our model for analysis of price behaviour in imperfectly competitive industrial markets. The conventionalised version of a market group which played a role in the works of Robinson and Chamberlin was found untenable by Triffin, who was manifestly dedicated to the abolition of the concept of the "industry" and its standardised "commodity."¹ Dialectically this expression of the ultimate logic of the argument helped to rejuvenate the attacked concepts and to provide them with the theoretical basis needed. Through the work of Triffin we now see that the old "industry" was a collective term applicable to two groups with overlapping memberships. It first denotes a group of producers with a common technology capable of producing with easy substitution a set of product types representing an "area within which a firm finds it relatively easy to expand as it grows."² Then there is a group of sellers of competitive commodities with positive or significant cross-elasticities suitably defined—a seller's market group. Though not generally emerged in the text-books, these two modern embodiments of the old industry concept are destined for sturdy life.³ They have been midwived by theorists of high reputation; they meet an obvious need for a level of aggregation in between that of the economy as a whole and the ultimate economising units and individual products; and they have emerged out of a gruelling process of theoretical debate and clarification.⁴ They correspond to very real entities in the real world. We shall employ both concepts. We need the market group, since

¹ Triffin's work had an impassioned character and a consistency which is even now admirable. See R. Triffin, *Monopolistic Competition and General Equilibrium Theory*, pp. 7, 9, 78–96.

² J. Robinson, "Imperfect Competition Revisited," *Economic Journal*, September 1953, p. 582.

³ Let me name an exception of an outstanding text-book treatment from which I have drawn. A. G. Papandreou and J. T. Wheeler, *Competition and Its Regulation* (1954), Chapters 2–4.

⁴ Besides Robinson and Papandreou and Wheeler let me cite M. Friedman, "The Marshallian Demand Curve," *Journal of Political Economy*, December 1949, with his emphasis on the desirability of including among the frozen "other things" of the demand equation the "prices of closely related commodities," pp. 465, 469 f.

it is in the market that value is determined. We also need the group of industrial co-associates, since we want to trace out lines of reflex action of market price on industrial growth. Hence we will deal with what Papandreou and Wheeler have denominated the "sub-group" of maker-sellers, "who belong to the same market group and at the same time also to the same industry."¹ Clearly, non-suppliers who use the same industrial technology represent potential competitors of a particularly dangerous kind. We need to be aware of their existence, but we need not at this stage bring them into the formal analysis.

The concept of the "commodity" is related to that of the "industry." The market and industrial sub-group produces a proliferated line of products distinguished by make-up, performance characteristics and a complex set of associated services of supply and availability. How can this family of specific product varieties be represented by a standard "commodity"?

We are faced here with the general principle of abstraction from meaningful differences for the purpose of forming artificial collective groups considered analytically as a homogeneous and operational entity. We cannot here endeavour to give an adequate statement of this general principle or of the trail of qualifications which must be worked into a complete statement of it. We may form an artificial group and abstract from intra-group variations in connection with behaviour, with regard to which these variations are relatively independent and which have insignificant or separable effects which may be analysed at a later stage with "additive" results. If the process of behaviour will induce alterations in these variations and be reciprocally affected by these alterations, we cannot abstract from these variations if we are to understand the behavioural process or even adequately to describe it.

Accepting these principles as controlling, may we form one commodity category for all products of a given sub-group of competing maker-sellers of the same industrial origin? More concretely: is it significant to consider all automobile manufacturers as maker-sellers of passenger vehicles; may we aggregate all wheat varieties into one commodity "wheat" and all unfabricated steel products into unprocessed steel? Interestingly enough, this assumption is regularly made in empirical studies dealing with product varieties in highly competitive agricultural markets where commodity differentiation over a wide range is normally encountered (grains, fibres, livestock, fruit, etc.). Even so standard a bread-grain as wheat comes in seven classes, each of which falls into five grades; and grading is sufficiently imperfect—considering the wide variety of foreign materials involved in any batch—that a high percentage of wheat is bought by direct sample evaluation.² Moreover, these commodity "variations" play an important role

¹ Papandreou, etc., *op. cit.*, p. 61.

² See the survey of commodity classification and pricing in W. C. Waite and H. Trelogan, *Agricultural Market Prices* (1948), Chapter 12, "Prices by Classes, Varieties and Grades." On grain marketing see L. J. Norton and L. L. Scranton, *Marketing of Farm Products* (1937), pp. 271 f.

since they correspond to major supply areas and are adapted to distinct user needs. Yet it would be ridiculous to argue that for these reasons we should give up attempts to analyse the demand and supply for all these varieties considered as an aggregate. Moreover, it is important to note that farm economists and econometricians have gone on to aggregate on a much wider basis taking more inclusive groupings of producers and products. They have even developed significant findings for the whole mass of food considered as a single commodity of a sub-group of a larger industry, a basic category of "classical" economics—"agriculture."¹ The only constraint on aggregation is that of "matching" the "commodity" with the industry. The "commodity" should include all varieties of product to which plant facilities and organisation of the "industry" may with moderate adjustments be shifted. An industry would then be defined as an embodied technology and developed organisation specialised in the production of a range of mutually substitutable products. A firm may combine two distinct technologies with their distinct "commodity" groupings and thus belong to two "industries"; yet both "commodities" may belong to the same "market group" of maker-sellers of mutually substitutable items though not to the same "sub-group." The basic units of our analysis are thus an "industry" and "commodity" (defined essentially in terms of technology), a "market group" of maker-sellers (defined in terms of "cross-elasticities"), and our ultimate atom of analysis, a "sub-group" made up of members of the same industry who belong in the same market group.

In deriving a "commodity" out of component product varieties, we may generally suppose, as Hicks and Samuelson have argued, that relative prices of component varieties will be unaffected by changes in commodity prices. This is only generally true since as Heflebower has indicated changes in commodity price levels sometime become accomplished wholly and will tend to become accomplished in part through changes in the pattern of product prices. This pattern will tend to change when commodity demand levels have become altered through a change in relative product preferences growing out of varying product income elasticities. Then too the dropping or adding of "low-end" items at seller initiative is a specific technique for modifying price levels in imperfect industrial markets.²

¹ See T. Schultz, *Economic Organization of Agriculture*; G. Tintner, *Econometrics*, pp. 11, 45, 132.

² We recall that Samuelson has declared that, "Among the many recent advances contained in Professor J. R. Hicks' *Value and Capital* perhaps the most important from an analytical point of view was the enunciation of the principle that a group of commodities has the property of a single commodity if their prices all change in the same proportion." *Foundations of Economic Analysis*, p. 130 (and for detailed proof see p. 141-6); see Hicks, *Value and Capital*, p. 33 and Appendix. In the first draft of this paper I had accepted this position. Professor Heflebower was kind enough to call my attention to the unsoundness of this position as is indicated in his essay, "An Economic Appraisal of Price Measures," *Journal of the American Statistical Association*, Vol. 46 (1951), pp. 461-79.

Granted the case for analytic aggregation on the industry and commodity level, we still have before us a greater hurdle to surmount: that of the consolidated supply schedule. There has been little or no objection to utilisation of a collective demand schedule for a standardised commodity class. But the resistance to its counterpart—that of the supply schedule—is widespread. As we all know, this resistance stems partly from Marshall's caution with regard to application of statical modes of analysis to industrial markets, dominated as they are by long-run tendencies of increasing returns. Marshall was quite clear as to the validity and suitability of a short-run supply schedule which he explicitly argued would be governed by variable costs for marginal increments of output and with rising values of supply price, as capacity is more fully utilised, and sharply rising values when capacity limits are strained.¹ But Marshall, with his characteristic genius, was so afraid of formalised supply and demand analysis in a static frame that he never even drew a hypothetical short-term supply schedule and warned against the validity of such a schedule applied to long-term supply movements.²

This resistance to formalisation of supply-schedule analysis was carried forward and completed in the work of Chamberlin and Robinson. Aside from the competitive case with its assumed homogeneous product, both authors eschew any formalisation or use of a supply schedule for a market group. Robinson wrote up a critique of an aggregated supply schedule which showed that with successively changing demand schedules the pathway of equilibrium quantities that would be supplied would depend in part upon demand elasticities which could not be expected to behave as a constant. But her critique was construed as demonstrating the invalidity of a supply schedule which was confused with a supply-pathway.³ Following Robinson theoretical analysis has eschewed the use of supply schedules on the group level for non-competitive markets. It is almost taken for granted that use of such a supply schedule would be inadmissible.⁴

The formal argument for the admissibility of the supply schedule is im-

¹ Marshall, *Principles of Economics* (8th ed.), pp. 373–6, 498, 501.

² Though Marshall discouraged wisely, not all of his specific dicta will bear close analysis, as when he flatly argued that "statical theory" was not "applicable" to our products (p. 501); or again as when he averred that supply price becomes "unstable" with increasing returns; and, of course, the whole drift of the always wise but sometimes cryptic utterances of his famous Appendix H.

³ Joan Robinson, *The Economics of Imperfect Competition*, Chapter 6, "The Supply Curve." There is indication in the chapter that Robinson herself approximated the supply-pathway to a true supply schedule.

⁴ Thus the manuals of advanced-price theory almost ritually derive and utilise a supply schedule for pure competition and avoid its use in imperfectly competitive settings. See the following statements: "Applied to a[n] [competitive] industry . . . the supply concept is clear-cut and therefore valuable. Unfortunately, it is somewhat confusing and therefore less useful when applied to the product of an industry which is not perfectly competitive, and hence it is rarely used for such industries." Tarshis, *Elements of Economics*, pp. 218 f. "We cannot construct a 'supply curve' for an industry in imperfect competition as we can for one in perfect competition, for there is no single price at which all the product is sold . . ." (Boulding, *Economic Analysis* (3rd ed.),

plicit in the argument for the principle of aggregation as noted by Reder.¹ It is no more invalid to aggregate supply than demand. In either case we couple with a list of alternative prices under given conditions the largest quantity that would be demanded or supplied and maximise profits. As Marshall himself emphasised, the short-run supply schedule is concerned only with prime costs and marginal quantities. It is derived from a schedule of total costs of the firm at varying rates of capacity utilisation. We simply aggregate individual firm supply schedules into a total group schedule. Since the lowest value of prices for which a positive supply will be forthcoming will be the lowest average variable cost, the schedule of Q_s will always rise as the value of price rises—this Marshall noted—or in a limiting case be constant over a certain quantity range.² The principal modification growing out of aggregation involves allowance for changed factor pricing induced by varying rates of industry capacity utilisation. The cost schedule of the individual firm is generally fixed assuming given factor supply-prices which are predicated upon given rates of industry activity. But these factor supply-prices will themselves be affected by rates of industry activity, as Keynes argued, and at bottleneck points industry supply factor prices will be affected wherever the factor markets permit fluid pricing practices.³

The supply schedule can be constructed only by invoking the profitability principle which reduces to the famous theorem of the equality between marginal revenue and marginal cost. What this theorem actually means when applied to imperfectly competitive behaviour of industrial markets is that the maker-seller will be willing to supply at alternative prices up to the point of equality between marginal revenue and cost. There must be included in marginal costs compensation for increased entrepreneurial effort where this is differentially involved; and likewise marginal revenue must include the present discounted value of anticipated future gains or possible damages arising out of any specific increment of business. Interpreted in this wise—and I believe we have not made our proposition tautological—the profitability principle finds its proper application in price theory right at this point. It is relevant in determining the limiting quantities which at alternative prices a seller will offer for sale.

¹ M. Reder, "Rehabilitation of Partial Equilibrium Theory," *American Economic Review Supplement* (1952), pp. 182–7. In Kalecki's valuable paper "The Supply Curve of an Industry under Imperfect Competition," *Review of Economic Studies*, February 1940, pp. 93 ff., his demonstration included the theorem of the equality of marginal cost and marginal revenue (see his equation 3 or 3a); hence changes in market imperfection result in changed values for the supply schedule (pp. 93 f.). See Tarshis, *op. cit.*, p. 221, for a detailed statement which exposes the error. He has not proved that the supply schedule is affected by demand elasticity values; he has only demonstrated "the amount produced at a certain price . . . depends not only upon the marginal cost function but also upon the elasticity of demand" (p. 221). Obviously, but that is not the point.

² For a rigorous proof see Samuelson, *op. cit.*, p. 41. See Keynes, *General Theory*, p. 281; Marget, *Theory of Prices*, II, pp. 526 f.

³ Kalecki, *loc. cit.*, pp. 95 f.; and for a modified supply curve see Bain, *Price Theory*, p. 139.

But, of course, a seller will not in fact offer varying quantities for sale at varying prices. That is the behaviour of the maker-seller in markets where prices are determined by mutual "bargaining." In such "bargaining" markets the eventual price outcome can be understood only in terms of conventional supply schedules. But the behaviour of the maker-seller in industrial markets cannot be understood in these terms. As Scitovsky has argued, here we deal with exchange at "set" as distinct from "bargained" prices, a completely different category of market behaviour.¹ The price is not "bargained" but "set" by the "price-maker" to constitute the terms on which supply will be available. Bargaining will be involved, but only at the fringes of a price-offer and as its background. The price-offer thus constitutes a horizontal price-line holding good over the range of quantities which in fact the maker-seller is prepared to provide at that price. Obviously the supply schedule will fix the limiting values of the price-offer. In terms of Fig. 1 at price ON the price-offer line would not extend

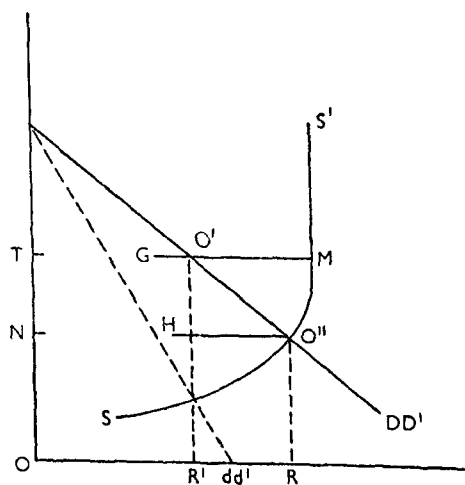


FIG. 1

farther than O'' ; and the price-offer line GO' will extend to GM . If demand presses beyond this point the maker-seller will be strongly tempted to raise his offer-line; but he may also "discourage" demand or build up a backlog of deferred orders. When the demand schedule retreats to the left there will likewise be a point at which the maker-seller will revise his price-offer. Hence the price-offer line can best be represented as a short horizontal line extended to meet the supply schedule and specifying the range of supply which at the given price the maker-seller is prepared to offer. These price-lines can be averaged for the various product lines in the industry and aggregated to form an industry price-offer line.

¹ Scitovsky, *op. cit.*, Chapter II

It is such a price-offer line which A. W. Marget advocated in his *Theory of Prices* and for which he developed a strong theoretical case. Marget analysed the relevant literature and discussed it in copious detail. He specifically identified the characteristics of the price-offer line and pointed out the role which it must play in market analysis. He expressly differentiated between it and the cost-derived supply schedule.¹ But possibly because the analysis was imbedded in an ostensibly polemical work addressed primarily to issues of Keynesian economics and because so much of the analysis is scattered over footnotes, his findings were unnoticed. Scitovsky picked up the same notion of market supply-curves in the form of "horizontal straight lines" and drew the diagrams to inform his readers, but did not himself call attention to the novelty of his work.² Present-day theory has recognised the price-offer curve only in the disguised form of the aggregate revenue line of the "break-even analysis." For in fact this revenue curve, when translated into its average derivative, will become a horizontal line spelling out the price-offer which the firm has set. Hence it is understandable that as modern theory has become concerned with behaviour of industrial markets, it has increasingly turned toward break-even analysis with its implicit price-offer lines though oftentimes without full awareness of the theoretical implications involved.³

¹ See Vol. II of his *Theory of Prices* (1942), Chapter V, "Particular Demand Curves and the Determination of Money Prices" (pp. 221-319) and Chapter XI, "Particular Supply Curves, Stream Equations and the Determination of Money Prices" (p. 549-627). See particularly the development of the third of his "series of propositions whose acceptance must be regarded as essential to any satisfactory synthesis of that part of the 'general' theory of value . . . and . . . the relevant parts of the Theory of Money and Prices" (pp. 221 f.). This third proposition holds that "the location of the . . . realized prices is given by the point of intersection of the market demand and supply curves prevailing at the time the price is realized" (p. 240). This Margetian "market supply curve" (as he denominates it) is expressly differentiated from our cost-derived supply schedule or from equilibrium values of a supply schedule (see pp. 233 ff., 537 ff.). Marget in his analysis of Chamberlin's work specifically identified the market-supply schedule in a particular case as a horizontal supply line (see p. 257, n. 81). Marget showed how a maker-seller will consider both the demand and supply schedules in deciding "what the supply price will be in any given market situation." This is, he noted "quite a different thing from saying that cost curves are in themselves sufficient, in all cases, to decide what the market supply price will be" (n. 82, see also n. 83). Marget distinguished between plans and calculations embodying a phase of what he called the "pre-formation" of prices and the actual decision of the price-maker to fix on a price-offer (pp. 239 ff., 261 f., 557 ff., 583 ff., 604). I think Wiles (in his *op. cit.*, Chapter. X, "The Supply Curve") attempted to elaborate the same kind of distinction between what we call offer and supply schedules and what he termed "production supply curves" and "sales supply curves."

² See Scitovsky, *op. cit.*, pp. 294 f.

³ Hence it is not surprising that Wheeler and Papandreou in their outstanding text on industrial economics develop their analysis of market behaviour in terms of break-even curve analysis (*op. cit.*, Chapter 5). P. W. S. Andrews in his *Manufacturing Business* actually derives the "average revenue per unit curve" and plots this as a horizontal line in the spirit of break-even curve analysis (see Diagram III and IV, pp. 255-6). See also the realisation by J. Dean of the usefulness of the revenue projection in break-even curve analysis as arising from the "constancy of selling price" (see Dean, *Managerial Economics*, pp. 337 f.). I hasten to add the break-even curve analysis is only one phase of analysis which supplements—rather than replaces—existing analyses.

III. EQUILIBRIUM VALUE, STATICAL ANALYSIS AND REALISED MARKET PRICE

Now that we have derived industry demand, supply and offer schedules we can set these to work in their simplest possible statical setting assuming a prolonged period in which price can be "adjusted" to given conditions of supply and demand. We have something to gain by going through the static exercises. But since the field of analysis is well-trodden, our rehearsal can be summary.

We will develop our analysis with the aid of the projections in Fig. 1. The supply schedule SS' starts at the lowest value of AVC for any of the supplying firms; it rises gradually as additional supply capacity, rates of plant utilisation and factor-supply prices are stepped up; and it develops a "kink" and becomes vertical when capacity output is reached.¹ We draw a regular demand schedule or average revenue curve and its side-shadow, dd , a marginal revenue curve. GM and HO'' are two sample price-offer lines. We will here call by the term "sellers' market" any price-offer line which is below ON ; whereas any price-offer line above ON will tend to create conditions corresponding to a "buyers' market." Though these terms have been otherwise used by Joan Robinson, they seem to fit the usage we mean for them.² For a price-offer line above ON will bring about a market condition in which suppliers will be willing to sell more than they will in fact sell at the prevailing offer-price. This "excess supply" will tend to make sellers anxious to please buyers and will provide buyers with bargaining power by which they may cause the price-offer line to fall to a lower level. The converse condition in which there is "excess demand" at a prevailing price-offer line corresponds to a so-called "sellers' market" (abundance of demand, customers can be chosen, quality standards can be relaxed). A price-line at ON will satisfy the conditions of "market-clearing" developed in recent analysis and corresponds to our old conceptions of the "equilibrium value of price" at which Q_s equals Q_d .³

Where will the price-offer line tend to settle under statical conditions? A price-offer line cannot readily become stabilised below ON for any maker-seller could raise his price and increase his profits. Since price-increases would bring no penalty other maker-sellers would follow suit; and thus price would gravitate toward ON . Similarly, price-offer lines above ON will be unstable, since any maker-seller will increase the profit-intake by reducing his price whether or not his competitors follow suit. A price-line at ON would yield the greatest profit return to the industry, but it would

¹ On this "kink" which Joan Robinson makes "angular" see her *The Accumulation of Capital*, p. 184, and Boulding, *Economic Analysis* (3rd ed.), p. 572.

² Robinson suggested them to characterise any market condition in which DD intersected SS above or below the "kink" (see *op. cit.*, pp. 187 f.); see also Wiles, *op. cit.*, 60 ff.

³ See L. Klein, *Economic Fluctuations in the United States*, pp. 51 f.; Hicks, *Value and Capital*,

be a difficult price-line to sustain under conditions of appreciable "excess supply." Maker-sellers will be anxious for additional business, price-concessions are possible. The greater the gap between Q_s and Q_d evoked by intersection of demand and price-offer schedules—i.e., the greater the relevant demand and supply elasticities—the greater the market pressure to lower the price-offer line and bring it closer to ON . But whether price-lines will in fact settle at equilibrium value level will in fact depend upon the strength of the competitive pressures in the industry, the fluidity of the price-making process, the "norms" of price-setting developed by price-makers.

Within the frame of a statical formulation of the problem we can hardly say much more. But, of course, price in industrial markets is not adjusted afresh to *given* and *experienced* conditions. The task faced by the price-setter is that of *altering* some *pre-existing* price-offer line carried over from a past period to meet the needs of *newly developing* conditions for which the past yields but dim guidance. And the price-maker faces the issue of price-alteration in industrial markets with a bias for price-stability. As Scitovsky has urged, it is an essential feature of his offer to do business on set terms that this offer must stand unchanged for a spell. This is first of all a matter of "commercial usage and ethics" and of convenience to buyers, who are put to greater trouble by an unstable or shifting price-universe. Price alteration also has its specific costs arising out of the issuance of new catalogues and sales literature needed to make the new price-offer effective and to dispel any illusions or misunderstandings. At best the new price-offer will become effective only at a lag. Then, too, price-alteration may, if it is not carefully timed and programmed, encourage speculative tendencies in buyers, and thus tend to unbalance market behaviour.¹ Finally, price alteration may upset the balance of power among rival firms in the industry and may be regarded as an aggressive thrust. As such it may induce retaliatory responses which may upset valuable and precarious competitive understandings.² For all these reasons it is a fundamental principle of price-making in industrial markets that price-alterations should be as few as possible and should pursue a single direction without reversing course.

Alteration of prices only periodically and in the direction of a consistent price-trend both simplifies and complicates the price-maker's task in industrial markets. His task is simplified because he is not forced to adjust his price-offer to meet temporary or special conditions of supply and demand. The price-offer line and equilibrium value must diverge in a constantly

¹ See on all this Scitovsky's brilliant performance "The Stability of the Price-Maker's Behavior," Chapter XII of *Welfare and Competition*; on the more empirical level see the excellent analysis in Nourse, *Price-Making in a Democracy*.

² See Rothschild's justly famous essay for emphasis on all of this. "Price Theory and Oligopoly," *Economic Journal*, September 1947, pp. 299–320. An important empirical characteristic of business enterprise in industrial markets is its oligopoloidal characteristics as noted in our introductory remarks.

changing world. Nor need a price-offer line coincide with maximum profitability criteria. But the task of the price-maker is more complicated in that he must consider long-run or distant needs, and among these is surely the future level of equilibrium values. He will not want to embark on a course of price-change which will carry him and his industry away from the drift of future equilibrium values. For the greater the spread between realised price and equilibrium value the more unsupportable the level of price will become. Yet the task of gauging price with reference to future states of equilibrium value would appear nearly impossible when even a present demand curve, as Robinson puts it, "is a mere smudge to which it is vain to attribute elegant geometrical properties."¹ Obviously then we have a tough nut to crack. For pricing must conform with admitted leeways to the pathway of future equilibrium values, though even present equilibrium values are difficult to estimate. It is to this problem and to the devices and short cuts evolved by the business world for its solution—and their theoretical reflection in classical economics—that we turn to consider in the concluding section of our paper.

IV. BACK TO MARSHALLIAN NORMAL VALUE AND REPRESENTATIVE FIRMS

The task of projecting future equilibrium values is very difficult, particularly for time-periods which are reasonably close to us. The supply schedule may be readily predicted, since it will carry over from the supply schedule of the immediate present, and this may be gauged in most industries by widely distributed information with regard to production capacity, cost levels and and factor-supply prices. The demand projections here are the troublesome factor. These projections first involve identification of elasticity characteristics for which quite extended market research would be needed. Then there would be involved a total income projection and the likely ruling prices of the most closely related product groups. Sudden changes of taste could be ruled out. But underlying trends of demand generally or in the structure of demand for various product varieties would play a material role, and these cannot usually be presented as a simple function of time. Since we are dealing with numerous independent estimates, the total range of error with regard to equilibrium value would be quite wide. Only where demand was highly inelastic price-wise, insensitive to small income changes and based on very stable taste factors could the demand projection be very close to the mark.

It might be thought that the difficulties of determining equilibrium value through demand and supply projections would increase as we attempted to devise them for more distant periods. But this is not at all the case if there is a steady demand growth. Then the more distant the period, the more

easy to identify the most likely range of equilibrium value. It is not necessary to know in advance the exact rate of demand growth so as to be able to draw in advance the demand schedule for the n th period as it sweeps across the horizontal axis. The order of magnitude of that growth factor should be estimated. One should know whether in ten years the demand (say at a range of highly likely prices) will rise only by (let us say) the percentage growth of general population, or by the total rise in current national product or at a compounded rate of $x\%$ per year. Beyond estimates of this order of magnitude—which does not unduly strain the capacities of abler business-men—precision is not needed.

The reason why even a gross error in demand projection can be accommodated for distant periods and why we can even ignore knowledge of income and price elasticities is ready at hand for all trained Marshallians. We do not need to know how to draw the exact shape or to locate the general position of the demand curve because we can anticipate the behaviour of the supply schedule and the neighbourhood or range of its likely intersection and determination of equilibrium value. We can anticipate this behaviour when we are analysing the case of a growing industry—a special case of a growing economy—with its demand enlargements. Thus we know that growing demand will only be satisfied if supply capacity is increased and the supply schedule given a displaced and more extended sweep. Supply capacity can be only increased if fresh investment is undertaken. But this investment will not be undertaken except on the expectation of profitable prices prevailing over the anticipated life-time of the investment and the average rate of capacity utilisation expected to be realised over that life-time. The shorter the estimated life-expectancy of the investment and the lower the rate of average capacity-utilisation projected, the higher will be the average total unit costs of production projected in the new investment. Moreover, the rate of profit on the investment must be high enough to offset risks and disabilities from all sources allowance being made for the hopes for capital appreciation, other investment possibilities, chances for windfalls and the non-economic drives which are urging industrial expansion. Usually the greater the anticipated rate of industry growth, the more liberal the profit margin to be taken if for no other reason than to provide the equity capital needed to finance the needed new investment. In some such wise each entrepreneur establishes a profit rate as a guide to his investment planning.¹

The basic unit-cost factors will be calculated assuming the newest and

¹ This profit rate may be a complex term with time characteristics, and obviously the profit rate utilised in investment planning will be reciprocally adjusted to—though in certain respects it will tend to control—experienced profit rates in this and other related industries. The relationship here is a complex one. But in any case I cannot sympathise with Joan Robinson's difficulties with this question. She avows that "In reality to find the expected rate of return which governs investment decisions is like the famous difficulty of looking in a dark room for a black cat that probably is not there and to give a true account of realized returns is like the famous difficulty of the chameleon on a plaid rug." Robinson, *The Accumulation of Capital*, p. 192.

¹ J. Robinson, "Imperfect Competition Revisited," *ECONOMIC JOURNAL*, September 1953, p. 585.

best technology laid out on the optimum size within reach of most of the firms which make up the industry and assuming command over the efficiencies of supply which the expected aggregate volume of production will generate. We can add a basic inflation trend of increase in basic supply factor prices; and we can offset this by allowance for an annual increase in productivity. The investment returns will be calculated on the assumption of the "normal" management of the "representative firm," that idealising construct which because it coincides with no exact reality has been so carelessly discarded from economic theory.¹ The "representative firm" is merely a mental construct to gauge abnormalities in any given firm which would require correction or offsetting before that firm's own experience would serve as a benchmark for gauging normal value. A new firm struggling to become established in its position in the industry, an old firm with a "derelict" management, a giant firm with a lay-out of excessive overhead costs can gauge normal value only by "correcting" their own cost-experience or investment projections with reference to some idealised construct of a more "representative" experience.² Of course, this correction is necessary if entry is open and if the number and quality of expanding firms is appreciable. If entry is highly restricted and the quality of the competitors can be appraised through specific identification, then we would "correct" not with the results of some unspecified "representative" firm but a set of specific firms with definitely known characteristics. In many ways the concept of the "representative firm" best fits the case of the generalised "potential competitor" when access is moderately open.

Normal value will thus be the average total unit costs (including needed profits) of new investment projects geared to the new technology and laid out on optimum size lines over the prospective life-time of the investment project. Normal value is thus an expectational level of value and is very closely related to the rate of returns on investment computed for a discrete investment project. But though normal value is only an expectational entity, it has crucial operating effects. For if the trend or level of realised prices is below current levels of projected normal value, new investment will not be forthcoming. The levels of normal value will diverge for various members of an industry so that some firms may be willing to expand capacity when others are holding back. There will obviously be a functional

¹ Thus H. J. Davenport as an outcome of a lengthy analysis complained that "generously estimated, therefore, the chances are some billions to one against any particular firm fulfilling these cumulative requirements [of the representative firm]." *The Economics of Alfred Marshall*, p. 373. But the "representative firm" is not a firm at all. He is only a mental construct against which operating experience is evaluated.

² See particularly Marshall, *Principles*, pp. 317 f., where Marshall worded the case as if he were looking for some actual firm which is actually "selected" on the basis of "a broad survey" (p. 318). But in other contexts, see chiefly p. 459, the representative firm is a "device" which comes to "our aid" by "imagination." We are specifically warned that "we cannot then regard the conditions of supply by an individual producer as typical of those which govern the general supply in a market."

relationship complexly developing over time between levels of realised price, induced new investment activity and projected levels of normal value. The slackening or acceleration of the rate of growth of capacity in an industry may not immediately affect levels of realised price. But obviously the growth of capacity moves downward the levels of equilibrium value, and to this extent exerts a downward influence on price. And if demand is steadily growing the regulative role of normal value will become more potent and must become controlling.

We can now see how crucial is the setting of increasing secular demand growth with positive investment activity.¹ For it is only with the integration of new investment planning with demand expansion that long-run rational price-strategy and planning becomes feasible in an open industry. Industries which adhere to normal value concepts and guides will have a minimum of price-instability. There will be less inventory speculation in their products, and hence stabler overall rates of operation, and with this steadier investment planning. The procedure of price-calculation is greatly facilitated, since it involves a series of basic cost projections which the business-man readily understands. By relaxing or intensifying marketing effort when normal value pricing would run short or exceed optimum capacity utilization an effort is made to achieve this utilisation-state. And meeting long-run needs will generate the reserves which can take care of short-term inadaptabilities. Pricing on the basis of normal values minimises the occasions for competitive wars and facilitates organic growth and smooth balance.² Is it any wonder that the Oxford study and every other serious study of the price-making process in business enterprise on the commodity level finds that full-cost pricing standards prevail, modelled variously but predominantly along Marshallian lines of normal value?

Naturally, normal value will not prevail rigidly even on the commodity level.³ There will be a tendency to modify in the direction of equilibrium value when the gap between normal trend-value and a given equilibrium value is considerable. It takes a very mature industry with a good deal of price-discipline to hold prices or the price-line below equilibrium values in the interest of following an anticipated trend-line of normal values. Where inflation factors are prominent and the long-run picture confused, there will be considerable temptation for industrial pricing to adapt itself to market

¹ For emphasis on the crucial role of "expanding demand" cf. Knight's 1921 article (in his *The Ethics of Competition*, p. 211).

² Thus we disagree with Kahn's interpretation of what Joan Robinson so happily called a "convenient institution," namely price-leadership or the above-noted collusive aspects of "normal value" pricing. Kahn wrote that full-cost pricing "plays the canonical role of keeping prices down at times of scarcity as well as keeping them up at times of abundance." Kahn, "Oxford Studies in the Price Mechanism," *ECONOMIC JOURNAL*, March 1952.

³ On the level of product-varieties its application is even looser or, as Marshall put it, "the influence of cost of production on value does not show itself clearly except in relatively long periods; and it is to be estimated with regard to a whole process of production rather than a particular product." *Principles*, 361 f.

conditions. Since new investment activity which enlarges capacity will usually embody new innovations—did not Marshall tell us that “we are here verging on the high theme of economic progress”?—this may make future cost projections uncertain, thus forcing an industry to price chiefly on the basis of current conditions or fissioning the industry into divergent basic price policies.¹ As Marshall pointed out, normal value will not yield a unique solution under conditions of diminishing costs as the aggregate scale of production is increased.² And Marshall himself pointed out how industry attempts to insulate itself from following equilibrium values when demand schedules fall off in what are regarded as temporary trade “dips.”³ New industries with fast-growing demand schedules will tend to treat themselves to abnormally high levels of profit return if for no other reason than to finance expansion on the necessary scale.

We thus see that dogmatic polarisation of “monopolistic” (marginal), “equilibrium” (market-clearing) and full-cost (normal-value) theories of pricing is theoretically invalid and practically nonsensical. These three criteria of price will all be effective in industrial markets, though to varying degrees and in varying and complex combinations. They support, interfuse with and displace each other by turns and according to circumstances, and set limits within which realised levels of price can move. Real market behaviour can only be analysed and understood by using all three criteria.

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¹ Marshall, *Principles*, p. 461.

² *Idem*, Appendix H.

³ *Ibid.*, p. 376.

ECONOMIC INTEGRATION AND THE AMERICAN EXAMPLE¹

It would be wrong to suppose that the current boom in customs unions and free-trade areas in several parts of the world owes all its vigour to economic theory. Much of the enthusiasm is based ultimately on political considerations. Still, in so far as economic reasoning has played its part, there can be little doubt that the spectacular success of the United States is uppermost in people's minds. It is freely asserted that American experience clearly demonstrates the advantages of a large integrated economy in exploiting to the full the benefits of mass production and specialisation.

The economic case for the integration of western Europe has been summarised as follows:

“In many fields there is an urgent need to transcend the many small national markets which are no longer adequate to cope with modern technological developments. On the other hand, many considerations, primarily political, render it impossible to lead this argument to its logical conclusion, *i.e.* world-wide integration. In fact, under contemporary conditions of State interventionism, economic integration can only become a reality between such like-minded nations as are already closely linked together, *inter alia*, by their economic interests. Western Europe provides such a setting, or at least an approximation to it.”²

It has also been argued that there are certain instances, such as oil refineries and continuous wide-strip steel mills, “in which the full-time operation of a single modern plant of optimum size produces an output that exceeds the entire national demand of some of the smaller Western European countries.”³

Notwithstanding the views of these and other authorities, the idea that the fullest exploitation of modern technology is obstructed by small national markets is not at all well documented. It would seem in fact that even the smaller western European countries have a national demand large enough to permit optimum production of the great bulk of goods and services.⁴ Such countries may satisfy their demand for goods which they cannot produce economically for themselves—whether because of inadequate size of the national market or for any other reason—through foreign trade. Nor is it

¹ The views expressed in this article are those of the author and do not necessarily represent the views of the United Nations Secretariat, to which he belongs. He wishes to acknowledge statistical help received from Mr. A. Khan and Mr. K. Moksnes.

² Research Directorate of the Secretariat-General of the Council of Europe, *The Present State of Economic Integration in Western Europe* (Strasbourg July 1955), p. 94.

³ T. Scitovsky, “Economies of Scale, Competition and European Integration,” *American Economic Review*, March 1956.

⁴ Inadequacy of national markets is, however, much more likely to be an obstacle to efficient production in certain parts of the under-developed world, such as, for example, Central America.

out of the question for these countries to set up even such plants as oil refineries provided they can sell part of their output abroad. An excellent case in point is the Netherlands, which in 1957 produced 14.1 million tons of petroleum products, while total domestic consumption amounted to only 5.5 million tons. Such possibilities are evidently not precluded even where there are considerable tariff and quota restrictions between countries, as there are in western Europe at the present time. A demonstration of the case for economic integration or free trade therefore requires much more than an exposition of the arithmetic of indivisible productive techniques.

Some doubt was thrown by Rostas on the importance of the size of the market in explaining the pre-war United States lead in output per man-hour over the United Kingdom. He concluded that:

"There is some inter-relationship between size of the market and productivity in the sense that the greatest relative advantage in productivity in the U.S. is shown in industries where the U.S. market is relatively very big. . . . But otherwise there does not appear to be a close inter-relationship. In cases where the U.S. market is big . . . relative productivity is not above average. . . . While it is obvious from this comparison that in certain industries the size of the market has an influence on output per worker, this is not as great as is usually assumed. If we compare the relative productivity and the size of the market in U.K. industry in relation to Sweden and Holland . . . we can see that relative productivity is in no way related to the size of the market. This points to the fact that the optimum plant (or firm) and specialization can be achieved within the limits of a smaller market (output)." ¹

Using a more limited sample for a post-war comparison of United States and United Kingdom output per man-hour, Marvin Frankel concludes that "there appears to exist no systematic relationship between differences in market size and differences in productivity." ² Only after excluding from consideration petroleum refining ("because of the grossly disparate importance of this industry in the two countries") and certain other industries ("whose markets tend to be narrowly regional or local in one or both countries") does Frankel find a correlation of 0.70 between these two variables.³ He points out, however, that:

¹ L. Rostas, *Comparative Productivity in British and American Industry* (Cambridge University Press, 1948), p. 59.

² Marvin Frankel, *British and American Manufacturing Productivity* (University of Illinois, 1957), pp. 64 *et seq.* Frankel's total selected sample of industries, before exclusions, covered about 16% of manufacturing employment in Britain and 18% in the United States. The Rostas sample of industries covered about half of the value of net output in Britain and two-fifths of the value of net output in United States manufacturing industry.

³ Frankel finds a partial coefficient of correlation between United Kingdom-United States differences in market size and in output per man-hour, with capital per worker and size of plant held constant, of 0.79. He warns the reader about the restricted character of the sample, and about the possibility of spurious correlation when the same factor, such as number of workers, is present in both dependent and independent variables.

"This relationship of itself gives little insight into the interaction of the two variables. . . . Initially large markets in the United States may have conduced to high productivity levels. . . . A second alternative is that high productivity, a result of some third variable, creates its own large markets through the consequence of low prices. . . . A third alternative would identify both high productivity and large markets as a joint outcome of some third factor. . . . The mere coincidence of large markets and high productivity does not indicate which of these interpretations, or combinations of them, should be preferred."

Particularly damaging to the view that large markets generate high productivity is the fact, noted by Rothbarth ¹ and others, that United States industry was more efficient than British industry as long ago as 1870, if not earlier, when the size of the United States market for manufactures was probably smaller than that of the United Kingdom. The belief that American productivity was superior to British productivity during the second half of the nineteenth century receives support both from qualitative evidence based on the findings of contemporary observers ² and from extrapolations into past periods on the basis of the Rostas-Frankel estimates of current productivity relationships, combined with available data on past trends in industrial output and employment in the two countries.³

If it were true that under present conditions small countries are precluded from taking full advantage of the economies to be realised from mass production and specialisation, this ought to be reflected in their *per capita* incomes. For example, countries ranked in descending order of market size ought presumably to show declining *per capita* incomes in terms of dollars. This is not, in fact, the case. It hardly seems necessary to go through the motions of calculating correlation coefficients to prove the point that in a world in which India, a country of nearly 400 million people, has one of the lowest *per capita* incomes and in which New Zealand, with a population of little more than 2 millions has one of the highest, the size of the national market bears little or no relation to productivity per head.

It might be argued that under-developed countries should be excluded from the reckoning, since the market economy accounts for only a fraction of their total product. In Table I nine of the industrial countries are ranked in terms of the dollar value of their *per capita* output in 1955, on the basis of

¹ Erwin Rothbarth, "Causes of the Superior Efficiency of U.S.A. Industry as compared with British Industry," *ECONOMIC JOURNAL*, September 1946.

² Thus Rostas, *op. cit.*, p. 61, cites evidence that as long ago as the 1850s British observers were impressed by the unusually widespread and rapid application of machinery and mass production methods in the United States.

³ Frankel, *op. cit.*, estimates that United States labour productivity probably began to forge ahead of United Kingdom productivity somewhere between 1830 and 1860. The trend he establishes in the ratio of United States to United Kingdom productivity from this period to 1955 appears to be consistent with the data given by F. W. Taussig in "Labour Costs in the United States Compared with Costs Elsewhere," *Quarterly Journal of Economics*, November 1924; Taussig shows ratios of United States to United Kingdom physical output per worker ranging from 1.2 to 2.9 for several industries in 1907-9.

purchasing-power parities established by Milton Gilbert and associates. Corresponding data on total output and on the ratio of exports of goods and services to total output are shown alongside. The coefficient of rank correlation between *per capita* and total output is less than 0.3. Nor is the correlation improved—quite the contrary—by taking into account the fact that

TABLE I

*Total and Per Capita Output, and Importance of Foreign Trade,
in Nine Industrial Countries, 1955*

(Ranks shown in brackets)

Country.	Per capita G.N.P. in dollars.	Total G.N.P. in billions of dollars.	Percentage ratio of exports of goods and services to G.N.P.
United States	2,310 (1)	380.0 (1)	6.7 (9)
United Kingdom . . .	1,152 (2)	74.9 (2)	23.6 (5)
Belgium	1,137 (3)	11.9 (7)	35.8 (3)
Norway	1,061 (4)	4.8 (9)	42.0 (2)
Denmark	1,007 (5)	5.7 (8)	33.5 (4)
Germany, western . .	975 (6)	66.6 (3)	21.3 (6)
France	969 (7)	55.7 (4)	15.8 (7)
Netherlands	892 (8)	13.1 (6)	52.8 (1)
Italy	464 (9)	38.7 (5)	12.7 (8)

Source: Milton Gilbert and Associates, "Comparative National Products and Price Levels," *O.E.E.C. General Statistical Bulletin*, January 1958. Estimates of G.N.P. based on United States relative price weights for all countries.

countries vary widely in the extent to which they participate in international trade and in the greater specialisation which this makes possible. Thus neither the size of the market nor the share of exports in total output—nor both factors taken together—are of any value in explaining inter-country differences in *per capita* income.

A second principal contention derived from American experience is that economic integration is likely to promote a higher level of efficiency by encouraging competition. Scitovsky has argued, for example, that the western European economies are less competitive than the American, that less effort is made by producers in western Europe to expand at the expense of their competitors, that the small family firm in western Europe has a staying power that makes it outlast its period of profitability, and that—

"optimal plant is not likely to be built in an oligopolistic situation when the industry operates with a price structure that allows each member an adequate profit, even though productive methods are sub-optimal and costs higher than they need be. . . . In Western Europe, where national markets are often small and relations among competitors friendly and personal, there are many industries in which the above situation holds."¹

Scitovsky concedes that quantitative evidence for this view cannot be produced. He refers to Rosenbluth's findings that the general level of

¹ T. Scitovsky, *op. cit.*, p. 83.

industrial concentration is higher in the United Kingdom than in the United States, in the sense that industry by industry, the three largest firms account for a higher proportion of employment, on the average, in the United Kingdom than in the United States.¹ However, closer scrutiny of Rosenbluth's data suggests that the difference between the two countries is hardly of major proportions. Of fifty-seven industries examined by Rosenbluth, thirty-three had higher concentration ratios in the United Kingdom than in the United States in 1935, and twenty-four had higher ratios in the United States.

More significant, however, is the fact that the data do not seem to warrant the inference that where concentration is high in the United Kingdom, productivity is relatively low. A comparison of Rosenbluth's data on concentration with Rostas' and Frankel's results concerning relative productivity in the two countries yields no such conclusion for the few industries in which this can be done.² Rosenbluth has also found that the concentration of industry is higher in Canada than in the United States,³ and it would presumably not be argued that the Canadian economy is subject to the same deficiencies in the field of competition as the European. Rosenbluth himself explains the higher Canadian concentration by saying that:

"Canadian industries have fewer firms because, while the average size of their firms is about the same as in the United States, the industries themselves are much smaller. This smaller industry size, finally, reflects the smaller size of the Canadian economy combined with an industrial pattern very similar to that of the United States."⁴

It is not at all unlikely that similar considerations apply to the United Kingdom, especially bearing in mind Sargent Florence's finding that measured in terms of the number of workers, the sizes of firms do not differ greatly between the United States and the United Kingdom.⁵

There are therefore no grounds for distinguishing between the United States and western Europe in terms of the extent of industrial concentration. If business-men have less dynamism in western Europe than in the United States, this has little or nothing to do with differences in the degree of oligopoly.

It is also important not to lose sight of the fact that however well entrenched western European producers may be in their domestic markets, a much larger proportion of their output than of the output of their opposite

¹ Gideon Rosenbluth, "Measures of Concentration," in *Business Concentration and Price Policy* (Princeton: National Bureau of Economic Research, 1955).

² Of twelve industries for which data on concentration and relative productivity are available from these sources, seven show relatively high productivity differentials associated with a relatively high degree of concentration and only five show results consistent with the view that high concentration makes for low productivity.

³ Gideon Rosenbluth, *Concentration in Canadian Manufacturing Industries* (Princeton, 1957).

⁴ *Ibid.*, p. 20.

⁵ P. Sargent Florence, *The Logic of British and American Industry*, p. 36.

numbers in the United States has to compete in world markets,¹ where it is probably far more difficult to come to restrictive agreements than it is at home. It is not unlikely, in fact, that competition in overseas markets by European producers, both with one another and with United States producers, is even more vigorous than internal competition within the borders of the United States. In view of the importance of overseas sales in the profits of European companies, it cannot be taken for granted that competition is a less important influence on them than it is on American companies.

But let us even suppose it were true that imperfect markets and restrictive business practices are more common in western Europe than in the United States. Why should it be supposed that economic integration would, in itself, undermine such conditions? The European Coal and Steel Community has many achievements to its credit, but it is nowhere argued that it has led to greater competition between coal and steel producers. Nor does the adoption of the basing-point system of pricing for steel within the Community imply a particularly competitive situation. Despite the establishment of the Community, Italian steel prices seem to have been maintained at levels well above those corresponding to delivered costs from producing centres elsewhere in the Community.

One reason to expect more aggressive competition in a larger unified market is doubtless that the difficulty of arriving at and enforcing restrictive agreements among producers multiplies rapidly as the number of producers increases. There is obviously something in this argument, but how important it is in practice is not easy to tell. In the first place, some of the major industries—those, in fact, which have exhibited the greatest dynamism both in the United States and elsewhere—are precisely those in which the bulk of the output is produced by a handful of companies, and which would therefore have relatively little difficulty in coming to market-sharing and other agreements if they chose. The operations of the Brussels steel cartel are suggestive of what might be feasible in this sphere. It is not at all impossible, in fact, that some industries which are now prepared to compete vigorously on the international market so long as the solid core of home sales is assured, might feel themselves compelled to come to agreements with their counterparts abroad if they felt that foreign competition might endanger them even at home. A case in point might well be the motor-vehicle industry. This is the western European industry perhaps most frequently cited as needing the stimulus of larger integrated markets to promote efficiency through greater competition and standardisation. Yet surely here is an industry which would have much less difficulty than most in maintaining restrictive agreements—even if the integrated market covered the whole of western Europe. For the number of producers is relatively small and confined to a few countries. Similar considerations probably apply in many of the other European industries.

¹ See Table I, third column.

Any argument for economic integration based upon the hypothesis that there is a more vigorous spirit of competition in the United States than in western Europe, cannot therefore content itself with demonstrating that the hypothesis itself is true, but must go on to show that the difference is due to the number of frontiers in western Europe, and not to other factors¹ which economic integration *per se* might do little or nothing to eliminate.

A third argument for economic integration is that it provides the most efficient means of spreading higher levels of productivity and *per capita* income throughout the territory encompassed, and that this likewise is one of the important lessons to be learned from American economic history. Uri writes that “L'évolution américaine récente fait apparaître la décentralisation comme la forme la plus élevée du développement.”²

Before studying the American example more closely, it ought perhaps to be observed that history records plenty of examples of the opposite tendency—namely of economic polarisation as the rich areas in a community or country grow continuously richer and the poor areas poorer.³ It has been shown, for example, that in France and Italy the gap in economic development between regions has been steadily growing while employment in industry has been expanding fastest in the most highly industrialised areas. And while regional differences in incomes and in the degree of industrialisation seem at the present time to be relatively small in Germany and the United Kingdom, it is pointed out that historically, the economic development of both these countries was accompanied by economic backwardness or stagnation in certain regions, notably the area that is now the Irish Republic and the German territories incorporated in Poland and the Soviet Union at the end of the Second World War.⁴

Nor does a careful examination of the history of the United States suggest that the forces of economic integration in that country have been as strong as is often contended. In the first place qualitative information suggests that throughout the greater part of the nineteenth century the Southern states lagged considerably behind the rest of the country in respect to the growth of output and income. Such manufacturing as existed in the South at the end of the eighteenth century disappeared quite rapidly with the development of the plantation economy. Meantime manufacturing was expanding rapidly in the North under the protection of high tariff barriers. By 1850–60 the proportion of the population engaged in manufacturing was about 1 to 8 in New England as against only 1 to 82 in the South.⁵

¹ Many possibilities suggest themselves. It is, for example, not surprising to find in the United States the natural vitality of a population based largely upon immigrant stock, active and resourceful enough to leave their home countries and determined to succeed in a new environment.

² Pierre Uri, “Harmonisation des politiques,” *Revue Economique*, March 1958.

³ The factors tending to lead to this result are reviewed at some length by Gunnar Myrdal in his *Economic Theory and Underdeveloped Regions* (Duckworth, 1957).

⁴ Economic Commission for Europe, *Economic Survey of Europe in 1954* (Geneva, 1955), pp. 142, 144.

⁵ See Victor S. Clark, *History of Manufactures in the United States*, 1929 edition, Vol. 1, p. 580.

A reasonably good indicator of the relative pace of economic progress in the South¹ may be found in data on the level and rate of change of the urban population, which is strongly correlated with the growth of factory production,² and hence with the rate of increase in *per capita* income. It will be seen from Table II that at the beginning of the nineteenth century the percentage of the population living in urban areas in the South was already lower than for the United States as a whole, but that the gap was being

TABLE II
United States: Growth of Urban Population

Year.	Percentage of total population recorded in urban areas.		Percentage increase in urban population over preceding census.	
	Continental United States.	South.*	Continental United States.	South.*
1790	5.1	2.1		
1800	6.1	3.0		
1810	7.3	4.1	59.9	84.6
1820	7.2	4.6	63.0	83.7
1830	8.8	5.3	31.9	42.4
1840	10.8	6.7	62.6	47.6
1850	15.3	8.3	63.7	53.9
1860	19.8	9.6	92.1	60.7
1870	25.7	12.2	75.4	43.4
1880	28.2	12.2	59.3	40.3
1890	35.1	16.3	42.7	34.8
1900	39.7	18.0	56.5	61.7
1910	45.7	22.5	36.4	35.6
1920	51.2	28.1	39.3	49.8
1930	56.2	34.1	29.0	40.4
1940	56.5	36.7	27.3	38.8
1950	59.0	44.0	7.9	18.5
			19.5	35.9

* United States Bureau of Census definition, as given in footnote above.

Source: *U.S. Census of Population 1950*, Vol. 1, Table 15, pp. 1-17.

narrowed, in relative terms, through a more rapid process of urbanisation in the South than elsewhere. By the decade of 1820-30, however, the ratio of urbanisation in the South had fallen behind that of the rest of the country, and continued in arrears until the decade of 1880-90. Not until the decade of 1910-20 did the rate of urbanisation in the South begin to lead the rest of the country by a considerable margin once more. It seems, therefore, on

¹ It has unfortunately not been possible to use the term "South" in a completely uniform sense in the course of the following discussion. So far as possible, the United States Bureau of the Census classification is adhered to. Under this classification the South consists of the following regions and states: South Atlantic—Delaware, Maryland, District of Columbia, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida; East South Central—Kentucky, Tennessee, Alabama, Mississippi; West South Central—Arkansas, Louisiana, Oklahoma, Texas. Departures from this classification are footnoted throughout.

² According to Frederic B. Garver, Francis M. Boddy and Alvar J. Nixon, *The Location of Manufactures in the United States 1899-1929* (University of Minnesota Press, 1933), the coefficient of rank correlation of the percentage of the population living in urban areas in 1930 with the number of wage-earners per 1,000 of the population in 1929, by states, was 0.72.

the basis of population data, that from about 1820 to about 1880 the economy of the South was probably lagging behind the rest of the country in much the same way as has occurred in France and Italy in more recent years.

From about 1900 onwards, however, the data suggest a change in the situation. This is confirmed by information on regional *per capita* income as a percentage of the national level available for various years going back to 1880, as shown in Table III. No relative improvement in *per capita* income in the Southern states (those referred to as the South Atlantic, East South Central and West South Central states) is shown from 1880 to 1900.

TABLE III
*United States: Regional Per Capita Income as Percentage of National Level, 1880-1950 **

	1880.	1900.	1920.	1930.	1940.	1950.
United States	100	100	100	100	100	100
New England	141	134	124	129	121	109
Middle Atlantic	141	139	134	140	124	116
East North Central	102	106	108	111	112	112
West North Central	90	97	87	82	84	94
South Atlantic	45	45	59	56	69	74
East South Central	51	49	52	48	55	62
West South Central	60	61	72	61	70	80
Mountain	168	139	100	83	92	96
Pacific	204	163	135	130	138	121
Arithmetic mean deviation of regional <i>per capita</i> income from national level, percentage points	45.6	36.6	25.7	31.1	25.0	16.9

* The regional definitions of the United States Bureau of the Census are used, except that the District of Columbia is omitted and Delaware and Maryland are included in the Middle Atlantic rather than the South Atlantic region.

Source: Richard A. Easterlin, "Interregional Differences in Per Capita Income, Population, and Total Income, United States, 1840-1950," in Conference on Research in Income and Wealth, *Studies in Income and Wealth*, Volume 24 (New York: National Bureau of Economic Research forthcoming), Table D-2. The entries for 1920, 1930, 1940 and 1950 are cycle averages for respectively 1919-21, 1927-32, 1937-44 and 1948-53.

Indeed, the East South Central states did not move significantly upwards, relatively to the rest of the country, until the decade 1940-50. The South Atlantic states did, however, show a marked upward trend in *per capita* income relatively to the country as a whole after 1900, and the East South Central states exhibited a weaker shift in the same direction, interrupted more sharply by the depression in agricultural prices in 1930.¹

¹ The rise in the arithmetic mean deviation of regional *per capita* incomes from the national level between 1920 and 1930 shown in Table III reflects the steep decline of prices received by farmers for their output in terms of prices paid by them for goods purchased, between those years. This tended to increase the income lag in the agricultural states at the lower end of the distribution, and conversely to raise the lead of such areas as the New England and Middle Atlantic states. From 1930 to 1950 prices received by farmers recovered in terms of prices paid to approximately the 1920 relationship. Thus over the period 1920-50 as a whole the terms of trade between farmers and the

There appear to be two main reasons for the relative increase in *per capita* incomes in the South since 1900. In the first place there were, as shown in Table IV, significant declines in the importance of agriculture, with its characteristically low incomes, and corresponding advances in higher paid forms of economic activity, such as manufacturing.¹

TABLE IV
Percentage Ratio of Agricultural to Total Workers 1870-1950 *

Year.	United States.	South.†
1870	51.5	71.5
1880	49.4	71.8
1890	40.6	62.8
1900	38.8	62.3
1910	32.5	56.3
1920	25.6	46.0
1930	21.4	39.0
1940	17.5	30.5
1950	11.6	18.8

* Data relate to males and females ten years old and over.

† United States Bureau of the Census definition, as given in footnote above.

Source: E. S. Lee, A. R. Miller, C. P. Brainerd and R. A. Easterlin, *Population Redistribution and Economic Growth, United States 1870-1950*, Volume 1.

The second main reason lies in the substantial migration which has been taking place for well over a century from low-income to high-income areas of the United States. This has involved not only heavy population movements from rural areas to the towns but also from one region to another. The South in particular has been an enormous reservoir of man-power for other parts of the country—North and West.

Several writers have suggested that there has been a long-term tendency

est of the community remained virtually unchanged, as may be seen from the following parity ratios (ratios of prices received by farmers to prices paid, 1910-14 = 100):

1910	107
1920	99
1930	83
1940	81
1950	101

Source: U.S. Department of Agriculture, *Agricultural Statistics 1954*, Table 660.

¹ It is true that the same development was taking place throughout the United States, as may be seen in Table IV. Given the greater importance of agriculture in the Southern economy, and assuming that the ratio of average agricultural to average non-agricultural incomes was the same in the South as in the country as a whole, the same absolute decline in the percentage ratio of agricultural to total workers both in the South and in the United States as a whole would have resulted in a greater *relative* increase in average Southern incomes, because they started from a lower level. In practice, the absolute declines in the percentage ratio of agricultural to total workers were larger in the South than in the country as a whole after 1910; and it is likely in addition that the ratio of average agricultural to average non-agricultural incomes was lower in the South than in the rest of the country. These two factors strengthened the relative advance in average Southern incomes resulting from the growth in non-agricultural activities.

for *per capita* incomes to rise most in the areas losing population by migration, and to advance least in areas of greatest increase in population.¹ Thus Easterlin has shown ² that during each of three periods (1880-1900, 1900-20 and 1920-50) the two lowest income areas were regions of substantial net out-migration,³ and two other regions of relatively low income were areas of net out-migration in at least one or two of the three periods. Conversely, all the high-income regions were areas of net in-migration. Moreover, there was a tendency for the magnitudes of the migration rates to be associated with the relative income levels. Easterlin concludes that the flow of persons among regions was a persistent force making for convergence of regional income levels.⁴

Significant though these influences were,⁵ they operated with much less force than might have been expected on the basis of the classical theory of international and inter-regional trade. Even by 1954, at the end of some eighty years of Southern industrial development, the South's share of 20% in total manufacturing employment in the United States compares with a share in total population of nearly 31%; and average incomes in the poorest states are still less than one-half of those in the richest.

Moreover, it seems that despite all the advantages of the unified United States market, with its free flow of labour and capital, the process of economic

¹ See, for example, Harvey S. Perloff, "Problems of Assessing Regional Economic Progress," in National Bureau of Economic Research, *Studies in Income and Wealth*, Volume 23, p. 49. It has also been suggested by Edward F. Denison *op. cit.*, p. 178, that the absence of a trend in the dispersion of *per capita* incomes during the post-war period (discussed further below) implies that the continuation in this period of differential regional trends in total income is associated with similar population trends.

² Richard A. Easterlin, "Long Term Regional Income Changes: Some Suggested Factors," in *Papers and Proceedings of the Regional Science Association*, Volume IV (forthcoming).

³ Easterlin includes both internal and international migration within the scope of his concept of net migration.

⁴ It should not be assumed that there is anything inevitable about this type of development. Lloyd Saville has shown (*Southern Economic Journal*, July 1956) that population growth in the south of Italy during the past fifty years has been little affected by domestic or foreign migration, while *per capita* income in southern Italy has been declining progressively relatively to the north-west since the beginning of the century. The lesser importance of internal migration in Italy than in the United States was probably due in part to slower growth of employment opportunities in northern Italy.

⁵ The narrowing of inter-regional differences in *per capita* incomes in the United States during the past half-century appears to owe little if anything to relative movements in wages. United States Department of Agriculture data (as set forth by H. J. Braunhut in the *Southern Economic Journal*, October 1949) indicate that the gap between monthly farm wage-rates in the South and in the country as a whole widened considerably between 1910 and 1948; and that the corresponding gap in daily rates did not narrow very much. Data published by the United States Bureau of Labor Statistics (*United States Monthly Labor Review*, Vol. 66, No. 4, April 1948) suggest that median Southern wage-rates in manufacturing industries were no higher a percentage of the corresponding rates in the North-east or Middle West in 1945-46 than they were in 1907. The frequently noted tendency for the South-North wage-rate differential to narrow in *specific industries* is not inconsistent with a stable differential in median wage-rates for manufacturing as a whole. As will be shown below, the industrial structure of the South is a relatively unfavourable one; and the over-all composition of manufacturing employment has shifted much more in favour of the high-wage-earning industries in the North than it has in the South.

decentralisation has been rather slow. Garver, Boddy and Nixon found that the "index of concentration" of industry, by states, declined only moderately from 1899 to 1929.¹ They concluded that although there was a remarkable increase in the number of wage-earners on the Pacific coast from 1899 to 1929, there was no general decentralisation of industry. The main Pacific industries were the "ubiquitous" ones—namely the industries typically dependent on local or regional markets—together with certain simple manufacturing industries based on indigenous raw materials (notably lumber). Likewise, in so far as there was an industrial drift to the South it was based mainly on textiles.

Similar results have been obtained by Easterlin. He finds no evidence of a pattern of change in agriculture since 1880 that would have consistently favoured low-income areas at the expense of high, thus making for convergence of regional levels of income per worker. He goes on to state that:

"An examination of almost sixty large manufacturing industries yields no more than a dozen where there was a fairly clear shift of substantial magnitude from high to low income regions. The well known shift of textile manufactures from the Northeast to the South accounts for about half of these; the others—also usually from the Northeast to the South—were the manufacture of cigarettes and other tobacco products; furniture; cooperage; marble, granite, slate and other stonework; beverages; and distilled liquors. Over against this one may set a roughly equal number of shifts that almost wholly benefited regions which already had relatively high income levels. This was notably so of iron and steel manufacture and various other metals industries and of several chemicals industries—candles and soap; paints and varnish; and drug and toilet preparations. True, most of these shifts were at the expense of a high income region, the Northeast, but since they tended to benefit other high income regions, principally the East North Central and Pacific, they would appear to yield little net tendency toward convergence of regional income levels."²

As a result of this, the South still has an industrial structure that is startlingly reminiscent of an under-developed country (though of course the average level of its *per capita* income is far higher than in any under-developed country). It will be seen from Table V that, like many of the under-developed countries, the South is most highly specialised in the textile industry, followed closely by industries processing local raw materials, especially lumber, tobacco and petroleum; furthermore, development has

¹ *Op. cit.*, Table III. The index of concentration declined from 73.9 in 1899 to 67.8 in 1929, the limits of the index being from 50 to 100. This index is the percentage ratio of the population living outside the States (ranked in order of the density of the industrial population) in which 50% of all industrial employees are found, to the total population of the United States. Thus the lowest index of concentration—50—implies that 50% of all industrial employees are found in states containing 50% of the total population of the United States; while the highest concentration theoretically obtainable is given by an index of 100, which implies that 50% of all industrial employees work in an area with zero population.

² Richard A. Easterlin, *op. cit.*

TABLE V

Distribution of Manufacturing Industries in the Southern States in 1947*

Industry group.	Specialisation ratio.†
Textile mill products	3.13
Lumber and products (excluding furniture)	3.09
Tobacco manufactures	2.93
Petroleum and coal products	2.03
Furniture and fixtures	1.36
Chemicals and allied products	1.32
Food and kindred products	1.05
Paper and allied products	0.99
Stone, clay and glass products	0.86
Apparel and related products	0.82
Printing and publishing industries	0.61
Primary metal industries	0.48
Rubber products	0.43
Transportation equipment	0.38
Fabricated metal products	0.37
Leather and leather products	0.34
Machinery (excluding electrical)	0.26
Miscellaneous manufactures	0.22
Instruments and related products	0.14
Electrical machinery	0.09
Total all manufactures	1.00

* Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas.

† Ratio of the share of the Southern states in the total number of production workers in each industry for the country as a whole, to the share of the Southern states in the total number of production workers in all manufacturing industries for the country as a whole.

Source: Based on a special tabulation prepared by the United States Bureau of the Census for the Bureau of Business Research, University of Alabama, as cited in H. H. Chapman and associates, *The Iron and Steel Industries of the South* (University of Alabama Press, 1953).

been largely in the early stages of processing. The South has far less than its fair share of industries producing finished goods, especially metal products, machinery and transport equipment—even if the term "fair share" is restricted to mean the same share as of manufacturing as a whole. The special structure of production characteristic of the South is further reflected in the following data on the distribution of employment in manufacturing in 1947:

	Employment in selected industries as per cent of total employment in manufacturing.	
	Southern states.	United States.
Textile, lumber and tobacco products	48.1	15.5
Primary and fabricated metal products, machinery, transportation equipment and instruments	13.3	41.0

Source: As for Table V.

It therefore seems to be an over-simplification to suggest, as Uri does, that United States experience reveals decentralisation as the highest form of

economic development. It is at least conceivable that had the South been able to limit imports of capital goods from the North, the low-income agricultural economy of the South would have yielded to industrial development much more than it in fact has, the industrial structure of the South would to-day be much less unbalanced, and the South would have, under its own control,¹ more of the industrial and other facilities essential to promote cumulative growth and a more rapid narrowing of the *per capita* income gap between South and North. What is surely remarkable about the gap is not that it was reduced from 1880 to 1950, but that it has lasted so long in the face of the competitive forces prevailing in the most dynamic economy in the Western World.

The above discussion relates to the long-term trend towards converging *per capita* incomes in the United States from 1880 to 1950. There is, however, strong reason to doubt whether this particular trend can be automatically projected into the future—still less used as an example to illustrate the probable consequences of economic integration in other countries. Reference to Table III will show that there was no reduction of regional *per capita* income differentials in the United States from 1920 to 1940. Since the Second World War, likewise, there has been little further narrowing of such differentials despite the rapid growth of the United States economy.² One factor helping to offset whatever tendencies to further convergence there may have been in recent years was the decline in prices received by farmers for crops produced in terms of prices paid by them for goods and services used—from 115 in 1947 to 84 in 1955 (1910–14 = 100).

This, however, is only a contributing factor in explaining post-war developments, since private non-farm income differentials have not narrowed very much either. The main reason for this follows from what was said above regarding the industrial structure of the South. The industries of principal importance in the South are those which, on the whole, have shown much smaller rates of expansion in the post-war period than the metal-using industries found mainly in the high-income areas. Increases in factory

¹ See, for example, H. H. Chapman and associates, *The Iron and Steel Industries of the South* (University of Alabama Press, 1953). In evaluating the slow development of iron and steel industries in the South, the conclusion is reached, *inter alia*, that: "The ownership of the largest units in the South by corporations whose operations are on a national or international scale removes most of the important decisions from the South and tends to place regional interests in a decidedly subordinate position" (p. 371). The evidence also suggests that the basing-point pricing policies of companies has tended to perpetuate the concentration of the iron and steel industry in existing centres, and has retarded growth in less-developed areas (p. 384); and that: "The record of prices of steel in Birmingham (Alabama) and of price differentials, when compared with other steel centers, is scarcely one that corresponds with what would be expected if pricing had been done with the interests of steel production in Birmingham as a central consideration. The evidence that could be assembled in this study indicates that Birmingham has been a relatively low-cost producer. . . . Yet prices of steel until 1938 were held above Pittsburgh and have never been permitted to fall below the base prices of other competing markets" (p. 388).

² The coefficient of variation of *per capita* personal incomes among the eight regions of the United States specified in Table VI below was 20.5 in 1947 and 19.0 in 1955.

pay-rolls in the South-east from 1948 to 1955 surpassed the country-wide average in seventeen of the twenty-one principal manufacturing industries, and matched it in two others. Yet total factory pay-rolls in manufacturing as a whole increased only a little more than the average for the whole country between these years, because of the unfavourable industrial structure.¹ Similar considerations appear to hold outside the manufacturing sector; they may also help to account for the stability in regional income differentials from 1920 to 1940.

In conclusion, it seems doubtful whether regional *per capita* income differences are now any smaller in the United States, with its vast integrated economy, than in western Europe, with its patchwork of independent states. The data in Table VI are suggestive in this respect. They show the dispersion of *per capita* personal incomes among the eight major regions into which the United States is divided by the Department of Commerce for the

TABLE VI

Variation in Income and Product in the United States and Western Europe in 1955

United States:

Coefficient of variation of <i>per capita</i> personal income, in current dollars, among eight regions *	19.0
--	------

Western Europe:

Coefficient of variation of <i>per capita</i> gross national product in current dollars, among eight countries,† based on purchasing power parities:	
--	--

At United States relative price weights—	
--	--

All eight countries	19.1
Excluding Italy	6.0

At European relative price weights—	
-------------------------------------	--

All eight countries	23.4
Excluding Italy	7.3

* New England, Mid-east, Great Lakes, Plains, South-east, South-west, Rocky Mountain, Far West, as defined in *Personal Income by States since 1929*.

† Belgium, Denmark, France, western Germany, Italy, Netherlands, Norway, United Kingdom.

Source: United States Department of Commerce, *Personal Income by States since 1929*; Milton Gilbert and associates, *Comparative National Products and Price Levels*, Table 2; O.E.E.C., *General Statistical Bulletin*, January 1958.

purposes of its regional income estimates. This is compared with the dispersion of *per capita* gross national product among eight of the major European countries as calculated on the basis of purchasing-power parities by Milton Gilbert and associates. The meaning of this comparison should obviously not be pushed too far. It is nevertheless striking that the coefficient of variation of *per capita* personal income or gross product was of a similar general order of magnitude in the two areas; and that excluding

¹ See U.S. Department of Commerce, *Personal Income by States Since 1929* (Washington, D.C., 1956), p. 21. The term "South-east" is defined in this publication to include Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, West Virginia.

Italy the coefficient was markedly lower in western Europe. Extrapolation of the data ¹ suggests that immediately before the Second World War regional *per capita* income differentials were actually greater in the United States than among the European countries listed, whether including or excluding Italy. Thus a unified market has apparently not induced any greater uniformity in the American economy than prevails in western Europe. This surprising result should in itself give pause to those who believe that the forces set up by economic integration are necessarily all-pervasive in their impact.

It does not follow from the foregoing discussion that arguments for economic integration are necessarily without merit, although a much heavier burden of proof lies upon the supporters of such arguments than has been assumed by them hitherto. What follows is that whatever benefits economic integration may bring under favourable conditions are neither as automatic nor as prompt as they are often held to be, if American experience is our guide; and that the American example cannot be held to rule out the danger of economic polarisation in an integrated Western Europe.

S. DELL

New York.

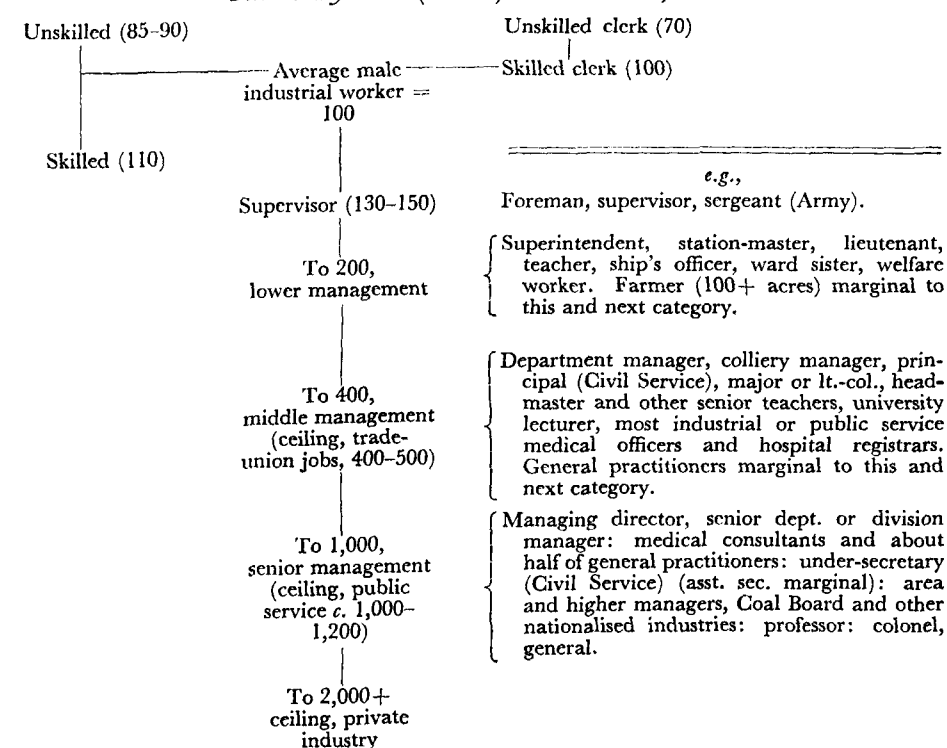
¹ Using volume indices of *per capita* gross national product for the eight European countries. For the United States pre-war data on state personal income are available directly.

THE WHITE-COLLAR PAY STRUCTURE IN BRITAIN

THE salary tree in Britain in the 1950s is shown in Table I. The base of the tree is the average earnings of male operatives, taken as 100. The average for skilled operatives would be around 110 and that for unskilled 85 to 90. Clerks' rates compare badly with these. A skilled male clerk, in the Office Management Association's Grade E, was likely in 1956 to have a coefficient of only about 100; his pay was thus equivalent to that, not of the skilled operative, but of the average or semi-skilled. Differentials between skilled and unskilled seem also to be wider in the office than in

TABLE I

The Salary Tree (Males) Great Britain, 1950s



the works. The least-skilled grade of adult clerk had in 1956 a coefficient of only about 70, 30% below the fully skilled. This put his income clearly below that of the unskilled labourer.

Supervisors, the next grade up, come in all shapes and sizes. One may be little more than an operative, another in effect a manager. Let us take as the standard what the National Institute of Industrial Psychology calls

the "Level B" supervisor, who in industry might be a foreman with 25-50 men and one or two assistants or charge-hands (levels C and D) under him. This man, or his opposite number in an office, is likely to have a coefficient of 130-150. The office supervisor's coefficient, compared to his own subordinates, is often higher than this. But since his subordinates earn less than their industrial opposite numbers, his coefficient compared to an average operative is not quite so impressive.

Lower management grades—superintendents, station-masters, army lieutenants, teachers—are likely to have coefficients in the range 100-200. Medium farmers, with, say, 100-200 acres, tend to fall on the margin between this group and the next above. Middle management grades, such as department or colliery manager or Civil Service principal, are usually in the range from 200 to 400. General practitioners are marginal to this group and the next. Senior managers or technicians or public servants—consultants, under-secretaries, managing directors, plus some other senior managers in large firms—are likely to have coefficients up to about 1,000. The ceiling for top jobs in private industry may go up to 2,000-3,000, or in a very few cases even more. That for the public services is usually around 1,000-1,200, with a few posts, such as chairman of a major nationalised board, at up to 1,500-1,600. For trade-union jobs the ceiling is usually around 400-500, the top of the middle management level; though one or two unions (they are not the biggest or best known) pay rather more. Besides these differences in direct pay, there are important differentials in fringe benefits; but of these, more in a moment.

At all levels there is a considerable spread according to region, industry, size of firm and sex. White-collar staff employed by large nation-wide organisations, public or private, are commonly paid 5-10% less in the provinces than in London. For clerks as a whole, whether paid on national scales or on local rates, the gap for similar grades between pay in the best-paid and worst-paid regions goes up to 25-30%. Similar grades of clerk or manager may also average 25-50% less in the worst-paid industries than in the best. The difference between small and large firms is difficult to put into figures for white-collar staff; but for manual workers it seems that small firms not uncommonly pay 10-20% less than large for workers of apparently similar grade. Sex differentials are in the process of being abolished in the public services, and in the professions they have always been limited, though there are exceptions: women personnel managers, for example, were averaging in 1956 20-40% less than their male colleagues in the same grade. But in clerical work in private industry and commerce the sex differential remains substantial. Job for job, women clerks in 1956 averaged 20-25% less pay than men.

Age is, of course, an important factor in the earnings of white-collar workers. But it is less significant as a factor in earnings within any particular white-collar grade than it is for manual workers. Manual workers

commonly reach their final grade early, rise to a peak of earnings within it, and then earn less towards retiring age. For unskilled labourers the peak seems to be around age 30, and the drop in earnings from then to age 55-64 averages 15-20%. For skilled men the peak is often around age 40 and the drop much smaller, say 10-15%. A similar pattern is found in some of the independent professions. General practitioners' earnings before nationalisation, for example, showed a drop of 10-15% from the age-peak to age 55-64 in both urban and rural areas, though curiously enough not in "mixed" areas. American data suggest that the same is probably true of lawyers. But white-collar employees, once they have reached a given grade, do not normally drop back in earnings within it. Nor do they drop back into a lower grade, at least until they reach the end of their careers. At that point they may retire and be re-employed at a lower level. The proportion of male clerks in trade and industry who are in the two least-skilled grades of the Office Management Association's classification is only 2% at age 35-44, but 16% at 65 plus. White-collar employees usually, of course, take much longer than manual workers or independent professional men to reach their final grade. Male clerks, for example, seem most often to reach it about age 35-44.

THE TREND OF WHITE-COLLAR DIFFERENTIALS

White-collar differentials were squeezed sharply between 1938 and 1948, during and immediately after the Second World War. Before 1914 an average clerk earned as much as a skilled manual worker, and in some sectors, such as the banks, much more. Figures for Liverpool and London in 1929-30 show that this was still true at that time. But by the end of the Second World War clerks had slipped a long way, and the present rule that a skilled male clerk equates with a semi-skilled operative was well established by the 1950s. Supervisors' margins over those they supervise seem to have been cut in the Army—though comparisons here are tricky because of the changing pattern of allowances—and the Civil Service by about one-third between 1938 and the late 'forties. There are no similar comparative figures for industry. But it is known that by 1948-50 some 13% of industrial supervisors were earning less than the top-paid employees under them. The data on which this figure is based do not include certain occupations, such as face-work in coal-mining or some departments in iron and steel, where it is in any case a tradition that supervisors should earn no more than piece-workers, and often much less.

Top managers' ceilings have also come down, at least where figures are available. A Permanent Secretary in the Civil Service earned 17 times as much as an average manual worker in 1938, but only 9½ times in 1956. The army General's coefficient—he is assumed to be married—came down from 1,620 in 1938 to 790 in 1958. A number of services have also been transferred from the high-ceilinged private sector to the much

lower ceilings on the public side, producing some interesting problems on the way. Some typical cases, and the way they were solved, include:

(a) The owner of a small road transport fleet was making around £5,000 a year before nationalisation. He was bought out and compensated as owner, and afterwards re-engaged by British Road Services at £1,600 a year.

(b) The National Coal Board inherited a number of executives at rates up to and over £10,000 a year, whereas its own ceiling even for Board members (except the Chairman) was at first £5,000. Some were dismissed, if necessary with compensation, others were given personal rates up to £7,500; but these were then kept frozen while inflation pushed the rest of the salary scale up.

(c) The National Health Service initially paid consultants, who had been earning very high incomes in private practice, at rates that equated all of them to higher management and some to members of nationalised Boards. But these rates, too, were then blocked. From 1948 to 1955 consultants received only a 12½% increase, whereas operatives' earnings rose in the same period by 60%.

The squeeze on management salaries in general is illustrated by the following table:

	Increase 1938-53, %.
Male operatives' earnings	172
Typical salaries at—	
Middle management level	100
Higher management level	80
Consumer prices	140

Based on estimates by R. G. D. Allen, quoted in Cmd. 9613, 1955, p. 216.

Even before any allowance for direct tax, most middle and higher management salaries represented not merely less relative but even less real purchasing power in 1953 than in 1938.

There has been a tendency for white-collar occupations to slide down, so to speak, *en bloc*, maintaining their own internal differentials much more effectively than their differential compared to manual workers. Ratios between the pay of unmarried officers in the Army in 1958 were almost identical with those of 1938, and those between married officers had narrowed much less than the ratio between officers' pay and industrial operatives' earnings. Internal differentials in the Civil Service are also, since 1956, very close to those of 1938. The same tendency is noticeable in teaching. University lecturers' and assistant lecturers' rates, for example, bore much the same relation to professors' in 1957 as in 1921.

There has been at least one important offset to the squeeze on management salaries. This is the speeding up of promotion, thanks to the expand-

ing opportunities in some white-collar occupations. So, for instance, Under-Secretaries whose Civil Service career began just before the First World War took on the average twenty-seven years to reach that rank, whereas those who entered in the period before the Second World War reached it in only sixteen. There has been a similar speed-up of promotion in banks. For industry generally there are no comparative figures. But it can certainly be said that in recent years rapid promotion has been possible. A survey by G. H. Copeman shows that salary increases of around 20% a year over a period of years have recently been nothing out of the ordinary for managers who are on the way to the top.

But as against this there have been a number of factors aggravating the squeeze on white-collar margins. The sex differential has been closing in a way that affects particularly the standard of living of the male white-collar worker. Male operatives have retained most of their sex differential. Women operatives were earning 47-48% as much as men in British industry in 1935-38, and 52% in 1956-57: the gap had narrowed somewhat, but remained very wide. In white-collar jobs, on the other hand, women have gained a great deal of ground. Women clerks in private trade and industry seem to have been averaging 65-70% of men's earnings, grade for grade, in 1938, but 75-80% in 1956. One result is that women's earnings in clerical work to-day compare much more favourably with those of their opposite numbers in industry than do those of male clerks. And in teaching, the Civil Service, and other public services a pre-war differential of 15-25% is due by 1961 to be wiped out altogether. This squeeze on the sex differential has not so far been compensated by an expansion of family allowances, at any rate on the scale now common in other European countries.

The cost of living, also, moved against the higher-paid white-collar workers during and after the Second World War. From 1938 to 1949 consumer prices seem to have risen about 10-15% more for families with incomes in 1938 around £700 a year than for those with incomes around £200, which was then near the average for working-class families where the father was fully employed. The price of directly charged public services has also been manipulated so as to fall more heavily on the middle and higher management group. A family at the top of the "middle management" range of income (say £2,000 a year in 1953) would have paid about 3% income tax on the corresponding income in 1913-14, 14% in 1938-39 and 28% in 1953. The "corresponding" income is taken here to mean, not an income of the same purchasing power before direct tax, but the income earned by a manager at the same distance up or down the hierarchy of jobs.

In respect of fringe benefits, also, the balance has been tilted against the white-collar worker, though as yet by no means violently. One traditional advantage of white-collar workers which has diminished less than might be thought lies in their opportunity to place their children in good

jobs. It is no longer, it is true, as great an advantage as it was to be "spoken for" to an employer by some reputable wearer of a white collar. Banks recruit more on personal merit and less on the weight of recommendations. In a North Wales steel works the proportion of managers and staff "spoken for" was 54% for those entering before 1925, but only 30% in 1947-54; though even this latter figure is impressively high. Also the road to the top in education is more open. The correlation between high I.Q. and grammar school admission is far higher than before the Education Act of 1944. But the parent's white collar nevertheless still correlates rather closely with the child's prospect of a career. Children from high-earning families are more likely, whether through inheritance or home background, to develop high I.Q., and so to get the best educational opportunity. The highest 5% of earners, in one recent national sample, turned out to have 43% of the children with very high I.Q. When a boy or girl gets into a grammar school it is the white-collar parent who is likely to push his studies along.

*Grammar Schools, England and Wales: Intake of 1946:
Percentage of Pupils Remaining Till the Sixth Form*

Father's job.	Boys.	Girls.
Professional, managerial	47	41
Clerk	31	27
Manual:		
Skilled	23	18
Semi-skilled	13	7
Unskilled	7	6

Ministry of Education, *Early Leaving*, 1954, p. 38.

If there is a question of paying for education, the clerk, so the Ministry of Labour found in its cost-of-living inquiry in 1953-54, is likely, income for income, to be more willing to do so than the manual worker. And so the circle closes again. The demand for educational qualifications is rising—education is more and more the way to the top;¹ and it is the white-collar worker who is most likely to see that his children, ability for ability, get the education required. There is also some American evidence, which it would be interesting to check for Britain, that a white-collar background leads to more push and dynamism—individualism, if you like—in building up a career.

In other fields also white-collar workers retain important fringe advantages. They have more chance than manual workers of a pension or sick pay: both, that is, more chance of getting something, and a good chance, if they do get something, of getting more. They sometimes—usually in higher grades—have longer holidays. They usually, except in the very top jobs, where work pursues the Managing Director or Permanent Secre-

¹ See below, p. 65.

tary home, work much shorter hours. In recent years clerks have commonly worked about forty hours a week, more or less, whereas industrial workers have been averaging forty-seven to forty-nine.

But in recent years, though both white-collar workers and operatives have gained more fringe benefits, and the white-collar group have retained

Employers' Contributions to Fringe Benefits, Unweighted Average of Seven Industries, United Kingdom, 1955*

	Contribution as percentage of total cash remuneration.	
	Staff.	Operatives.
State insurance	2.1	2.7
Pensions	7.5	0.9
Accidents	0.0	0.3
Sick pay and "other social security" †	1.1	0.2
Holidays	7.3	6.2
	18.0	10.3

Ministry of Labour Gazette, August 1957.

* Iron and steel, shipbuilding, machine tools, cotton, shoes, coal-mining.

† Includes family allowances and similar payments, severance pay, etc. But the main payment in this class is sick pay, or contributions to sickness insurance. Payments for medical services, canteen, etc., cannot be subdivided between staff and operatives.

many advantages, it is operatives who have gained most. The contrast between office and works is less absolute than it was. The senior clerk may still get three weeks' paid holiday where the operative gets two. But twenty years ago the operative very often got none at all. The clerk is more likely to get a pension—apart from the State pension—when he retires. But the operative is catching up on him: by 1957 half the adult male work force was covered by industrial pension schemes, and about 2-2½% more of the total work force, largely manual, were being brought under similar schemes each year. The clerk still has a better chance of getting sick pay, and more of it; but the operative has an increasing chance of getting at least some. Higher management perquisites may also well have diminished in the last couple of generations: the chance, for example, to do business for oneself on the side. This is a sticky subject, on which data are scattered and subjective. But it would seem that on a long view these perquisites have at least not increased, either absolutely or relatively to other fringe benefits, and may have grown smaller; though there has certainly been some tendency, held in check more or less effectively by the Inland Revenue, for them to come up again since the Second World War.

WILL THE SQUEEZE GO ON?

By 1950 the squeeze on white-collar salaries was coming to an end. Since 1948-49, clerks' rates, have increased as fast as rates for operatives,

and not much slower than operatives' earnings. Trends in management salaries have been confused, but by the 1950s some firms at least were not merely stabilising differentials but widening them, though not usually very far. In 1953-55 the Royal Commission on the Civil Service was impressed enough with this trend to recommend that Civil Service differentials, too, should be widened, as was in fact done in 1956. But there has been no general drive to widen differentials back to their level of 1939. How comes it that the very sharp reduction of middle-class relative to working-class living standards during and just after the War—often, as has been shown, the absolute reduction as well—has so easily been rendered permanent? And is the squeeze likely presently to begin again?

In answering these questions, the central consideration is that the market for white-collar labour in Britain is precisely that—a market, ruled by the ordinary laws of a market. It is not a perfect market. There is, as always in labour markets, a certain looseness in it, a margin of indeterminacy within which rates have to be settled by executive decision. Comparisons tend to be closest in "clusters" such as that formed by the rates for a given grade of work within a single firm, and rather loose within the "contour" made up by rates for the same grade in the many firms of a given industry or region. Regional differentials within one firm, for instance, are as has been said usually confined within a range of 5-10%, but the spread between regional averages in an industry containing many firms may range up to 25-30%. But though industry-wide or regional salary contours may be loose, they do exist. The British Employers' Confederation has expressed scepticism about comparisons between jobs above the clerical level, unless within unified and standardised organisations, such as individual firms or the public services. Yet in fact such comparisons are continually made, and more and more closely as time goes on. The Office Management Association's biennial survey of clerical salaries reaches only as high as the supervisor or senior clerk. But since 1956 the Civil Service Pay Research Unit has set manfully though cautiously about assembling from firms and organisations outside the Service the data on duties and pay, even for technical and managerial posts, needed to apply the principle of "fair comparison" on which Civil Service salaries are based. Professional bodies, such as the Institute of Personnel Managers or the Royal Institute of Chemistry, have surveyed and compared their members' pay up and down the country. The British Institute of Management keeps a comparative record of top salaries. Definite career patterns and associated salary levels reveal themselves for managers of different speciality and who enter industry in different ways.

For a labour market to work effectively, it is not necessary to compare every job in it directly with every other. Comparison usually focuses on a limited number of key jobs, round which others cluster. To-day enough key white-collar jobs are being compared to ensure that there does exist,

over a wide range of higher as well as lower-paid occupations, a true market rate for the job. And this rate is based—subject always to the margin of indeterminacy—on the one hand on the supply of people able and willing to do work of a given grade at a given price, and on the other, on the readiness of employers to buy at a given price the services of given numbers of these applicants. Supply and demand do not work only through labour mobility. Dumb disaffection, as the Royal Commission on the Civil Service pointed out, can also be a very compelling force. But whatever the way in which they take effect, it is in the end supply and demand that fix the main framework of white-collar rates.

Will, then, the conditions of supply and demand shift in future so as to cut the white-collar worker's relative pay still further or perhaps to raise it? And how comes it that the readiness of white-collar workers to supply their services for a lower relative and, often, absolute return has increased so sharply since 1938? Will it increase still farther? The main considerations would seem to be as follows:

First, there is what I shall call the Wootton effect. In *Social Foundations of Wages Policy*, Barbara Wootton argues that there is a reserve price on white-collar labour. Let the number of individuals willing and able to teach history in universities for a pittance be as great as you will. If it be socially accepted that a lecturer should be paid, at his minimum, not less than 40-50% above an industrial worker—roughly his present margin—then the university teachers and their white-collar allies will see to it directly or indirectly, by acting on demand or on training facilities, that the number of people actually taking up history lectureships is limited to what those who finance the universities are willing to pay for at that rate. Exactly this policy of setting a reserve price and adjusting supply to it is, of course, also traditional in skilled manual crafts. But Mrs. Wootton does not merely point out (as it was hardly necessary for her to do) that reserve prices exist. She also argues that these prices can be beaten down, so that the price at which labour of a given quality is forthcoming is reduced. And just this has, in fact, happened since 1938.

Initially, it happened because during and just after the War people, including those in white collars, agreed that current consumption must be cut in the interests of war and recovery, and that the cuts should fall chiefly on those with most to spare. But why were the initial cuts accepted so much more easily by low-paid clerks than by similarly paid manual workers? And why was the reserve price of higher as well as lower-qualified white-collar labour not restored to its old figure after 1948 or 1949? There would seem to be four main answers:

- (1) The cut in white-collar incomes has been felt less keenly than might be thought, because for many families it has affected the standard of living much less than cash purchasing power. One reason for this

is the domestic revolution of the last generation: the spread of mechanisation and rationalisation in the home. The other is the growth of certain fringe benefits, such as the National Health Service or the improved educational system after 1944, which have helped white-collar as well as manual workers' families.

(2) The contour of comparison for white-collar rates of pay seems till recently to have included other white-collar jobs, but not—or rather not so directly—manual jobs. It has often been possible to maintain differentials between white-collar jobs even when those between white-collar and manual jobs were closing. The closing of this latter differential, or even its reversal in the case of clerks, has thus caused less alarm than might have been thought. It is also made easier to explain. The contour of comparison for manual jobs includes many jobs, for instance, in engineering, where, thanks to devices such as payment by results or merit rating, earnings rise rather easily even when the advance of wage-rates is restrained. The wage-drift in these occupations levers up, in the end, pay in other more static jobs. But the white-collar contour contains rather few jobs where a wage-drift of this kind is so easy. The upward pressure on manual rates could therefore have been felt as strongly on the white-collar side only if the white-collar contour had been extended to include manual jobs; and, till recently, it was not.

(3) Thanks to the late peak of earnings in a white-collar career, substantial numbers of younger white-collar workers have seen their personal standard of living maintained or improved over many years through promotion increments, even at a time when the income associated with any one white-collar grade was falling, relatively or absolutely; and of course, even though the promotion increments themselves have added less to their recipients' relative or absolute income than they would have done had the general fall not taken place. By the time this younger generation's personal income curves had flattened out, they had become accustomed to the new and lower level of payment for each grade.

(4) The war and post-war emergency was prolonged enough for many even of those who did not benefit from promotion increments to get used to a lower standard of living, relative or absolute.

Will this Woottonian *tour de force* repeat itself, and the relative or absolute standard expected by the white-collar workers be reduced still further? I doubt it, if events are left to themselves. It might be made to repeat itself, as Mrs. Wootton herself proposes in *Social Foundations*, but even that will not be done without a hard struggle. For, first, the four reasons just enumerated did not *cause* the initial squeeze on white-collar margins: they merely assisted it and prevented it from being reversed. The cause was an

acute national emergency, in the face of which people were prepared to curtail their consumption, voluntarily as well as statutorily, or to forgo increases of income, in a way not normally to be expected in face of the less-compelling problems of (if this is the right word) peace. And secondly, at least two of the four reasons are in any case likely to act much less powerfully in favour of a squeeze on white-collar margins in future than in the past. The contour of comparison in salary negotiations *does* to-day more and more often include manual wage-rates and earnings. And, though the domestic revolution still has plenty of scope for development—as anyone who knows the slums and ancient monuments of the English middle-class home will agree—I doubt if its implications as between this generation and the next will be as revolutionary as they have been between the last generation and the present. That, at any rate, is the impression I get from studying the shape of things to come in American kitchens and stores.

We may then expect white-collar employees to fight harder than in the 'forties at least to maintain the margins they have now. And in this fight they are likely to be helped by the rising demand for at any rate the higher-qualified among them. In the 1870s there were five manual workers in Britain for every one in the white-collar group; to-day there are only two. And whereas a century ago two-thirds of the white-collar workers belonged to the "old" or independent middle class of farmers, small employers or professional men, to-day upwards of three-quarters are employees. The ratio of staff to operatives in British industry was 8.6% in 1907 and 20.0% in 1948.

This expansion of the white-collar occupations has consisted largely of a wave of clerks, and notably of women clerks and typists, entailing among other things a revolution in the meaning of women's work. In 1881 there were seven women factory workers, domestic servants or other operatives for every woman in a white-collar job. To-day the ratio is about evens: 1 : 1. It remains to be seen whether this vast growth in the demand for clerks will continue. Mechanisation and automation have already made a big dent in the demand for routine clerical work. But about the demand for higher-qualified managers or technologists there is no such doubt. It is rising, and will go on doing so. Modern process firms typically employ, size for size, twice or three times as many managerial and supervisory staff as firms engaged in small batch or unique-article production. This increasing number of managers is increasingly expected to have high qualifications. Among managers in a recent sample of large British private firms:

Of those reaching the age of 21 in:					The percentage of graduates was:
1916-20	.	.	.	.	10
1940-45	.	.	.	.	34

The Committee on Scientific Man-power suggested in 1956 that, whereas in a progressive modern economy the total man-power employed of course increases less rapidly than output—it is in this that progress consists—the ratio between the increase in output and the increase in the employment of scientists and engineers may well be 1 : 1.

But, if demand is rising—at least for the more highly-qualified—so also is supply. It has long been predicted, and has finally come to pass, that the spread of education would increase the supply of clerks, till their rates could (not necessarily would—that depends on the reserve price that clerks enforce) fall below those of manual workers. But the higher-qualified groups are affected as well. The universities are expected by the 1960s to be graduating three times as many students as in 1938. A recent estimate suggests, on the basis of current trends, that whereas in 1956 five students left school at fifteen for every one who stayed till seventeen or eighteen, by 1965 this ratio may have fallen to 3 : 1. Ultimately, of course, the expansion of supply must come up against the limit set by the quantity of inherited ability available in the country and hitherto unused. It is not at all clear where this limit lies, but two things can be said about it. First, it may not be far off so far as first-class minds are concerned. The number of university students in Britain increased by 60% from 1938 to the middle 'fifties, but the number of first-class degrees by only 25%. But secondly, for less-high qualifications the limit of supply is as yet only dimly in sight. Preventable wastage in the schools is still high. Of those boys and girls in the grammar school intake of 1946—the intended sixth-form leavers of 1952–53—whom their schools thought suitable for Advanced work, about one-third (boys) or two-fifths (girls) were withdrawn from school before reaching that stage. The Committee on Scientific Man-power commented that: "We do not doubt that enough talented boys and girls will be available . . . to be trained at the rates which we recommend should be the basis of national policy." And there is a large unexploited reservoir of those who missed their educational chance before 1944, and indeed after that date, but could still be trained. Of the 25–30% of the children aged 9–12½ in England and Wales in 1932 whose intelligence was highest, three-fifths had no chance of full-time education beyond the elementary level.

We are dealing here with global figures, and leaving out of account such particular surpluses and bottlenecks, genuine or alleged, as may appear from time to time: as for instance, at the time of writing, a genuine shortage of engineers and an alleged tendency to graduate too many doctors. But, taking the global figures as they stand, their meaning may be summed up as follows:

- (1) The white-collar groups are likely to fight much harder for their differentials in future—failing a war of other catastrophe—than

they did in 1938–48. A new "Wootton effect" *might* be produced, but only with great difficulty.

(2) The chance of clerks rising above their present lowly relative status seems dim. The trend of supply—the ever-widening spread of education—is against them, and so may be, thanks to automation, the trend of demand. They can probably not hope for much more than to set on their labour a reserve price which will help them from slipping back farther in relation to manual workers.

(3) For higher-qualified white-collar occupations no general conclusion seems possible. Supply and demand are rising together, but not in a clearly predictable way. The one reasonably clear point is that first-class ability is likely to be, or rather remain, in short supply; very possibly even more so in future than in the past. This presumably, if it does not actually force pay ceilings up, will help to prevent them from falling further. Whatever the less highly qualified manager's or scientist's position on his own merits, he should at least be free from the fear of being crushed, as has happened in the last twenty years, beneath the descending ceilings of his betters.

Lastly, a moral footnote. I have been talking of what has happened or might happen. But what about what *ought* to happen, or be made to happen, even at the cost of a struggle? I see no reason to object to the tendency for rising demand to push up differentials or for falling supply to lower them. If a given class of work becomes worth more or less, by all means let its relative pay vary accordingly. But what about the system of reserve pricing? Few would object if reserve pricing were simply an attempt to stabilise rates, to keep them from fluctuating around the trend set up by supply and demand. Nor is there any objection to the principle of the "red-circle rate," the guarantee to people already in a job that their personal rate will not be cut even if the rate for the job is lowered. But in fact reserve pricing goes well beyond this. It has commonly amounted, as Mrs. Wootton has correctly shown, to the creation of a true monopoly. The number of good jobs has been restricted so that it has been worth while for employers to pay far more than would have been necessary if each man's and woman's ability had been fully developed through education, and if each had been free to sell his abilities where he wished for what they would fetch. Ought we not to rise up in fury against this state of affairs and demand that differentials be squeezed and barriers cracked until justice prevails?

I do not think we need to make such a great fuss about it. For, first, we are already since 1944 doing, imperfectly but with gathering success, the first and most important thing required to break the monopoly of good jobs. We have made a start towards throwing open the schools and the Universities—subject to the current axe that is slaughtering eligible matricu-

lands by the hundred: I write as a Dean whose hands are red with innocent blood—to all who have the necessary ability. And, secondly, the rising tide of demand for high abilities and high qualifications is already overtaking us. If the Education Act of 1944 and the various measures that are gradually following it had been in operation thirty years earlier, differentials to-day would certainly be lower than they are. But they were not. By the time, starting from now, that education has spread to its limits, or can reasonably be expected to do so, demand may well have risen so far that the whole of the newly educated can be absorbed at something like the present differentials. The spread of differentials may be rather wider or rather narrower than now; just how they will come out is a matter for the astrologer, rather than the economist. But their present level could well be rather close to their ultimate and, by then, proper level.

We might, by speeding up educational development and backing it with an attack on professional exclusiveness, cause a dip in differentials in the meantime. But I doubt whether it would be wise: educational advance cannot usefully be speeded beyond a certain limit. And I am certain that it would not be worth the trouble. I do not deny that there may be good grounds for a special intervention in this profession or in that. But, in general, let us get on with the job of increasing supply and demand together, and leave differentials to look after themselves. On that basis, our consciences can rest in peace.

Cardiff.

MICHAEL P. FOGARTY

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THE AMBIGUOUS NOTION OF EFFICIENCY¹

THE measurement of efficiency is one of the frequent tasks of economists. The word "efficiency" is an everyday one, and we are most of us convinced that we know what it means. Often we do, especially when faced with a practical problem. But such problems, involving ideas of efficiency, are so many and so intimately related to each other, that it is easy to slip from one sense of the word to another, and so to confuse issues in the discussion. The purpose of this paper is to point out some of the contrasts between concepts; to show in what way different types of problem lead to different types of analysis and calculation, and in particular to introduce explicitly the problem of the "merging of scales (or ranks)," which is of great importance in this field. Since the range of problems is so large we cannot claim to do more than pick out a few important points of reference, which will exemplify the main points to be considered. Finally, we will show how some problems in the assessment of economic forecasting give rise to a similar range of ideas.

We must suppose that we are to make a comparison of efficiency between two or more units. The comparison will be based in some way on the performance of these units. These units may be firms, or individual plants or workshops, or industries or whole economies, where it is appropriate to consider these as productive units. Economists may find themselves discussing any of these.

As an example, to introduce the main ideas, suppose we are comparing mines, each with a similar labour force producing the same sort of coal with the same type and quantities of equipment. Then at the simplest level of efficiency analysis we can suppose that we are concerned with deciding where to produce coal, at mine A or at mine B. The answer would be, other things being equal, at the mine producing more coal. No allowance need be made in such a straightforward decision for the different difficulties with which those working the different mines have to cope, such as different depth of coal, or seam thickness, and so on.

If, on the other hand, we were trying to judge, not whether or not to use a mine, but how well its management was doing its job, we might use very different criteria, and the result of the comparison might be very different. At one mine conditions may be simple, at another very tricky. We may feel, that though one manager achieves less output than another, both cope with their different problems equally well. Thus there are two different questions of efficiency, which though close, and both partly decided on the basis of the output of the mines, may have different answers with different practical implications.

¹ We wish to express our thanks to the Nuffield Foundation. Part of this work was done in a project which they sponsored.

The first type of problem is related to the determination of what is sometimes called *social efficiency*. One aspect of the idea behind this term is that the resources of a society should be used to produce as "large" a bundle of output as possible. In the case of the mines the same amount of coal from either of the mines would equally satisfy the needs of society; so that if there were a decision to be made on the basis of which mine makes better use of the community's resources, *ceteris paribus*, the mine which produces more coal would be chosen. (The economist, in doing such calculations, is implicitly discussing the possibility of such a decision.) But equally a firm or other organisation may be interested in just such a decision, so a more general name seems desirable. We will call this type of efficiency question one of "efficiency in present use" or "efficiency in use" to emphasise that it is the productive unit itself that is being judged on its current output. On the other hand, comparisons of performance based on the very different criteria appropriate to judging management performance we will call comparisons of *managerial efficiency*. The broad distinction between the two types is that managerial efficiency judges the management, but efficiency in use judges the plant.

These two concepts of efficiency already illustrate some important characteristics of such problems. First, they are both concerned implicitly with decisions, or rather with providing material to assist in decisions. (Which plant to use? Which manager is better—with the choice implied by such a statement?) Secondly, just because they are concerned with decisions they are concerned with ranking the alternatives or putting them on a scale. Moreover, the rankings achieved may be different for the two cases. We will return to this question of rankings later. But the single classification of managerial efficiency itself covers a wide range of questions, each of which can give rise to a different ranking. For each firm, in order to decide the difficulty of the task, it is necessary to specify the range of opportunity open to the manager. These ranges may vary from firm to firm, and thus make a considerable difference to the situation with which they have to cope. For example, supposing one firm is operating at an unfavourable site; does the analysis take this site as given, and make allowance for poor performance because of its inconvenience, or does it allow the site itself to be a variable at the choice of the management, so that no allowance need be made? Clearly such questions depend partly on whether the analysis is of performance over the long or the short run. In the short run the analysis might take the site as given, in the long run as supposed within their field of choice. We may define those factors which, in a particular analysis, are taken as not within the management's field of choice as *environmental*. Each different environment implies a different range of choice.

The decision as to what is environment does not necessarily represent the choice as it is seen by the management of the firm. A management may be inefficient just because it does not see its full range of choices, or because its

attitudes preclude it from taking advantage of them. For example, a management may always have a short time horizon, and thus continually do things which, though right for the short run, are wrong in the long run. Such possibilities show that in any analysis of this sort it is up to the investigator to give in detail what factors are being taken as environmental.

With efficiency in use, problems of environment do not arise. But there is a further important difference between the measurement of efficiency in use and that of the various types of managerial efficiency, that we have just described. These can be summed up by saying that efficiency in use is a "measured efficiency" while managerial efficiency is usually only ordinal. The distinction can be made clearer by elaborating the example of the mines. Suppose there are three types of mines, mines of type A produce 4 units of output per week, mines of type B produce 2 units, and mines of type C produce 1 unit, per week. All the inputs of the mines are either similar, or valued similarly. Then we could define the efficiency in use of mines of type A as 1, those of type B as $\frac{1}{2}$ and those of type C as $\frac{1}{4}$. We could also say that the relative efficiency (in use) of C compared with B was $\frac{1}{2}$, and was just the same as the relative efficiency of B compared with A. Moreover, these relative efficiencies obey the relation that

$$\text{Efficiency of C relative to A} = \frac{\text{Efficiency of C relative to B} \times \text{Efficiency of B relative to A}}{\text{Efficiency of B relative to A}}$$

These relations are just those of a cardinal scale. (Indeed, if we worked in terms of the logarithms of efficiencies, the multiplication would be replaced by an addition, and the efficiencies would be points on a scale. A relative efficiency would be the distance from one point to another.) Moreover, such relative efficiencies and the relations between them are useful in the type of calculations which are actually made with efficiencies in use. Even if we did not know the actual outputs, we could say, for example, on the basis of the relative efficiencies alone that it would be just as sensible to use three mines of type B as one of type A and two of type C.

On the other hand, managerial efficiency is usually given only by a ranking. This is so just because it is measured with the hope that it will be useful in a wider range of circumstances, and in conditions which present the management with problems of varying difficulty. If environments were all the same, then it is true that we could measure the relative efficiencies of different managers. But even then we would usually be interested in the question of how they would fare in different circumstances. And even if one manager did produce twice as much as the other in one set of circumstances, he is unlikely to produce exactly twice as much in other circumstances. He may then only produce one and a half times as much. It would then be futile to say he was twice as efficient. Thus the most we can hope to do as a rule is to rank the managers. The point becomes even more clear cut if the figures from which the managerial efficiencies were

to be obtained were derived from performances in tasks of different degrees of difficulty.

However, if the managers are to be ranked, this may be done by assigning to them a number, just as a mark can be given to an examination candidate. The number would be an indication of the rank only, and no meaning could be attached to the ratio or difference of such numbers.

The argument of the last section will be expanded later in the discussion of the merging of ranks and scales. However, there is another class of efficiency concepts, in some respects lying between the two we have just described, and in fact overlapping such concepts in extreme cases. We may call such efficiencies *target* or *technical* efficiencies. A target efficiency is concerned with a situation where a number of different units (firms say) are operating in a number of different environments. We are interested in how much a particular firm has to increase its output to reach the best in its particular environment. This is a measured efficiency, on a scale, and not merely a ranking. We are interested in how far the firm has to go to reach the best, and not, as in the more general case of managerial efficiency, how difficult it is to get there. Target efficiencies answer the type of question: if only we could change the management of this firm for the best of its kind, how much output could we obtain? The calculation of technical efficiency, on the other hand, is concerned to answer such questions as "if only we replaced this technique by the one producing most in its particular environment how much more could we obtain." Again the question of how difficult such a task is to achieve is not raised.

But we still have to answer the question: What is the meaning of environment in these two cases? In each case a particular environment defines, as it did in the case of managerial efficiency, a class of choices. In the case of target efficiency variables which are considered by the investigator as being under the control of the manager are taken. This procedure provides ranges of comparison, firms being comparable with each other if they could choose themselves to be in each other's position in all but unimportant respects (*i.e.*, respects which do not change their output). Then the factors which differ between these classes of comparison, and do influence the output, define the changes in environment.

We still have to define the best output in a given environment. We will return to a discussion of this point later, but, in the meantime, will point out that like the environment it depends on the range of comparisons thought suitable by the investigator. An example is given by a topic on which one of us (M. H.) has worked; it is known that the percentages of working-class people in an area affects the success of Co-operatives in that area. Some Co-operative managers will therefore consider it fair only to compare themselves with other regions with a similar class composition. But an investigator might feel that, even in middle-class areas, the Co-operatives should capture the trade. He would then set higher standards in the middle-class

areas than the Co-operative managers themselves might admit. Again such a point emphasises that, in any study, the standards and classes of comparison must be clearly defined and set out by the investigator.

In the case of technical efficiency the logic may be just a little different, though this type of efficiency clearly overlaps with target efficiency. Technique can have a rather general meaning in economies. For example, a chain of shops may adopt the technique of siting their shops in the main streets of towns, or on the edges of towns. If such siting is accepted as a part of technique, then site cannot be an environmental variable. The lines between different classes of comparison are drawn by those changes which do not count as changes of technique, but which do influence output. Thus it is important for the investigation in this field to mark out exactly what changes he counts as changes of technique.

It should be noted that target efficiency reduces to efficiency in use if there is no difference in environment. Even if there are such differences, then the concept is useful as a generalisation of one aspect of social efficiency. It can be used to answer the question "What output could be produced if each firm were as good as the best in its own particular environment given that no new firms were to be started, or old ones closed down?"

Yet a further concept of efficiency is that held by equity shareholders of firms, namely return on equity capital. But comparisons of returns present all the same problems that we discuss below. However, this concept has gained a wider currency than is really justified. The quantities which economists consider are normally measured in money, and it has been widely held that the most efficient use of resources is the most profitable, *i.e.*, that which gives the largest output (sales-return) over input (cost). But people have hoped to find a criterion of efficiency which can be used generally over the whole economy in a money measure, *viz.*, profit. But this measure is inherently an arbitrary one until a valuation is put upon the current distribution of income (which affects sales-return) and on the current factor prices. Given that, it would follow that differences in efficiency in this sense (rate of profit on equity capital) at any one time are the outcome of either disequilibrium positions which will be removed by competitive forces; or gains arising from some specialised resource, technique or environmental features (rents or monopoly or monopsony profits); or are attributable to the special qualities of the particular entrepreneurs. Alternatively, historical comparisons are made which are either historical rather than economic (this industry is more up to date than that because innovation is faster); or comparisons of profitability which can be reduced to the three cases mentioned above. It should be noted, in any case, that profits themselves have an element of arbitrariness in them, depending as they do on book-keeping conventions. We shall not discuss this matter further in the present paper.

In this sketch we have introduced some of the most important questions that have to be asked in the discussion of efficiency:

- (1) Is one using the analysis to judge the performance of the economic units involved or their managers or techniques used in them?
- (2) What, if any, allowance should be made for the different degrees of difficulty with which they are faced?
- (3) What range of choice should be imputed to the units considered? If it is to be different for different units, what allowance should be made for such differences? In particular, what maximum standards (if any) should be set within each range of choice?
- (4) Is the number obtained a ranking or on a scale of measurement?

To these we must add two more questions:

- (5) What is to be considered the output of the process? If there are two or more outputs, how, if at all, should they be combined?
- (6) The same question as (5) can be asked about inputs.

In most cases, it is possible to give a wide range of answers to each of these questions. Each set of answers corresponds to a different analysis.

THE MERGING OF SCALES

This is a technique which is essential if the results of firms with different environmental conditions, or different choices, are to be compared. To obtain a measure of managerial efficiency involves the amalgamation of fragmentary orderings into one consolidated ordering. Such a process is often used implicitly, but, because it can be carried out in different ways, with different results, it is necessary to spell it out explicitly, so that the assumptions underlying it can be thought out afresh in their application to different problems.

The situation before the merging is that we have a number of fragments of ordering. For example, we know that

$$A > B > C$$

$$a > b$$

and that

but these two fragmentary orderings are not yet related to each other.

A merging ranks all the units considered (firms, plants, etc.) on one scale in such a way that their ordering in the fragmentary scale is not lost. For example, two possible mergings of the fragments given above are

$$A > a > B > C > b$$

$$A > B > C > a > b$$

and

We can allow equality in the above definition, so that

$$A > B = C > D$$

$$a > b$$

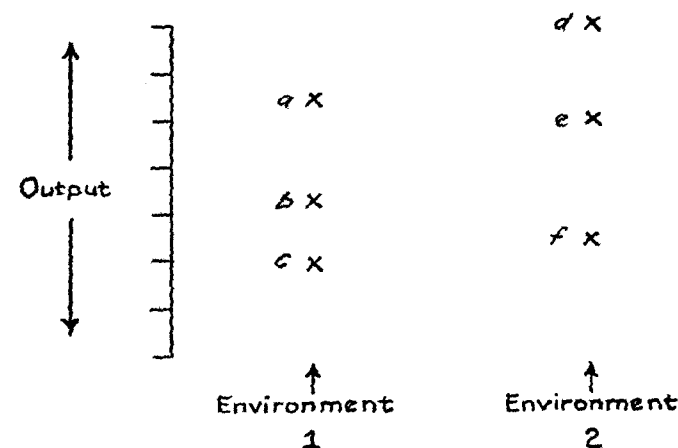
and

might be merged as

$$A = a > b = B = C > D$$

for example.

A case where different environments lead to fragmentary scales might be provided by the idealised mines we have been discussing. Suppose there are just two sorts of mines: those with thick seams and those with narrow seams (these provide the two different environmental circumstances). Suppose, as before, that inputs are the same, and that we have measurements of outputs from several different mines in each environment. The outputs might be plotted like this:



In each environment the different outputs give a fragmentary scale. We know that

$$a > b > c$$

and that

$$d > e > f$$

but want to consider the implications of combining a, b, c and d, e, f on one scale, and possibly attaching a measurement to them.

We must first make clear that, in the case of target efficiencies, there is no real merging. The performance of any firm (or productive unit), is compared only with the actual or possible performances in the same environment as itself. The comparisons provide separate scales for each environment. However, when the target efficiencies have been calculated, they give the appearance of a merging. One is tempted, for example, to suppose that all firms with 80% target efficiency are ranked as equal. But this would not be accurate. The firms may be in different environments, and the scales would only be merged if the type of efficiency question asked was changed. For example, if the question raised were changed to one of efficiency in use, then on our chart, the scales would be merged to give:

$$d > a > e > b > f > c$$

The same sort of merging would be obtained if we judged that what we had previously considered as environment was really under the control of the manager, in a measurement of managerial efficiency. For example, if environment (1) represented "bad labour relations" and environment (2) represented "good labour relations" we might, in measuring managerial efficiency, use the type of merging given above, if we thought that the labour relations in a firm "ought" to be under the control of the manager. However, the possibilities are more general than that.

MAKING ALLOWANCE FOR DIFFICULTY

When we consider the idea of managerial efficiency we must bring in the idea of degree of difficulty of the task. The concept of managerial efficiency allows of much elaboration, but we can start from the same diagram as that given above. We must remember that, in this case, the managers are considered unable to "change" the environmental variables. What suitable criterion can be used for merging?

One such, in principle, might be that of virtual interchange of managers. To be useful for efficiency analysis it would be convenient to assume that the order of scoring of any group of managers would not be changed if they moved from environment 1 to environment 2. For example, if a manager in environment 1 obtained an output O_1 and was transferred to environment 2, and there (after sufficient time to acclimatise) obtained output O_2 , then O_1 and O_2 might be ranked equal on the scale. The assumptions are implicitly made that all managers who obtained score O_1 in environment 1 would score O_2 in environment 2, and also that managers who originally scored O_2 in environment 2 would score O_1 when transferred to environment 1. These are the simplest assumptions for a suitable analysis, though it would be possible to relax them and still get, for example, average or probabilistic results.

Such a method is not useful in practice, since the interchange experiment can hardly ever be done. However, it provides a useful conceptual comparison with other methods of merging in common use, including those sketched above.

In Fig. 1 we might assume that the set (or population) of managers in environment 1 was much the same as the set in environment 2. This would lead us, as a first attempt, if we were comparing the same number of individuals in each environment, to equate the first in environment 1 with the first in environment 2, the second with the second and so on. Or as a more sophisticated method of analysis, to fit distributions of output of some plausible form to the two sets of outputs, and equate percentiles. This method would give similar, but not necessarily identical, results to the previous one, and can be applied to cases where the numbers are not equal.

A similar type of method is, we believe, used in "normalising" the marks of schoolchildren in different years. The assumption is that the

capabilities of children taking examinations do not vary from year to year, but that the difficulty of the examination (the "environment" the child faces) does change. In the smaller-scale examinations (such as University degree examinations) the examiners are more confident that they can see how a candidate would do in different circumstances (a different year's examination or environment), and are less confident that two given years' groups of candidates will be identical. Therefore, they vary, for example, the proportion of candidates given different classes of degrees, and if we accept a class as a stable entity from year to year, then these classes provide a method of merging the scales for different years.

Occasionally it may be possible to find some other criteria for merging scales. For example, we might judge the production managers by some particular aspect of their work (or even personality) and then equate those who seem the same by this partial criterion in the different environments. It does not seem necessary to emphasise how misleading this method can be, and how widespread is its use.

On the other hand, we may feel, on the basis of considerations other than the immediate data, that an output P in the first environment is just as easy to obtain as an output kP in the second environment, where k is a constant independent of P . There would still remain the problem of finding k . This, too, may be known from considerations apart from the data. If it is not, then it may still be known that some particular characteristic of the set of results in one environment is the same as a characteristic in the other. If, for example, the best actually achieved in the two environments were achieved by managements of equal skill, then a value of k could be obtained. In this case, the resulting efficiencies would, with suitable numbering of the ranks, look very much like target efficiencies, though the underlying logic used would be very different.

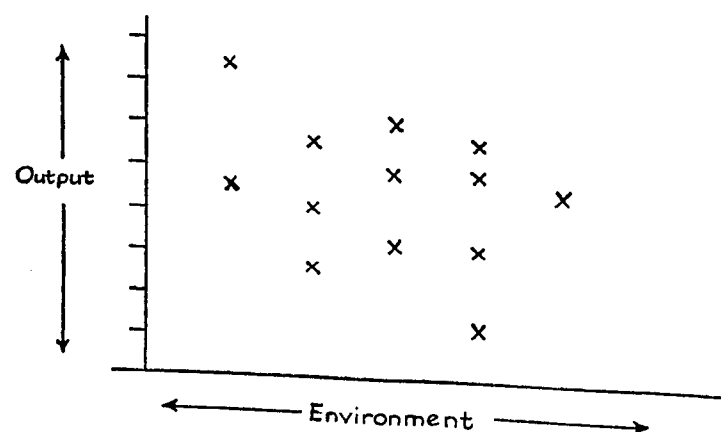
A different kind of merging occurs in the case of technical efficiencies when it is decided that factors which had been considered environmental are to be included in the category of technique. Then it is likely that the values of the efficiencies of some firms would be altered. For example, if the output of firm A was P_A and that of firm B was P_B , and they were considered as being in the same environment, in which the highest output was P , their efficiencies would be $\frac{P_A}{P}$ and $\frac{P_B}{P}$. Now if there were a new definition of environment, less detailed than the old, then the highest output would be raised to P' , and the efficiencies would be altered too. However, the relative efficiency $\frac{P_A}{P_B}$ would not be altered. Thus in the merging into a more extensive scale, the actual values of the efficiencies are changed, but the relative efficiencies are not. Such a property makes it useful to work in terms of relative efficiencies as much as possible. The absolute maximum may occasionally be very difficult to determine, or even rather arbitrary. If

it is possible to avoid bringing it into the argument so much the better. Note that the argument works the other way round too. If we transfer factors from the technique to the environment, thus breaking the comparisons into smaller sets, the relative efficiency of *A* compared with *B* is unaltered as long as *A* is in the same environment as *B*.

QUANTIFIABLE ENVIRONMENT

It may happen that, for the purpose of a particular problem, there are not just two environments from which the results have to be merged, but many. In that case, it may be possible to characterise the environment of each unit by the value of one or more variables. For example, average seam thickness may be taken as an environmental variable for some problems concerned with mines. The population density of an area and the income at the disposal of the population might be considered as two relevant environmental variables in comparing the efficiency of retail distributive systems in different areas. Climatic variables might be considered as environmental for those problems in farm efficiency for which farmers cannot choose where they are to work. Sometimes the environmental variables may be of a different kind. In comparing different regions or economies when there cannot be migration of work-people from one to another, the total labour force may be a datum of the problem, and would therefore be environmental in that case, even though it was one of the "inputs" of the economy. Or electric power may be limited in the short run by limitations of capacity, so that for a short-run problem generating capacity may be environmental.

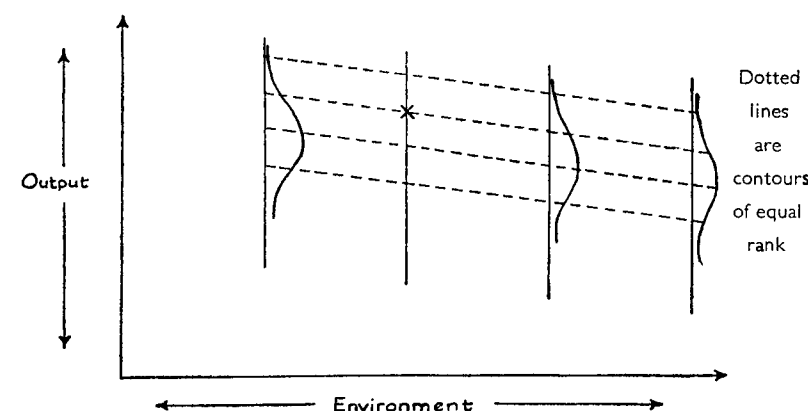
It is convenient to take first the case in which the environment can be quantified in a single variable. In that case the data can be plotted in a scatter diagram, which might look like this:



The next step one is tempted to take is to fit a regression line. But the argument of the preceding sections shows that one must hesitate before

doing such a thing. What is the type of efficiency one is trying to measure? We may here really be trying to merge several fragments of scales, one fragment for each value of the environment variable that occurs in our set of results, into a single scale. The problems of such merging remain. Or we may be trying to find the maximum output at each value of the environmental variable, so as to find target or technical efficiencies.

However, in this case there is one further technique which can be used—that of interpolation, and in applying this the problems are closely parallel to those of regression analysis. The idea is to apply any trends in scales discernible from the data (or expected *a priori* by the investigator) to help fix the scale at any particular environmental level. To take one extreme example where the problem is one of merging scales: suppose that there are many readings at some levels of the environmental data, but only one result at one intermediate level. Suppose, too, that it had been decided that where there were enough data, the percentiles of the distribution at each level provide the merging (as they did in the example of examination of schoolchildren given above). Then we might get a situation like this:

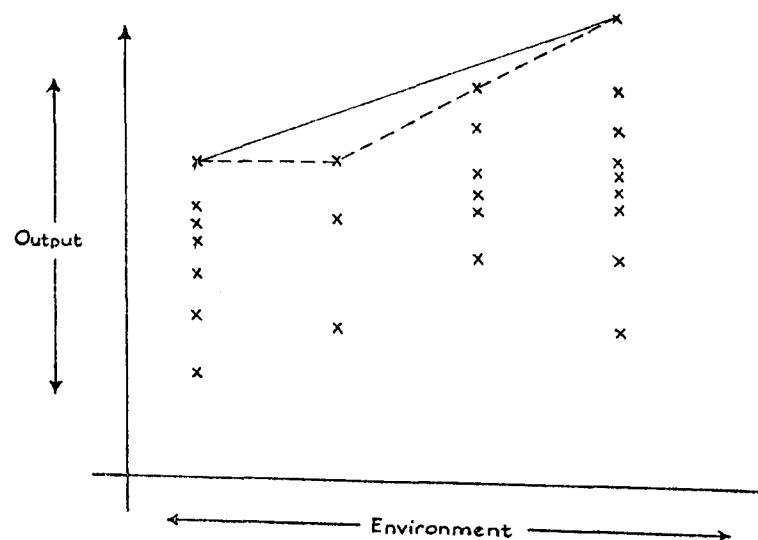


and the solitary observation would be merged with the others by the drawing of smooth lines through the percentiles.

Alternatively, the problem may be of a kind where it is necessary to estimate the maximum attainable in each environment. Suppose the observations looked like the diagram on p. 82.

One way which might be considered suitable for certain target efficiency problems, is to draw a broken line consisting of a series of straight segments, joining the "top" points of the diagram, as the dotted line has been drawn in the figure. The top reading at environment 2 would then be counted as inefficient. But in some cases it may be known that there are indivisibilities in the problem. For example, suppose the environmental variable was electric generating capacity. Suppose that it was known, apart from the

numerical data, that it was not possible to use a certain technique until there were a certain minimum supply of electricity, but that when such a minimum was available the new technique was very productive. Then this would be a case for using a rather step-like line of maxima, as with the unbroken line in the figure above. If this line is used as the 100% line, then the top point at environment 2 turns out to be 100% efficient. Such difficulties which occur even apart from the difficulties of sampling fluctuations, show that the method of interpolation used must often depend on extraneous information,



or even the investigator's own hunches. In general, the more restricted hypothesis, such as the fitting of a single straight line across the data, means that the analyst is willing to have readings at low environmental levels to influence the scale at high ones, so that the scales are fitted with the help of many readings, but depend on how near an approximation to truth is the rigid assumption of a straight line. The less-restricted hypothesis, such as the set of straight segments, means that observations for one extreme of the environmental variable are not likely to affect the scales at the other extreme, but then each scale is determined by effectively fewer observations. Thus extraneous data, as in regression analysis, do help to improve the accuracy of the merging or of the fixing of maximum attainable levels, if they help to limit the type of mergings or maxima considered feasible *a priori*.

The case for which the environment has to be quantified into not one but several variables, presents essentially the same range of problems at a slightly more complicated level. But the case shows more clearly the distinction between an *explanatory* analysis and *efficiency* measurement. In

an explanatory analysis (of the type for which multiple regression is used), it is important in principle to include all the variables which contribute to the variation in the output variable. Otherwise, a variable which is omitted may give a spurious extra effect (or spuriously take away from the effect) of another variable. But in efficiency analysis only those variables considered as environmental for the particular problem in hand are included in the analysis. Thus there might be a variable which the management of firms did not attempt to control, but which the analyst thought that they could. It might, for example, take much time to control the variable, and the firms' time horizons may be too short for them to consider such an undertaking worth while. But the analyst may judge them from a long-term point of view, and may consider the variable as controllable. Then the variable would contribute to an *explanation* of the variations of output, but would not be plotted as environmental in the efficiency analysis. Of course it may, like other extraneous material, be helpful in indirect ways in deciding on a method of merging scales.

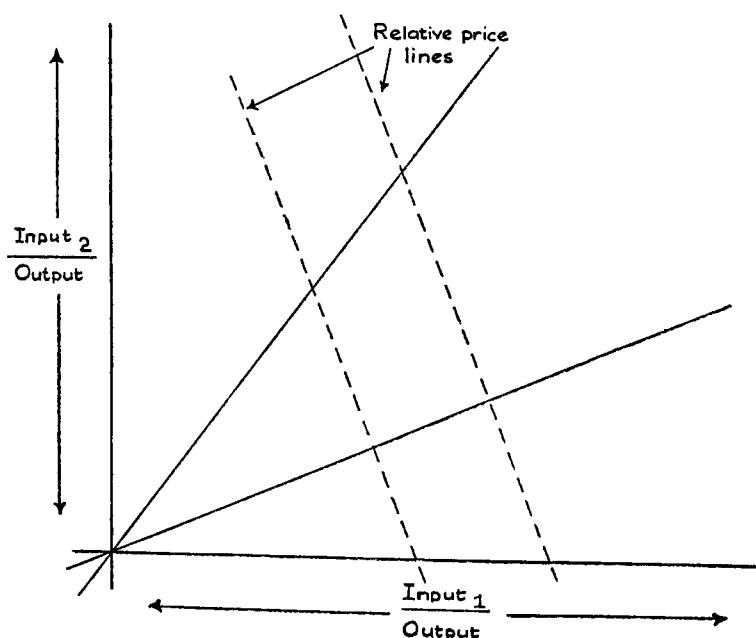
VARIABLE INPUTS

There are various ways in which scales are merged so naturally that one is apt to take the method for granted. Such a situation is the following. An output is made from one raw material at several plants. All other environmental factors are the same at all the plants. Both the quantity of raw material used and the output differ from plant to plant. What measures of efficiency may be appropriate then?

On analogy with what has gone before, one scale might be constructed for each value of the input. But if one was interested in efficiency in use, or some aspect of technical efficiency, then the output one obtained per unit of input would sometimes be most relevant. This immediately implies a merging of the fragmentary scales established at each level of input. It is not necessarily the only merging possible. For example, one may be interested in comparing only small units with small units (where size is measured by input, say). Especially in managerial efficiency, small plants may provide quite different problems from the large plants, and it may only be fair to restrict comparisons to the management of similar size of plants.

The problem is extended in the case for which two raw materials can be used in making a given output. Usually it will be the case that one or other of the inputs will be used exclusively. For each particular mixture of input there will be a fragment of scale. If we accept the type of merging given just above, size of operation will be irrelevant. For many types of question about managerial efficiency the problem is primarily one of reducing money costs. If there are relative prices for the inputs, then amount of output per unit of expenditure of money could be calculated. This itself constitutes a

particular merging of scales. We can use a diagram (suggested for use in this context by Mr. M. J. Farrell):



Even if relative prices are not known, a fragment of scale is provided along each ray in the diagram passing through the origin, since on each such ray the ratio of inputs remains constant, and prices do not enter the picture. (A similar remark could be made about rays parallel to either of the axes.) These fragments can appropriately be merged by working in terms of cost rather than individual inputs. Again in this case there might be questions for which this method of merging was wrong. To take just one example, the prices may not represent accurately the relative scarcity of the inputs.

Now suppose that the environment were such that for each range of choice the ratio of inputs were constant. Different firms might have different ranges of choice, and thus each range of choice would be restricted along a ray of the diagram. If we were interested in target efficiencies, then it would still be necessary to find the 100% point. One way might be to draw a broken line round the edge of the set of points and use this as the 100% line. The method is reminiscent of the so-called "efficient" set of linear programming, and can be counted as a method of interpolation. However, the number of cases for which the environmental restrictions will be of this particular form must surely be small.

This method was recommended for quite general use in a paper¹ by Mr. Farrell irrespective of the form of the constraints. He even suggested that what he called quasi-inputs, such as climatic variables, should be discussed in much the same way as inputs. The present authors pointed out some of the conceptual difficulties discussed here. (Mr. Farrell's work has the virtue of making a start—though perhaps a false start—where previously little had been done. However, his paper still suffers from a central weakness: that he does not analyse the concept of efficiency nor the crucial question of environment.)

The example of linear programming does indeed provide another example of the merging of scales. There it is assumed that there are a set of methods of production, each of which has constant technical coefficients, i.e., a given amount of each input per unit of output. Inefficiency arises from combining these methods of production in the wrong proportion. The boundary line of the points is called the "efficient" set or the set of "efficient points." But in the usual case of use of the method there is a relative price attached to the inputs and then the partial ordering given by "better use a smaller bundle of inputs than a larger one" is replaced by a complete ordering which most of the points of the "efficient" set will certainly be counted as inefficient. All we can be sure of is that the point that is ranked as efficient in the complete ordering will be one of the "efficient set." Thus the set is misnamed. A more appropriate title for the points of the set would be the "not certainly inefficient points." The name given would not be important but for the fact that "efficient point" is a title that persuades us that such a point is a good point to reach. (A similar point arises in the definition of a Pareto Optimum. See I. M. D. Little, *Welfare Economics*.)

Rather similar issues to those given above were raised by some work on prediction by H. Theil.² Theil's work is concerned with the problem of comparing the predictions of the government economists of three different countries, Britain, Holland and Sweden. He looks at their predictions of several of the most important economic aggregates, and compares these with the figures eventually estimated by the government statisticians of the respective countries of the realised values. For each type of magnitude, e.g., industrial production, he calculates a coefficient intended to summarise the achievement of the forecaster in a particular country.

$$f_1 = \frac{1}{T} \sum_{t=1}^T (v_t - r_t)^2$$

Here v_t is the forecast of the magnitude for the year t , and r_t is the realised value for the year t .

¹ "Measurement of Productive Efficiency," *Journal of the Royal Statistical Society*, 1957, Vol. 120, Part 3.

² *International Economic Papers*, No. 5, 1955, H. Theil, "Who Forecasts Best?" quoted by R. L. Marris, *The Position of Economics and Economists in the Government Machine*, *Economic Journal*, Vol. LXIV, 1954, pp. 759-83.

He also calculates a coefficient

$$f_2 = \frac{1}{T} \sum_{t=1}^T (r_t - r_{t-1})^2$$

intended to represent the difficulty of forecasting.

He then calculates an index

$$\Psi = \sqrt{f_1/f_2}$$

as a "forecasting coefficient." With this routine completed he compares the performance of forecasters in different countries forecasting the same type of series, and of a forecaster in a particular country forecasting different series.

Such a measure, because it explicitly introduces the difficulty of the task, is an attempt to assess the forecasters, and not the usefulness of their forecasts. It corresponds to the measure of managerial efficiency, and not to any notion of technical or target efficiency. To obtain the latter, we would have to assess not the difficulty of the task, but how useful a particular forecast was when it was wrong. This would be a formidable, if not an impossible, undertaking. Given that we are judging how well forecasters cope with their difficulties, we are not at all convinced that the author's particular measure of difficulty stands up to analysis. To take an example: if countries predicted, as well they might, changes in some economic magnitude rather than the magnitude itself, and this change in fact remained constant, each would be attempting to predict a magnitude which stays constant. But on Theil's criterion, a high constant change would be counted as more difficult to predict than a small constant change.

Or to take a more practical case. One of the types of series compared is of National Product. Now the level of one country's National Product may be closely related to, say, the level of United States imports, and thus be predictable only if this level is also predictable. Another country might be rather more independent of outside events. The level of its National Product might depend more on, say, the fiscal measures of its own government authorities. The forecaster would in that case be partly, in effect, trying to predict the measures of his own government. These examples could be multiplied almost indefinitely—indeed, there is a lengthy discussion of such problems in the paper¹ from which Professor Theil obtained his figures. We only want here to point out the dangers of merging scales on too arbitrary a criterion.

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¹ "National Budgets in Western Europe," *Economic Bulletin for Europe*, July 1953

IS THE "ECONOMIC EFFICIENCY" OF TAXATION IMPORTANT?¹

FUNCTIONAL Finance tells us that the purpose of taxation should be to cut down private spending when this is necessary to prevent excessive aggregate demand and inflation.² An implication of this principle is that, since their effects on aggregate demand might differ, two equi-revenue taxes are not necessarily perfect substitutes as anti-inflationary fiscal tools: "The more they are paid at the expense of potential savings, the higher the taxes have to be to secure a given restraint on spending."³ Following Kaldor, one may call the net expenditure-restraining effects of taxation per unit of receipt its "economic efficiency."⁴

Possible reasons for unequal "economic efficiency" of the various equi-revenue taxes—and there is an infinite number of taxes if any tax formula is taken as a separate tax—are: (a) differences in the distributional impact of the taxes; (b) announcement effects—the Public Finance equivalents of substitution effects—due to differences in tax formulae; (c) "money illusions" of various kinds. Outlay taxes, for example, are often claimed to be more "efficient" than income taxes,⁵ and corporation taxes—less "efficient" than personal taxes.⁶ Although some arguments to the effect that specific taxes are comparatively "efficient" (or vice versa) might be disputed, it is very probable, for example, that the deflationary effects of a given tax payment of the very rich are considerably lower than those of an identical tax payment of the very poor.

Obviously, for purposes of *fiscal policy* it is important to know what the "economic efficiency" of each tax is, so that what money yield of any tax is economically equivalent—that is, cuts aggregate demand to the same extent and has identical effects on the equilibrium price level—to that of any other tax could be easily determined. All discussions of the inflationary significance of a government budget, balanced or unbalanced, must pay attention to possible differences in the "economic efficiency" of the various taxes. To repeat Shoup's words

¹ I am indebted to Michael Michaely and Don Patinkin for their constructive comments.

² Cf. Abba P. Lerner, *The Economics of Control* (Macmillan, 1949), p. 308.

³ Nicholas Kaldor, *An Expenditure Tax* (Allen and Unwin, 1955), p. 175.

⁴ *Ibid.*, pp. 173–5.

⁵ See E. C. Brown, "Consumption Taxes and Income Determination," *A.E.R.*, Vol. XL, March 1950, pp. 74–89; Richard Goode, "Anti-inflationary Implications of Alternative Forms of Taxation," *Papers and Proceedings, A.E.R.*, Vol. XLII, No. 2, May 1952, pp. 154–9; William H. White, "Measuring the Inflationary Significance of a Government Budget," *I.M.F. Staff Papers*, Vol. I, pp. 362–3.

⁶ Cf. Richard Goode, "The Corporate Income Tax in a Depression," *Policies to Combat Depression* (Princeton University Press, 1956), pp. 149–70.

"... when the discussion centers on degrees of employment, and on inflation or deflation, equality of revenue-raising power is no longer an appropriate unit for comparison. A more relevant unit would be the extent to which the tax forces a reduction in consumer spending or in investment spending; that is, the extent to which the tax forces the private sector of the economy to release economic resources that the government may then hire."¹

But does the comparative "economic efficiency" of taxes have other implications? Is it one of the criteria that should be taken into account when choosing between taxes? Specifically, if two taxes are similar in all other respects,² but tax A requires—because of its lower "economic efficiency"—more money yield than tax B, should tax A be preferred to tax B or should it not?

Since Kaldor lists the relative "inefficiency" of income taxes as an argument for an expenditure tax, he apparently believes that the comparative "economic efficiency" of a tax is a clear advantage to be taken into account when choosing from alternative taxes. To George F. Break, on the other hand, the efficiency argument seems "a considerable overestimation of the advantages of economising on tax revenues, which are not a scarce resource."³

The purpose of this paper is to re-discuss this problem.

THE PROBLEM FROM THE POINT OF VIEW OF AN INDIVIDUAL

The very question would, we think, be hard to explain not only to a Secretary of the Treasury, but also to any layman who, naturally, would tend to think of it in terms of the comparative burdens imposed by unequal money taxes on a single individual. For any individual the superiority of the smaller money tax is only too obvious. Since prices are given to him, a lower money tax is a lower real tax as far as he is concerned; even if the two unequal money taxes reduce the current resource-using expenditures of the individual to the same extent, the lower money tax leaves him with more money balances and/or bonds, so that he *must* be better off.⁴ This, incidentally, is why saving⁵—even compulsory saving—is considered preferable by *any single individual* to paying the very same amount of money

¹ Carl S. Shoup, "Taxation and Fiscal Policy," *Income Stabilization for a Developing Democracy*, edited by Max F. Millikan (New Haven: Yale University Press, 1953), p. 267.

² Shoup (*ibid.*, p. 267) notes that "there is no *a priori* rule in this matter to be applied in advance; we cannot say that, among taxes having the same resource-releasing power, we shall always want to select the one that yields the largest revenue." That is true, of course, but the same might be said of any criterion for tax policy.

³ George F. Break, in his review of Kaldor's *An Expenditure Tax*, in *Economica*, Vol. XXIII, May 1956, p. 177.

⁴ Cf. James Tobin, "Taxes, Saving, and Inflation," *A.E.R.*, Vol. XXXIX, December 1949, p. 1229.

⁵ By savings we shall henceforth mean non-resources-using savings, i.e., savings which are not investment in the national accounting sense.

in taxes, and this is the basis for various tax schemes, that have been often proposed and sometimes also implemented, the essence of which is to induce individuals to save more, pay less in income taxes and other taxes as well.¹ Savings are politically less painful than taxation, not only because in the case of savings there is no compulsion (unless the savings are compulsory) but also because, unlike the tax-payer, the saver is compensated for the cut in his present consumption by bonds and/or money balances.

It follows that if an individual has to cut to a *given level* the present use of some *specific* resources that are needed for the Government's programme, he will prefer that this be done by a partial tax which discriminates against these resources or the commodities produced from them rather than by supposedly non-discriminating taxes. Discriminating taxes, having substitution effects in addition to their income effects, will require less money revenue to achieve their purpose in this case. Thus the argument that excises² impose an excess burden in comparison to an equivalent income tax³ is, *for the case of a single individual*, completely reversed when the function of the alternative taxes is to release a given amount of specific real resources, and when the excise discriminates against those specific resources.

In Fig. 1, *WR* is the pre-tax price-line for *X*, *WB* the amount of *X* bought at this price by an individual, and *AB* the amount of money income spent by him on *X*. An imposition of an excise on *X* changes the price line from *WR* to *WT*; the amount of *X* bought falls to *WD* and the new amount of money income spent on *X* is *CD*. But out of *CD*, *CF* represents the payment of the excise on *X*. If the Government is indifferent to sources of revenues (as long as they are tax revenues) and is interested only in their money size, an income tax *HW* may be substituted for the excise tax *CF*. Since *HW* is equal to *CF*, the Government still gets the same amount of tax revenues from the individual. But this substitution, to which the Government is completely indifferent, is in favour of the individual who is transferred by it from indifference curve *I*₂ to the higher indifference curve *I*₃. As a matter of "fact," the burden of paying *CF* as an excise tax is equal to the burden of paying *WG* as income tax, as both leave the individual on the same indifference curve, *I*₃. *HG* represents, so the argument ends, the excess burden of the excise.

Yet, although the individual is better off if he pays *HW* in income tax than if he pays the same money amount, *CF*, as an excise, his consumption of

¹ Cf. Tobin, *op. cit.*, pp. 1223–32; of course, all proposals for an expenditure tax—temporary or permanent—belong to this category.

² Although the argument was put in terms of *direct vs. indirect* taxes, its essence is a comparison of the welfare effects of *partial vs. general* taxes.

³ Cf. M. F. W. Joseph, "The Excess Burden of Indirect Taxation," *Review of Economic Studies*, VI (June 1939), pp. 226–31. The argument has often been repeated and often criticised. Due to the mainly methodological criticism of Milton Friedman, and the substantial criticism of I. M. D. Little, Earl R. Rolph and George F. Break, the argument is out of favour now. Our reversal of the argument should be taken as an additional criticism of it, rather than as a new "theorem."

No doubt, a distinction should be drawn between the "objective burden," the burden as economists see it, and the "subjective aggregate burden,"¹ but it should be stressed that the subjective burden is the aggregate burden as perceived by individuals not because they are illogical,² but because they are logical individuals. Trescott suggests the term "fallacies of decomposition" for such attitudes of individuals. It should be stressed, however, that while fallacies of compositions are fallacies due to logical pitfalls, the fallacies of decomposition are not fallacies at all from the point of view of those who make them, just as the assumption of perfectly elastic demand that each "pure competitor" makes, for example, is not fallacious. If such perceived burdens are, *ceteris paribus*, to be minimised, then the "economic efficiency" of taxation must be of some importance. However, the importance of the economic efficiency of taxation can be proved in purely economic terms. It is true that generalisations of propositions derived for isolated individuals to the community as a whole, are often invalid,³ but a discussion in terms of the economy as a whole has its own pitfalls; for, after all, the economy consists of individuals. The argument denying the importance of the economic efficiency of taxation is based on the assumption that money is not a scarce resource, as indeed it is not for the economy or for the Government, but money is definitely a scarce resource for each individual. Utilities of individuals are not derived only from real resources, but from money and bonds as well, and, as Patinkin stresses,⁴ money gives utility for individuals not only because it can be spent on goods and services—in that case its utility is only derived—but because it can be held. Money reserves provide security against financial embarrassment because of lack of synchronisation between the inflow and outflow of money in a given period,⁵ and if uncertainty is introduced into the argument, as it should, then the utility supplied by cash balances—precautionary and/or speculative—is greatly enhanced.⁶

The common argument for the case of the economy as a whole neglects the utility of money, perhaps because one cannot represent money on a transformation curve. That argument stresses that if two unequal amounts of money taxes are economically equivalent because of compensating differences in "economic efficiency," they both reduce the resource-using ex-

¹ The terms "subjective burden" and "objective burden" were coined by Paul B. Trescott in his "The 'Burdens' of Government Finance," *Public Finance*, Vol. XI, No. 2, 1956, pp. 164–76, and esp. pp. 164–70.

² We do not suggest that economists should take into consideration welfare losses of individuals which are due to irrational or mistaken beliefs of the individuals; how to treat such beliefs must be decided by politicians. Economists must do their best to enlighten the mistaken.

³ Cf. Milton Friedman, "The 'Welfare' Effect of an Income Tax and an Excise Tax," *Essays in Positive Economics* (The University of Chicago Press, 1953), pp. 100–13.

⁴ Don Patinkin, *Money, Interest, and Prices* (Row-Peterson, 1956), Chapters V, VI, esp. pp. 62–4.

⁵ *Ibid.*, p. 63.

⁶ Cf. J. C. Gilbert, "The Demand for Money: The Development of an Economic Concept," *J.P.E.*, Vol. LXI, April 1953, pp. 144–59.

penditures at the given period to the same extent, which is true by definition, and that, at least if current investment is given, the total potential resource-using expenditures in a future period is also given, which is also true; however, the conclusion that, *ceteris paribus*, both taxes are equally good (or bad) is wrong, *since the lower money taxes leave the private economy* (= all individuals taken together) *with more money balances and/or bonds*.

It should, perhaps, be stressed that the higher money balances that the more "efficient" tax leaves to individuals are also higher real balances, for—and that was the basic assumption—the two alternative taxes are economically equivalent so that the current price level must be the same in both cases.¹ As to bonds, if the present real value of bonds is determined by deflating their money value by the present index of the price level, then, since their money value is greater and the price level is the same in both cases, the real value of the bonds left by the more "efficient" tax is also higher than that left by the less "efficient" tax. Truly, if the owner of bonds plans to have them redeemed in a future period and to consume the proceeds, the real value of bonds should—conceptually—be reached by deflating their money value by the price level in that future period, and it is also true that the real amount of resource-using expenditures in that future period depends only on the resources available in that period, so that, in this case, the aggregate welfare of individuals in that future period does not seem to depend on the amount of their bond holdings in the present period.² However, not all bond-holders plan to redeem all their bonds and to consume all the proceeds. The functions of savings are numerous, and savings cannot therefore be identified with future consumption, an identification which is the essence of the "waiting theory" of interest.³ Even if one does not agree with Rolph that this theory is "a travesty on the evidence," it is hard to deny that savings do add to individuals' wealth, and that greater wealth not only allows more consumption in the future, but also supplies—as long as unspent—more security, prestige and power. To the extent that the function of the bonds is not to provide for future consumption, they might be considered, for the purposes of the problem discussed in this paper, together with money balances. The more "efficient" tax leaves to individuals collectively more of either cash balances or bonds or more of both, and, therefore, *ceteris paribus*, the "economy" must be better off. To conclude: the "economic efficiency" of taxation is important, and it should be taken into account when choosing between taxes.

Incidentally, since consumption taxes—as partial taxes having both sub-

¹ The same price level is, obviously, consistent with different quantities of money if there are compensating changes in the velocity of circulation, which, in this case, must occur.

² If debts be considered as negative bond-holdings, the total amount of bond-holdings in a closed economy should, of course, be zero, but not so if the debts of the government are not considered negative bond-holdings.

³ See Earl R. Rolph, *op. cit.*, pp. 262–5. Rolph notes that his ideas lean heavily upon those of the late Henry Simons.

stitution effects and income effects—are more “efficient” than general income taxes (and this can easily be demonstrated also for the economy as a whole), then this point should not be forgotten when a problem such as the “excess-burden-of-indirect-taxes” is discussed. For the case of the economy as a whole it has been conceded that the excess-burden-argument is correct *if*: (a) it is clear that the partial tax actually distorts the allocation of resources, which is obvious only if the original situation was an optimal one;¹ (b) it is also clear that the effects of the partial and the general taxes on the supply of effort are identical.² When the welfare-implications of the “economic efficiency” of taxation are recognised, these two qualifications are not yet sufficient.

Needless to say, the advantages of the “economic efficiency” of taxation do not imply that more “efficient” taxes should always be preferred. For instance, if the comparative “economic efficiency” of a tax is caused by its comparative regressivity, it should not be forgotten that the less-progressive tax leaves indeed to individuals more money balances than does the economically alternative tax, but that the greater amount of money balances is not left to the very same persons in both cases. Naturally, the possibility that a smaller tax payment by the poor might serve the same purposes as a higher tax payment by the rich certainly does not imply that the payment should be made by the poor.³ Similarly, if two equi-progressive taxes have different “economic efficiency,” the disadvantages of the more “efficient” tax—say, distortive effects as defined by the Paretian welfare economics—should be weighed against the advantages of “efficiency.”

It should also be remembered that more savings and less taxes collected—the characteristics of a more “efficient” tax system—mean that the share of the current generation in the output of future periods will be higher than otherwise, which may or may not be desirable.

The choice among taxes is always a complicated business; that, however, does not detract from the importance of all the factors that complicate it.

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¹ See E. R. Rolph and G. F. Break, “The Welfare Aspects of Excise Taxes,” *J.P.E.*, Vol. LVII, February 1949, pp. 46–54.

² See I. M. D. Little, “Direct *versus* Indirect Taxes,” *ECONOMIC JOURNAL*, Vol. LXI, September 1951, pp. 577–84. Incidentally, the additional point that partial taxes—and a general consumption tax is a partial tax—are more efficient, does not imply that more effort will be supplied under partial taxes; that depends, as usual, on the elasticity of demand for income in terms of effort.

³ The (often disputed) tendency of the marginal propensity to spend to decline as income rises, should—where it exists—be reflected in the rate of decrease of the marginal utility of income, so that the conclusions of the sacrifice-doctrines analysis should still be valid.

NATIONAL AND AGRICULTURAL INCOME 1851

IN a review of national income estimates by nineteenth-century writers—including, as chief contributors, Levi, Baxter, Mulhall and Giffen—Phyllis Deane has brought the results to a focus by showing them in relation to an estimate for 1850,¹ obtained by extrapolating from the series given for later years by Jefferys and Walters.² The projection to 1850 was made by means of Stamp’s series of gross taxable incomes and Bowley’s wages index, and yielded a figure of £591 million as a broad measure of “order of magnitude for total national income” at that date.

Another estimate is £588 million for 1851, given by Levi in *Wages and Earnings of the Working Classes*, 1885. Although there was some improvement in trade from 1850 to 1851, the two assessments may be regarded as reasonably concordant. Both, however, are much below Mulhall’s “implicit estimate” for 1850—an aggregate of £690 million not actually presented by Mulhall, but obtained by Miss Deane by the use of methods and figures given in his *Dictionary of Statistics* and other works. In view of the usefulness of a firm judgment on the level of income per head in the middle of the century, it may be of interest to add further independent evidence tending to reinforce an estimate near £591 million and suggesting that at least this is not too low.

An alternative way of assessing the national income is to project backwards the trends of certain ratios showing the relationship between wage-rates³ and other forms of income. Owing to the great perseverance of G. H. Wood in his inquiries at the Board of Trade before 1909, it is possible to obtain a wide enough range of wage-rates for England and Wales, Scotland and Ireland to prepare United Kingdom series separately for agriculture and industry reaching back to 1850. An important source is Wood’s unpublished bound volume, *Rates of Wages and Hours of Labour, 1851–1906*, a copy of which is in the Library of the Royal Statistical Society. The relevant material from this has been extracted by W. D. McMullen and combined with further data from *British and Foreign Trade and Industry*, Cd. 4954, 1909, and an article by Bowley and Wood in the *Statistical Journal*, 1906, to form a series of the annual average of industrial wage-rates.

A second index of industrial wage-rates was obtained by making an

¹ *Economic History Review*, April 1957.

² “National Income and Expenditure of the United Kingdom, 1870–1952,” *Income and Wealth*, Series V.

³ The industrial and agricultural wage-rate series used in this comment are parts of two annual series for 1850–1947 prepared for an inquiry into economic relationships between agriculture and industry, and are described in Research Papers available at the Agricultural Economics Research Institute, Oxford.

unweighted average of the relatives for eight industries or trades, given by Wood in the *Statistical Journal* for 1909. These relatives refer only to selected dates from 1850 to 1906, but as the article containing them was the last of a long series and took into account all that had gone before, the relatives have been used as the basis for the series presented here (Table I), with interpolations by means of the first-mentioned index. Close agreement between the two was to be expected, and with 1906 as 100 both give 69 at 1850.

A series of average *agricultural* wage-rates was similarly obtained by extending an index for 1880–1914, published in the *Nineteenth Abstract of Labour Statistics*, back to 1850 with the aid of Wood's agricultural wage relatives. Basically, the resulting series refers to the payment for a full normal week without overtime,¹ and is thus comparable with the industrial series. Where allowances in kind are a recognised part of the weekly contract wage they are included in the series. The total money value of the average rate paid in 1914 to ordinary farm labourers has been assessed from records,² mainly official, for England and Wales, Scotland and Ireland. It has been possible to check the nineteenth-century estimates, obtained by linking the index to the money rate for 1914, by comparisons with the agricultural wage studies of Purdy,³ Wilson Fox⁴ and Little.⁵ No subject appears to have been more exhaustively examined by statisticians than farm wages in the period 1850–1907.

Ratios

The true interpretation of ratios reflecting trends in the nineteenth century seems possible only by first taking into account the conditions which led to the Great Famine in Ireland in 1845–46, from which a million people died. Though the immediate cause was the failure of two successive potato harvests, the Famine was the outcome essentially of some thirty years of economic decline, during which the rapid growth in numbers was associated with a deterioration in the condition of the farm community to the point at which large sections of them were living perpetually on the verge of destitution. They had become stripped of every basic resource except the potato, on which even their meagre livestock depended. After the Famine the recovery was only gradual. Thus in the period immediately following 1846 every index purporting to show the economic position of British farm-

¹ In the years between Wood's selected points from 1850 to 1880, however, entries have been interpolated by means of Bowley's series, which relates to farm labour earnings.

² *Labour Gazette*; 17th and 22nd *Abs. of Labour Stats*; *Report on the Financial Results of the Occupation of Agricultural Land and the Cost of Living of Rural Workers*, Cmd. 76, 1919; *Earnings and Hours Report*, Cd. 5460, 1910; *Report on the Operations of the Agricultural Wages Board for Ireland during Sept. 1917–Sept. 1921*; Bowley, *Prices and Wages in the United Kingdom, 1914–1920*; Orwin and Felton, *J.R.A.S.E.*, 1931.

³ *Statistical Journal*, 1861, 1862.

⁴ Cd. 2376, 1913.

⁵ Royal Commission on Labour, 1891–94, *The Agricultural Labourer*, Vols. II, V.

ing was weighted heavily by the numbers¹ and stark poverty of the Irish. When the indexes began to show improvement it was partly because the Irish figures reflected recovery from an extreme depth of privation.

Industrial workers in Ireland were by no means in the same plight. The few manufacturing firms which had survived the competition from Great Britain after the establishment of Free Trade in 1800–25 were obliged to pay wages high enough to prevent skilled workers from migrating either east or west, and builders were under the same compulsion. The average weekly rate for bricklayers, masons, carpenters and joiners, compositors and fitters, as shown in Wood's data for Ireland, was 27s. 3d. in 1860, compared with 28s. 1d. in Yorkshire and Lancashire, 33s. 2d. in London and 23s. in Scotland. At that time the farm labourer in Ireland was receiving, on the average, only 6s. 5d. a week. The *ratio* of the farm labourer's wage to the average for these industrial trades was thus 24% in Ireland, compared with a percentage for Scotland of 50; Wales, 55; London (agriculture: Kent, Surrey, Herts, Essex), 35; Yorkshire and Lancashire, 51; and South-west England, 45.

Over the whole of the United Kingdom a comparison of the average weekly rate for farm labourers and the average for all classes of adult male industrial wage-earners, as assessed for the series in Table I, was 44% in 1850, 48% in 1860 and 51% in 1880.² Thereafter the depression in agriculture led to a decline to 46% in 1899.

Table I, column 1, gives the annual estimates of the average of weekly wage-rates for adult males in industry in the United Kingdom; and the corresponding series for agriculture is in column 2. These two wage series provide the basis of the further ratios which will be used for assessing certain major parts of the national-income estimate to be compared with that of Miss Deane for 1850.

NET FARM INCOME

From and including the period 1870–73, the totals of the net income of British agriculture in Table II, column 2, have been compiled without reference to wages or other factor income.³ They represent the difference between total farm production—that is, the product of acreage, yield per

¹ The Census records a larger number of farmers in Ireland than in Great Britain throughout 1851–1911. The number of contract workers on Irish farms was, however, less than in Great Britain during this period, and declined rapidly from 1851.

² With the growth in the size of towns there developed important differences in urban and rural retail prices, the average excess of the urban over the rural level being estimated to be about 11–12% in 1904.—“United Kingdom: Comparison of Urban and Rural Retail Prices,” *Statistical Journal*, 1953.

³ The estimates are part of a series prepared for the Agriculture and Industry Enquiry. Contributors included A. J. Boreham, D. K. Britton, K. E. Hunt and F. D. W. Taylor; and assistance was given by E. M. Ojala in the initial stage. Methods of construction are described in the Agricultural Economic Society's *Journal of Proceedings*, February 1953. The groups of years distinguish phases in agriculture, whether determined by price changes or physical conditions.

TABLE I

United Kingdom: Industrial and Agricultural Wage-rates, and Non-farm Incentive Income per Man-Week

	Average of industrial weekly wage-rates.	Average of agricultural weekly wage-rates.	Ratio of non-farm incentive income per man-week to (1).		Average of industrial weekly wage-rates.	Average of agricultural weekly wage-rates.	Ratio of non-farm incentive income per man-week to (1).
	(1)	(2)	(3)		(1)	(2)	(3)
	s. d.	s. d.	%		s. d.	s. d.	%
1850	21 2	9 3		1882	27 3	13 6	99.3
1851	21 5	9 4		1883	27 2	13 7	102.0
1852	21 10	9 5		1884	26 10	13 8	99.1
1853	23 3	9 9		1885	26 7	13 7	98.0
1854	23 11	10 7		1886	26 2	13 6	101.5
1855	24 0	10 7		1887	26 1	13 5	102.8
1856	23 9	10 8		1888	26 8	13 5	107.7
1857	23 4	10 9		1889	27 7	13 5	111.1
1858	23 0	10 5		1890	28 9	13 7	111.9
1859	22 9	10 5		1891	29 2	13 9	107.8
1860	22 8	11 0		1892	29 0	13 11	107.5
1861	22 11	11 0		1893	28 7	13 11	105.7
1862	23 3	11 0		1894	28 6	13 11	110.0
1863	23 9	11 0		1895	28 5	13 11	115.2
1864	24 2	11 1		1896	28 10	14 0	112.7
1865	24 6	11 6		1897	29 6	14 1	113.0
1866	25 6	11 7		1898	30 3	14 3	115.7
1867	25 2	11 4		1899	31 1	14 5	115.6
1868	25 1	11 6		1900	31 11	14 9	118.3
1869	25 0	11 4		1901	32 0	14 11	113.3
1870	25 2	12 1	98.3	1902	31 9	15 0	112.7
1871	25 3	12 2	102.7	1903	31 5	15 2	110.2
1872	27 1	13 4	100.2	1904	31 2	15 2	112.0
1873	28 8	13 5	103.1	1905	31 0	15 3	115.7
1874	29 0	13 5	101.2	1906	30 10	15 3	122.4
1875	28 4	13 9	95.8	1907	31 9	15 4	124.3
1876	27 10	13 10	98.1	1908	32 4	15 5	113.6
1877	27 4	13 10	101.4	1909	31 11	15 5	117.0
1878	27 3	13 10	99.8	1910	31 9	15 6	120.1
1879	26 9	13 8	98.7	1911	31 11	15 8	119.8
1880	26 10	13 6	98.1	1912	32 4	15 10	125.8
1881	27 0	13 6	98.8	1913	33 4	16 3	125.9

acre and the farm-gate price for crops, plus the realisation value of livestock and their products, whether marketed or consumed by the farm households,¹ plus or minus the relevant inventory change—and, as principal deductions: (1) the value of British crops used for further production on farms; (2) inputs from outside British agriculture; (3) taxes on farm land, rates and tithe; (4) depreciation of farm and estate assets; and (5) payments by landowners to non-farm, non-estate personnel for estate repairs. The difference between the two groups of items is “net income” in the sense that it is the residual remaining to the factors or owners of factors, on the assumption that they maintain their assets in an unchanging efficient state. In comparisons with other historical estimates of farm output or income it will be found that the definitions traditionally used have more nearly reflected “gross product,” having taken little account of expenses (4) and (5) and the not inconsiderable item of tithe.

1851

The estimate for 1851, in column 2, is based on the strict definition of net farm income; but it is reached by the alternative method, that of

¹ Self-supplies are valued so far as possible as though purchased in local shops.

summing the separate assessments of net rent, interest, wages and the farmer's incentive income for 1851.

Net Rent

The averages in column 3 are derived from an annual series originally covering 1867–1938.¹ This has been extended to 1851 to yield an estimate of aggregate net rent of £28 million, which, if it has a bias, is thought to be somewhat high.²

Interest on Occupier's Capital

An annual series of occupier's capital, 1867–1938, compiled by A. J. Boreham,³ gives a total for the United Kingdom of £477 million for 1867–69. His data have been reproduced in a smooth curve for comparison with contemporary estimates; and it is noticeable that the curve shows movements similar to those of our gross rent totals. The long-term agreement in the nineteenth century is particularly close: in both series there is a net fall of 22% from 1870 to 1900. If broadly uniform change is assumed between 1851 and 1867, the estimate of occupier's capital for 1851 is £380–£390 million. At the current yield of consols plus 1%, the interest on this is about £16 million.

¹ “Gross and Net Farm Rent in the United Kingdom, 1867–1938,” *Journal of Proceedings of the A.E.S.*, March 1954.

² Gross rent, or the actual payment made by the tenant to the landowner, has been evaluated as an average per acre at certain dates by means of official and other land inquiries (see a summary by D. K. Britton in *The Rent of Agricultural Land in England and Wales, 1870–1946*, published by the Central Landowners' Association, 1949). If the average gross rent per acre ascertained for any year be multiplied by the recorded national acreage, the resulting total rent may be linked to an annual index of rent per acre (e.g., *op. cit.*: Memorandum by H. A. Rhee) and national acreage, to yield gross rent totals for all years covered by the index. This method gives convincing results for England and Wales for a period extending back to 1850. The graph of the aggregates of rent thus obtained shows a continuous interpretable relationship to the graph of the Schedule A valuation of land. Once the relationship is apparent, the Schedule A data can be used for imputing gross rent in conditions where there is no adequate basis for the first method. For Scotland and Ireland, reliance on the Schedule A returns, adjusted according to principles detailed by Lord Stamp in *British Incomes and Property*, is in fact often necessary. The two methods, applied to 1851, lead to an estimate for the United Kingdom of slightly over £50 million.

From this figure certain costs or charges, some of which have been mentioned, are deducted to obtain net rent: land tax, tithe, rates paid by the landlord, repair, depreciation, insurance and management, and the rental value of dwellings as distinct from farm buildings. (The value of house accommodation appears in columns 5 and 6 as part of the income of farmers and farm workers; and the landowner's receipts from dwellings, on or off the farm, are in the difference between columns 9 and 10, with the receipts of other owners of house property.)

The ratio of net rent to gross rent, on these definitions, is estimated to have been 55% in 1867. This percentage was above normal; the date falls in the middle of the Golden Age, and in good times the occupier more readily undertakes some of the many minor repairs which are ordinarily the landlord's responsibility. As the ratio of 55% has in fact been applied to 1851, to give the net rent figure of £28 million in column 3, this may be regarded as certainly not too low.

³ *Farm Economist*, Vol. VII, No. 6. The series represents the summation of annual estimates of livestock; machinery and implements; harvested crops, growing crops, cultivations and reserve cash; and other items.

Wages, etc.

Farm labourers in 1851 received a weekly rate, including allowances in kind, of about 9s. 4d.,¹ according to our estimate of the average for the United Kingdom; and following the principle adopted in the estimates for 1867–1938 an addition of 6% may be made for the excess of earnings over the wage-rate. A further 3%² increase in the average would probably account for the higher pay of bailiffs, managers and salaried workers. Included in the “Wages, etc.,” is the “incentive income” of market gardeners, nurserymen and others who appear under agriculture in the census and contribute to agricultural output, but are not ordinarily classed as farmers. Their “incentive income” is what remains of their income after deduction of the return to their property; and for 1851 it was probably duly covered by the total of 9% in excess of the average weekly rate for land workers. Including the recipients of this incentive income, there were approximately 2.2 million man-units³ in the group receiving the “wages, etc.,” in 1851, and their aggregate income in this category is estimated to have been not more than £58 million.

Farmers' and Relatives' Incentive Income

If the sum of the foregoing classes of income, net rent, interest and wages, etc., be deducted from total net farm income, there remains a residual which is the “incentive income” of the farmers and their relatives engaged on the work of the farm. It is their return for initiative, effort, management, decision and risk, or the return to the human factor as such. Theory would suggest that there must be some long-term relationship between this income per head and the level of farm wages, inasmuch as there is a continuing change of roles between independent producers and hired workers, and the inducement to assume the responsibility of entrepreneurship is likely to be reflected in some premium. However, in practice the premium varies widely according to market conditions. During the part of the Golden Age for which there are figures, 1867–78, the incentive income per man-unit in the farmer-and-relative group is estimated to have averaged 145% of the basic wage-rate in agriculture. In the later depression, 1879–96, when farmers clung to their vocation till driven out by bankruptcy, the average was barely 80%. In the further years to 1913 there was some relief from overseas competition and the downward pressure of prices, and the ratio for the whole period recovered to about 108%.

¹ The number of contract labourers in Irish agriculture in 1851 is uncertain, and the wage series is probably high at that date owing to the under-weighting of the Irish component.

² Reduced from the 4% allowed in 1867 because of the higher proportion of Irish in 1851 and on grounds of other trends.

³ Women and girls are assessed at two-thirds of a man-unit and youths at two-thirds or three-quarters according to the dividing age between adults and youths as shown in the Census.

The available evidence suggests that the season of 1851 was somewhat below normal.¹ On the other hand, competition from abroad had reached no great intensity by the middle of the century, and the normal ratio itself may have been moderately high. It seems that a reasonable figure to adopt would be 110%, and that in any event this would not be too low. Applied to a wage-rate of 9s. 4d. and 0.8 million man-units, this gives an aggregate incentive income of £21 million.

The summing of the four shares thus produces an estimate of net farm income in 1851 of £123 million as probably an upper limit.

NON-FARM NET INCOME

It is proposed to make an estimate of non-farm income for 1851 in two parts: the return to non-farm property; and the remainder, that is, non-farm incentive income, which may conveniently be considered first.

Non-farm Incentive Income

Table I, column 3, presents a ratio, the projection of which provides a basis for judging the level of non-farm incentive income per man-unit in 1851. It is the ratio between the wage-rate computed for industry, in column 1, and the average per man-unit of non-farm incentive income.² This average is obtained by dividing the aggregate of non-farm wages, salaries and profits, net of the return to property, by the total number of non-farm producers, whether independent or employed, reduced to man-units.

Owing to the great expansion, as from the middle of the century, in the professional, banking, commercial and administrative classes—composing the new urban middle class, which in due course replaced the landowning community in every sphere of national influence—the rise in incentive income per head relatively to wage-rates persisted, in its general trend, till 1913. If the data in Table I, column 3, be plotted, the trend line of the graph curves upwards into the nineteenth century; and when the line is projected back to 1851, the ratio which may reasonably be read at that date is about 94–97%.⁴ The possibility of there being a *normal* ratio less than 100% arises from the existence of numbers of small-scale producers

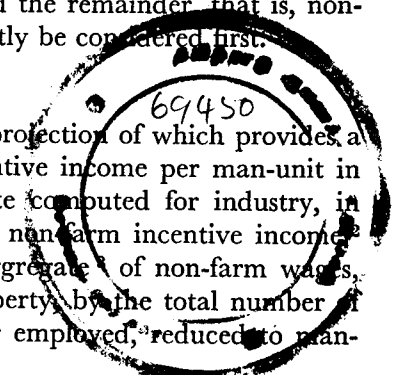
¹ 1850 was appreciably worse, and for that reason there would have been greater difficulty in adopting it here as basis.

² The money amount of the average is not shown except indirectly through the ratio in column 3.

³ From 1870 the aggregates have been derived as residuals from Prest's national income estimates, *ECONOMIC JOURNAL*, 1948, by subtracting from these our totals for: (1) net farm income and (2) the return to non-farm property (see later). Prest's series have been used by us since 1948 as the foundation for analyses needed for the agriculture and industry inquiry. If, as the author thought, their bias might be towards understatement, this would tend to make the present non-farm residuals low; but the tests implicit in our analyses lead to the opinion that any understatement is very slight.

⁴ The difficulty of projecting the trend line is accentuated by the effect on the ratio of the three years of rising prices and prosperity 1871–73, and there is a fairly wide margin of doubt at 1851.

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Xm 56/1900
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whose income, less an allowance for interest, is below the average wage-rate.¹ This group was probably numerous in the days when machinery was still ousting independent craftsmen on a large scale. Moreover, concealed unemployment was no doubt widespread; and it may be noted that every pedlar, odd-job man and country itinerant registered in the census comes within the class receiving the incentive income.

During a trade slump the ratio falls below its norm, inasmuch as profits receive the impact more severely than wage-rates. And when interest is deducted from profits at a relatively inflexible rate the residual incentive income declines more than proportionately to total profits. This has considerable relevance for 1851, for according to Beveridge's series of industrial activity² the year was 5% below normal.

It would seem impossible to arrive at an exact figure for the ratio in 1851, but for present purposes it will be sufficient to take an estimate of 97% as being, on the evidence of both the trend of the curve of the normal ratio and the state of trade in 1851, not too low. When used in conjunction with a wage-rate of 21s. 5d. and 7.5 million man-units, this leads to an aggregate non-farm incentive income of £405 million.

Interest and Rent on Non-farm Property

Giffen's method of assessing the value of an industry's capital by multiplying the annual profit or yield by an estimated number of years' purchase has given unrealistic results when applied to farm capital, but it is more reliable for appraising non-farm property, the yield of which is ascertainable from tax records, and can be related fairly accurately to the capital entailed in production. For purposes of the difference between columns 9 and 10, in Table II, the estimates of property³ have been derived from a sequence of statements based on Giffen's method, including those which he published in *The Growth of Capital*. As it is desirable to use the same basis for the assessment for 1851, Giffen's capital account for 1865 has been taken as the point of departure. Two ways of projection⁴ to 1851 led to similar results. At an interest rate of 4.09% one method gave slightly more and the other slightly less than £85 million as the total return to non-farm property.

¹ This average rate takes no account of unemployment, and therefore the comparison in Table I, column 3, is between incentive income per man-unit and the amount which adult male wage-earners would earn if they received the weekly rate for fifty-two weeks in the year.

² *Full Employment in a Free Society*, p. 311.

³ See *Agriculture and Industry: Relative Income*, 1956, p. 229—the report of the agriculture and industry inquiry.

⁴ One method consisted in dividing the non-farm capital items in 1865 into (a) house property and (b) business capital, so that the first could be linked back to 1851 by an index of houses drawn mainly from the census, and the other could be carried back by means of Stamp's series of total profits assessed under Schedule D, in *British Incomes and Property*, p. 218. The second method was to interpolate a figure between Giffen's broad estimate of £4,000 million for 1845—assuming a certain ratio of farm to non-farm capital at that time—and that of 1865, with the aid of indices of prices and business activity.

NATIONAL INCOME

The aggregate emerging as net national income at factor cost is £613 million. For reasons noted earlier the agricultural component of this, £123 million, is thought to be somewhat high, having regard to the evidence that 1851 was agriculturally sub-normal. Concerning the non-farm part

TABLE II

United Kingdom: Income Distribution

Annual average.	National income.	Agriculture.						
		Total factor income.	Net rent.	Interest.	Wages, etc.	"Farmer's incentive income."	Farmers and relatives man-units.	"Farmer's incentive income" per man-week.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	£ mn.	£ mn.	£ mn.	£ mn.	£ mn.	£ mn.	000s	£
1851	613.3	123.2	27.8	15.8	58.2	21.4	800	0.514
1870-3	1,022.5	154.7	35.8	19.9	55.1	43.9	812.7	1.038
1874-8	1,094.0	149.0	36.4	20.3	54.4	37.9	870.6	0.837
1879-83	1,109.2	121.9	32.8	16.7	48.7	23.7	930.2	0.490
1884-7	1,138.3	114.6	29.0	15.6	45.7	24.2	932.7	0.499
1888-91	1,341.0	115.9	26.4	14.6	44.7	30.2	926.8	0.627
1892-6	1,396.0	107.6	23.4	13.7	43.3	27.3	932.8	0.562
1897-1902	1,668.3	115.0	21.8	13.3	42.7	37.2	946.8	0.756
1903-5	1,758.0	114.9	22.8	14.7	43.7	35.8	939.4	0.692
1906-10	1,987.2	126.7	24.5	15.2	44.6	42.4	922.4	0.884
1911-14	2,260.5	143.4	26.5	17.6	47.6	51.7	917.8	1.084

Annual average.	National income.	Non-farm enterprise.				Incentive income ratio (8):(12).
		Total non-farm income.	Non-farm incentive income.	Non-farm producers man-units.	Non-farm incentive income per man-week.	
	(1)	(9)	(10)	(11)	(12)	(13)
	£ mn.	£ mn.	£ mn.	000s	£	%
1851	613.3	490.1	405.1	7,500	1.039	49.5
1870-3	1,022.5	867.8	669.1	9,577.7	1.343	77.3
1874-8	1,094.0	945.0	714.6	9,909.6	1.387	60.3
1879-83	1,109.2	987.3	724.3	10,373.8	1.342	36.5
1884-7	1,138.3	1,023.7	736.1	10,684.0	1.325	37.7
1888-91	1,341.0	1,225.1	935.5	11,697.0	1.538	40.8
1892-6	1,396.0	1,288.4	996.6	12,121.4	1.581	35.5
1897-1902	1,668.3	1,553.3	1,227.3	13,220.3	1.786	42.3
1903-5	1,758.0	1,643.1	1,255.1	13,735.5	1.758	39.4
1906-10	1,987.2	1,860.5	1,416.8	14,379.7	1.895	46.6
1911-14	2,260.5	2,117.1	1,593.2	15,419.9	1.987	54.6

assessed at £490 million, it is less easy to comment definitely, as the scope for error is greater; but 1851 was an industrially poor year, and it seems that the allowance for this is more likely to have been too little than too much, and that the resulting aggregate has an upward bias.

In so far as a figure in the neighbourhood of £613 million can be accepted as not too low, it would suggest that the estimate of £591 million given by Phyllis Deane for the previous year was also not too low if the conditions of the particular year are taken into account. Beveridge's index of industrial activity was 8.6% below the current normal in 1850, and the price index

was 107¹ compared with 130 in 1847 and 138 in 1854. The season of 1850 was reported to be one of great agricultural distress.²

In the comparison between Levi's national income total of £588 million for 1851 and the "implicit estimate" of £690 million for 1850 derived from Mulhall's data, the evidence thus favours Levi's result, though this may be low by, say, 1-3%. As a round figure, a national income of £600 million for 1851 may be held in mind and regarded in the same light as the approximate assessment of £2,000 million for 1907, a key date marked by the taking of the first Census of Production.

Rate of Expansion

The significance of 1851 rests not merely on its being a convenient base year at the start of the second half of the century; it was also near the beginning of a phase of unprecedented national investment. The great expansion of railway transport and shipbuilding, coupled with coal-mining, mechanisation and mass production in factories, joint-stock enterprise, banking and commerce, which took place in the third quarter of the century, was the prelude to the immense outpouring of goods and the comprehensive rise in the level of consumption in the fourth quarter. Inasmuch as the growth in instruments of production and economic organisation denoted a rise in the incomes of investors and administrators, the accompanying rise in the national income was both appreciable and unevenly distributed. If the national income in 1851 be assumed to have been between £600 million and £613 million the rise in real income per head from that date to 1878—a year of transition at the end of the Golden Age of Agriculture—would appear to have been about 27-30%. The increase in agricultural real wage-rates exceeded this, partly because the level was much depressed in all parts of the Kingdom in 1851. But the rise in industrial wage-rates, in real terms, was only 15% according to our series based mainly on Wood's material.³

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¹ Jevons index reproduced in Layton and Crowther, *The Study of Prices*, Third Ed., p. 237. Sauerbeck's figure for 1850 is 91, between 112 (1847) and 120 (1854).

² R. J. Thompson, *Statistical Journal*, 1907.

³ It is not impossible that Wood's collection contains a disproportionate number of series for skilled or other organised trades whose wages were relatively high in 1851 and showed a less than average rise from that date to 1878. If, however, the average derived from his series had an upward bias in 1851, there is stronger reason for judging a national-income estimate of £613 million for 1851 to be a maximum, and for considering Miss Deane's appraisal of £591 million for 1850 to be a maximum also.

INTERNATIONAL ECONOMIC DEVELOPMENT

THE three books that will be discussed in this article¹ mark Dr. Myrdal's entry into the field of the economics of under-developed countries and their development. The entry is massive, not only because the author is a leading social scientist and former international administrator, but also because of the sheer bulk of these volumes. Although they are bound to be influential, albeit in the limited sense of supporting already fashionable and widely held points of view and lines of approach which are rarely examined critically, the books may disappoint not only those who recall his earlier contributions to monetary theory but also many of those who, in the present field, sympathise with his proposals for policy. This disappointment may, I fear, arise because the books provide so little in the way of new knowledge or new insights; and the most substantial, *An International Economy*, seems to me to fall seriously short of its author's expressed objectives "to state [his] premises explicitly, get the facts straight, and think things out stringently" (*Preface*). The books do, however, deserve serious attention as presenting a comprehensive exposition and convenient examples of the most influential and widely held ideas on under-developed countries and economic development. In discussing the following I shall try to keep distinct arguments based on technical economic reasoning or on empirical evidence from those stemming from a political position.

Since the books cover an immense field and a vast range of topics, a review which attempts to deal with the most important issues inevitably has to wander far afield. I shall begin (Section I) with some comments on Dr. Myrdal's explicit methodological position which underlies the whole treatment. A second section summarises his central argument. The principal elements within the argument are discussed in Sections III-VI, followed in Sections VII-IX by a discussion of his main policy concepts and proposals.

¹ *An International Economy: problems and prospects*. By Gunnar Myrdal. (London: Routledge & Kegan Paul, 1956, pp. xi + 381. 37s. 6d.)

Development and Under-development: A note of the mechanism of national and international inequality. By Gunnar Myrdal. National Bank of Egypt 50th Anniversary Commemoration Lectures. (Cairo: National Bank of Egypt, 1956, pp. 88.)

Economic Theory and Under-developed Regions. By Gunnar Myrdal. (London: Gerald Duckworth, 1957, pp. xii + 168. 18s.)

The general argument of these three books is essentially the same. The first (referred to here as *International Economy*) is the more substantial; references to the other two books (quoted as *Cairo Lectures* and *Under-developed Regions*), the presentation of which is somewhat clearer and crisper, help to interpret the author's meaning when the exposition of the larger book is ambiguous.

I

Dr. Myrdal writes in the Methodological Appendix to *An International Economy*:

"There is no way of studying social reality other than from the viewpoint of human ideals. A 'disinterested social science' has never existed and, for logical reasons, cannot exist. The value connotation of our main concepts represents our interest in a matter, gives direction to our thoughts and significance to our inferences."

"The recognition that our very concepts are value-loaded implies that they cannot be defined except in terms of political valuations. It is, indeed, on account of scientific stringency that these valuations should be made explicit. They represent value premises for the scientific analysis; contrary to widely held opinions, not only the practical conclusions from a scientific analysis, but this analysis itself depends necessarily on value premises."¹

I find this difficult to accept. The principal concepts of economics are surely not value loaded; for example, concepts of opportunity cost, demand and supply as functions of price, the volume of output or of imports and exports, the consumption function and the multiplier, and the devices of aggregation, general and partial equilibrium analysis and period analysis.² This is true also of such positive propositions as the law of demand or the conditions of price discrimination.

Dr. Myrdal's methodology, and with it much of his argument, seems to me to be vitiated by failure to distinguish between positive and normative propositions, a distinction which is essential in discussions on method, analysis and policy. The substantive propositions of positive economics embody the discernment of uniformities underlying the diversity of phenomena. Their validity is entirely independent of political positions.³ But equally they are insufficient for policy recommendations, since these depend on assessments of the probable results of particular courses of action on the total social situation allowing for administrative and political possibilities; and also on value judgments, notably concerning alternative types of society and social arrangements. Thus, when policies are proposed, value premises and, more generally, bases for recommendation should be set out. But this is very different from the suggestion that the validity of economic propositions depends on value judgments; that they are scientific only if these are stated; and that the statement of value judgments confers scientific status on a proposition; or that their value and significance depends on their contribution to goals of policy.⁴

The significance of a scientific proposition depends primarily on its

¹ *International Economy*, pp. 336-7.

² This freedom from value loading does not, of course, in itself establish their usefulness.

³ The same applies to the useful tautologies which serve as an analytical filing system of economic theory.

⁴ On this argument scientific significance would be conferred on the biological theories of the National Socialists if that party returned to power in Germany.

generality and depth;¹ and in a phenomenal subject its validity depends on conformity with observed phenomena. Of course, there is often disagreement in the interpretation of phenomena which may or may not stem from a political position. Further, the field of study selected may be much affected by a political position. But this does not bear either on the significance or on the scientific value of a proposition.

The following appear to be some of the inevitable results and implications of the author's methodological position. It confuses the advancement of knowledge with the promotion of policy. It obscures the meaning of achievement of results: instead of meaning the establishment of successful and illuminating generalisations, it comes to mean the achievement of particular aims of policy. It prevents assessment of the competence of reasoning and of the validity of propositions, and thus also the establishment of minimum standards of technical competence, since criticism can always be ascribed to political differences. It destroys all possibility of reaching agreement even on elementary propositions, and it also prevents definition of the grounds of disagreement. It implies that the logical status of all propositions in economics is equal, because their validity depends on political acceptability or on their political results, and not on internal consistency or on correspondence with empirical evidence. This in turn spuriously justifies in academic and scientific work the emergence of political statements which do not reflect and promote systematic reasoning, though they may advance the social causes espoused by the author. Such a position destroys economics as a systematic discipline.

Although his position is untenable, Dr. Myrdal has rendered a great service with the statement of his position. Henceforth disregard of these important issues of method will be much more difficult, both because he has had the courage explicitly to state a position which is widely accepted implicitly or even subconsciously; and also because these books reveal the results of its thoroughgoing espousal.

II

The main theme of these books is world economic integration. An economy is integrated if its members have equal opportunities, which implies substantial equality of wealth and income. But the unchecked operation of social forces, especially of the market mechanism, is towards cumulative disequilibrium and disintegration by a process of circular cumulative causation. The few developed countries have become largely integrated internally through state action helping poor people and depressed industries. Very different is the position of the under-developed countries in which live over

¹ That is on the penetration required to discern the uniformities which they embody. It is a valid generalisation that human beings are born with two legs, but this is of no significance because it is obvious. The recognition that Mendel's Law applies to all forms of life is of very great interest and significance, because it is far from immediately obvious that the laws of heredity are the same for all living organisms.

two-thirds of humanity. Poverty and colonial rule have prevented their national economic integration, and have set up a wide and widening gap between the developed countries (often termed by Dr. Myrdal the privileged and upper crust, or upper class of nations) and the under-developed countries (often termed the under-privileged nations). This disparity, the result principally of the cumulative tendency towards inequality in the absence of world government, is the outstanding example of disintegration in the world to-day.

Without deliberate large-scale national and international action this gap must widen further until it results in a world political catastrophe. Large-scale government planning is necessary to integrate and develop the stagnant under-developed economies. Internationally the under-developed countries must combine to increase their bargaining power; and developed countries must abandon their unsympathetic attitude, which is a partial cause of the desperate poverty of the under-developed world, and assist it chiefly by capital and income transfers, by raising or stabilising their export prices, by assisting them in their national planning, by refraining from forcing their own exports on the under-developed countries, and by reducing the barriers facing exports and to some extent immigrants from the under-developed world.

III

The position and prospects of the under-developed world are Dr. Myrdal's overwhelming concern. Two basic arguments underlie and pervade the treatment. One is that all economic propositions are political statements. This follows from the methodological position already examined.

The second proposition, the general operation of circular cumulative causation, is the cornerstone of the whole discussion. The author argues that circular cumulative causation, which he describes as his main hypothesis, is the general rule in social life, and that the vicious circle of poverty and the stagnation it implies constitutes an important example of this phenomenon. However, this is a simple assertion, the general validity of which is readily refuted by observation. Poverty is compatible with even rapid advance if this is of recent origin, and has begun from a low level. This is the position of many, indeed most, under-developed countries and regions which have only in recent decades begun to develop materially. The identification of poverty with stagnation in effect confuses a low economic level with a zero rate of change.

The general validity of the thesis of the vicious circle of poverty is refuted by the experience of individuals and groups throughout the world in rising from poverty to prosperity. It is also refuted by the very existence of developed countries, all of which began in an under-developed state, and also by the more recent experience of many under-developed countries, especially in South-East Asia, Africa and Latin America, which have progressed

rapidly. It is also contradicted by the frequent changes in industrial and commercial leadership between different nations, both in general economic attainment and also in the position and performance of particular industries. Indeed, if it applied so generally, there would for centuries past have been only one developed country constantly increasing its lead over the others.

The poverty, stagnation or even retrogression of the under-privileged nations, and the consequent ever-widening economic inequality, are a principal theme of these books.

"The trend is actually towards greater world inequality. It is, in fact, the richer countries that are advancing while the poorer ones, with the large populations, are stagnating or progressing much more slowly . . . for mankind as a whole there has actually been no progress at all."¹

"I have chosen to focus attention on one particular aspect of the international situation, namely the very large and steadily increasing economic inequalities as between developed and under-developed countries . . . these inequalities and their tendency to grow are flagrant realities."²

No evidence is offered for this opinion beyond reference to two articles, one by Dr. H. W. Singer, the other by Professor P. N. Rosenstein-Rodan, which, however, do not provide evidence either. This is not surprising. There are no series of national-income figures for any Asian or African country before the First World War, and only a few hazardous estimates for individual years or periods of the inter-war years. Even if they were available, there would be serious difficulties in interpreting the statistics and even greater difficulties in drawing meaningful comparisons because of fundamental changes in the conditions of existence in many under-developed countries over this period. Neither Dr. Myrdal nor these articles discuss either the national income figures in the developed countries or in the under-developed countries over the relevant period (the last half-century), nor any other information on the absolute or relative position of different countries, nor do they mention the extreme heterogeneity of the under-developed world. The references to the ever-widening inequality, retrogression, stagnation and bottomless misery are *obiter dicta*, based on an axiomatic and inadmissible identification of low per capita incomes with stagnation.³

¹ *International Economy*, pp. 1-2.

² *Cairo Lectures*, p. 7.

³ Dr. Myrdal writes "as Mr. H. W. Singer has rightly pointed out, world real income per capita, and with it the standard of living of the *average* human being, is probably lower than twenty-five years ago, and perhaps lower than in 1900, because the nations that have rapidly raised their economic standards have been a shrinking proportion of the total world population" *International Economy*, p. 2 [my italics]. The relevant passage in Dr. Singer's article reads: "If we define the 'average' world income as that of the median world citizen, the spectacular improvement which has occurred at one extreme and which has fascinated economists and other observers becomes irrelevant" ("Economic Progress in Under-developed Countries," *Social Research*, March 1949, pp. 2 f., quoted in *International Economy*, p. 341). Quite apart from the absence of statistics, these passages rest on an unwarranted use of the concept of the average. The median income

Dr. Myrdal suggests that "the economic level at which they [the under-developed countries] start is in most cases very much lower [than the starting point of developed countries]." ¹ As it stands, the statement eludes precise interpretation, since the historical starting-point for the now developed countries is not indicated. Other contexts in these books refer to the industrial revolution of the eighteenth century as the starting-point for these comparisons. This approach neglects the long period of progress of Western Europe, including Great Britain, before the eighteenth century, a process which extended over many centuries. At times (indeed usually) Dr. Myrdal ignores this long process altogether, while at other times he greatly underestimates its significance. The present under-developed countries, most of which began to develop only recently, cannot be expected to reach the level of developed countries in the space of decades; and there is nothing abnormal or paradoxical or inherently cumulative in the disparity between them and the developed world. There is no prescriptive right or general law that all communities must develop simultaneously and equally.²

While the author stresses cumulative processes, it is odd that the discussion is nevertheless static, because it is so often timeless, in that it ignores the relevance and importance of differences in starting-points on the road of progress, or in the length of period over which certain processes have occurred. Yet consideration of the period of certain processes, and the dating of turning-points, are essential to dynamic treatment.

Dr. Myrdal applies the thesis of increasing inequality to the different regions of the same country in the absence of deliberate government action; this is premised chiefly on differences in capital, transport facilities and education. This again, however, is contradicted by much historical evidence. The unsubsidised development of manufacturing industry in the North of England, away from the established centres of industry and commerce, is only one example. The decline of industry in South Italy following the removal of tariffs with the unification of Italy, is instanced as a particularly well-documented example of the damage to a backward region through unification with a more advanced area. The simple question is not asked how the South has been able to pay for the industrial products of the North. The decline of particular activities previously subsidised by the rest of the region does not in the least indicate that the region as a whole has been affected adversely by a particular change. The removal of tariffs and of other barriers to mobility following the unification of Italy, widened the

could fall even if individual incomes rise everywhere, if population in poorer countries increased much faster than in richer countries.

¹ *Under-developed Regions*, p. 98. This is one of many passages referring to initial differences in wealth and income between developed and under-developed countries without specifying either the starting point or the length of the process.

² The remarks in the text do not imply acceptance of the state of under-development of large parts of the world, nor are they a plea for *laissez-faire* or any other particular type of policy. These are matters quite different from the criticisms of Dr. Myrdal's treatment of the process of development.

range of alternatives open to the people of Southern Italy; and the extension of the range of choice normally benefits rather than harms a population. Besides identifying an historical change with a functional relationship, this treatment identifies one sector in a region with the economy of the region as a whole, that is, it ignores the transfer of resources between sectors. There are also other instances in these books of an unwarranted identification of the performance of one sector with that of the whole economy, which are likely to mislead the unwary reader.

IV

In Dr. Myrdal's view the developed countries have contributed to the plight of the under-developed countries, especially through the effects of colonialism and the operation of the international demonstration effect.

The treatment of the economic results of colonialism seems to me at times ambiguous. On occasions the author implies¹ that it has raised material standards in the colonies. But more often he suggests that it has retarded progress by discouraging wider economic contacts, by promoting economic activities benefiting the metropolitan countries and especially by preventing the pursuit of policies designed to promote the general economic development of the colonies.² Yet politically independent countries in Asia and Africa are among the most backward, usually much more so than the neighbouring colonies. Again, some of the former colonies became prosperous while they were colonies, and this indeed paved the way for their political independence. Further, the developed countries were already far ahead of the under-developed countries before the Industrial Revolution, and the differences had become even more marked by the time large parts of the under-developed world came to be colonised in the nineteenth century. Dr. Myrdal is—I cannot help feeling—prepared to strain language to be able to suggest that extraneous powers are in part responsible for the poverty of the under-developed world. This is exemplified by his frequent use of the term colonial economies to refer to independent sovereign countries whose policies are not controlled by other governments. This usage identifies poverty with colonialism, and thereby empties the latter term of all meaning.

Dr. Myrdal argues that colonialism has prevented the governments of under-developed countries from pursuing deliberate policies of economic development. This argument seems to me to be built on the assumption, explicit in the discussions on policy, and implicit throughout these books, that economic progress is largely a matter of government policy. Whatever is thought about the general validity of this assumption, it is not clear what sort of intervention by efficient independent governments would

¹ For instance, *Under-developed Regions*, p. 56.

² For instance, *Cairo Lectures*, pp. 53–5, 75, 76; *International Economy*, pp. 100–2, 168, 225; *Under-developed Regions*, pp. 57–9, 74.

have accelerated development in eighteenth- and nineteenth-century Africa and Asia. But it is certain that no conscious development policies would have been pursued by the rulers of the remains of the Mogul empire and of its successor states, by the local sultans of South-East Asia, or by African chiefs.

The mere presence of developed countries is thought by Dr. Myrdal to have damaged the under-developed world through the international demonstration effect, which discourages the small amount of saving which might otherwise take place. But is it not the case that now, as in the past, the most advanced of the under-developed regions and sectors are those in contact with developed countries? Contact with more advanced economies may indeed suggest new wants, but these can be satisfied only if incomes are first earned to purchase the consumer goods, and this usually requires saving and investment, invariably so when it requires the replacement of subsistence cultivation by production for exchange. Moreover, these contacts generally suggest new methods and the introduction of new crops, and also provide markets. Thus they induce higher economic performance, especially by encouraging agriculturalists to produce for sale. Dr. Myrdal's treatment appears to ignore the influence of the real reward in the exchange sector and of consumption prospects on the supply of effort and saving. This is surely unwarranted, especially in under-developed countries, where subsistence production is large, and the extension of the exchange sector an important factor in development. These contacts also often result in the import of capital, and in the import of entrepreneurial, technical, and administrative skills, or their acquisition by the local population. There are also other objections to the suggestion that under-developed countries are harmed by contact with more advanced economies. Thus the population of these countries can imitate the saving and investing habits as well as the consumption habits of the more advanced countries. Further, there are generally some rich people in under-developed countries, so that the demonstration effect can operate even in the absence of international contacts.

Dr. Myrdal recognises, albeit reluctantly, the presence of advanced and advancing (in fact often rapidly advancing) countries, regions, sectors and activities in the under-developed world. Following other influential writers, he refers to these as enclaves carved out of the local economies by the advanced countries, as colonial types of economic activity, which do not serve to improve the economic position or prospects of the local population.¹ Thus the thesis of the vicious circle and stagnation appears to remain intact. But the thesis of the alien enclave is no more securely based than the main argument. First, the fact that foreign personnel, enterprise and capital played a large part in the development of these advancing sectors does not mean that the process has not benefited the local population. The suggested contrast or incompatibility of interest is spurious, and ignores the

¹ For instance, *International Economy*, pp. 100-2, 107, 168, 225; and *Cairo Lectures and Under-developed Regions*, *passim*.

complementarity of resources. Again, there has generally been no single centralised force acting in this sphere as a representative or agent of the interests of the developed countries. Moreover, in many instances the progressive sectors are largely, or wholly, owned and operated by members of the local community. For instance, all West African agricultural exports, including such world staples as cocoa, groundnuts and oil-palm products, are produced entirely by Africans on their own lands, and the same applies to the bulk of South-East Asian and South American exports. In all these areas the local population also has a large share in the transport, distribution and simple processing of these exports, and in the distribution of imports. The same holds also for the often appreciable commercial, transport and industrial activity for sale locally or even for export, as in Hong Kong, Puerto Rico, India and Pakistan. But even when the enterprises in the advanced sectors are foreign, they usually still assist development by contributing to government revenues, by spreading skills and generally by promoting the exchange economy.

These sectors are not enclaves cut off from the rest of the economy, but focal points, the points of first impact, of development, a process which always affects certain activities and regions first, from which it spreads to the rest of the economy. The time this requires depends, among other factors, on the qualities of the population, on institutional factors and on physical communications. There is nothing unusual or abnormal in the uneven regional or sectoral incidence of economic progress.¹ There are countless examples in the most highly developed countries.

The practice of regarding the more rapidly advancing regions and sectors (at times even to entire countries) as enclaves has now become general in the literature on under-developed countries. The appeal of this catchphrase seems to stem in part from the spurious reconciliation it offers between the manifest evidence of progress in many under-developed countries and the thesis of the vicious circle of poverty, which is both the cornerstones of the current orthodoxy on development of which Dr. Myrdal is now a leading exponent, and a basis for far-reaching proposals for policy. The sharp and clear-cut contrast between a progressive but alien enclave and the stagnant local economy is also more readily intelligible and more congenial to current intellectual habits than that of a varied economic landscape, of differing levels of attainment and rates of advance. The habitual reference to advancing sectors in under-developed economies as alien enclaves serves to obscure the process by which developed economies made their initial advances, and it is also likely to prevent us from encouraging this process in the contemporary under-developed world.

¹ In this context also Dr. Myrdal seems to regard as reprehensible any difference in rates of development or levels of attainment. Though it is not disputed that in Latin America the advancing sectors are largely locally owned and administered, he notes with disapproval that progress (in fact rapid progress) has been uneven, and that certain areas have advanced much further than others. For instance, *International Economy*, p. 152.

V

In describing the social landscape of the under-developed world, Dr. Myrdal presents a picture of rigid, highly stratified societies, with groups privileged in wealth, power and status lording it over under-privileged masses; and the more backward the country, the wider the inequalities.¹ This deep disintegration is, again, as he pictures it, evidence of circular cumulative causation, since the stagnant and impoverished countries do not have the resources necessary to achieve the integration required for economic progress.

In fact, economic inequality is by no means general in under-developed countries; much of what inequality exists does not reflect privilege; nor is economic equality necessarily evidence or instrument of economic development, or inequality an obstacle to its achievement. Over large parts of the under-developed world, and especially in the most backward areas of Africa, and South-East Asia, the social landscape is not that of wide inequality, but rather one of masses of undifferentiated people. Moreover, some of the most striking examples of inequality do not reflect privilege in the accepted sense of the term. Originally penniless immigrants or their sons are the richest people in South-East Asia, and the wealth of Indians in East Africa and Lebanese in West Africa has been accumulated by poor unprivileged immigrants or their descendants in one or two generations. Throughout the under-developed world there are often rich indigenous merchants, industrialists, executives, professional men and even agriculturalists, who were not beneficiaries of inherited wealth or official favours. These inequalities are thus bound up with differences in economic qualities, such as perception of economic opportunity, industry, thrift, persistence, unrelated to special privileges or institutional or monopolistic restrictions; and, of course, in some cases they result from sheer luck. Such differences in aptitudes are just as real as, say, those in physical appearance. These may or may not be regarded as justification of differences in wealth, but they are certainly not the result of privilege.

Though at times his treatment is ambiguous, Dr. Myrdal seems to suggest generally that people's capacities and qualities are fundamentally equal, and that differences in attainment and prosperity reflect the operation of external factors.² Complex and debatable issues are raised by such questions as the different grounds on which different types of material inequality, or different policies for reducing inequality, may be justified; and

¹ Especially *International Economy*, Chapter XII: cf. *Cairo Lectures*, p. 48: "In a rather close correlation to their poverty they are ridden by internal economic inequalities . . ."

² Thus he writes: "As we know, modern research has in the last half century gone a long way to prove scientifically the correctness of this essential kernel of the naturalistic doctrine of human equality with respect to natural endowments and thereby justified the environmental approach in the social sciences which lays the basis for rational radicalism in social reform." *Under-developed Regions*, p. 108. And later, p. 113, he adds that recent research has confirmed "that as a general rule men are equally endowed by nature."

again the concept of responsibility for one's fortune is not simple. It could, of course, be argued legitimately that those less intelligent, able and enterprising should be comforted and compensated for their disabilities by redistribution. But at any moment of time there are real and wide differences between individuals and groups in relevant economic capacities. These differences are almost certainly not fixed for all time, but they are real and relevant over decades or even centuries.¹ The treatment here reflects, and is likely to reinforce, two related opinions, which are difficult to substantiate but are widely held and influentially advocated. These are that there is no significant difference in economic qualities between individuals and groups, even at any given time or over any given period; and that differences in economic attainment and progress are abnormal and reprehensible, calling for special explanations of their presence and for far-reaching policies for their removal.

Inequality of wealth need not be an obstacle to economic advance. For example, the accumulated wealth of the Chinese immigrants of South-East Asia, which implies a high degree of economic inequality conventionally measured, has obviously not retarded economic advance. It reflects largely the result of the performance of economic services, and the fruits of saving and of re-investment of profits. The prospects of higher incomes and of the possibility of accumulating wealth have attracted enterprising people of various races to South-East Asia, and elicited a supply of productive effort, enterprise, saving and investment.

Moreover, a differentiated social structure may be both a reflection and an instrument of economic development, particularly where social intercourse and mobility are reasonably free. Such a social structure, which emerges only after a long period of social and economic progress, is congenial to further and accelerated development. It conduces to long views in economic decisions, since in such a society people are apt to consider their descendants; and at the same time such a society is compatible with mobility and flexibility, and with a spirit of experimentation and curiosity, which is indeed encouraged by the perception of differences.

VI

The treatment of a number of more specific topics deserves closer examination. They are selected on two grounds. First, they treat of issues

¹ In the general context of material inequality an important problem of measurement may be noted. The meaning of a given measure of inequality is very different if the composition of the different social groups is stable or is changing rapidly. Even a large measure of apparent material inequality is compatible with a high degree of equality over a period of time if the composition of the different groups is very unstable. The composition of, say, the upper 5% of wealth owners and income receivers is likely to change much more rapidly in South-East Asia than, say, in India. This is usually neglected in discussions on inequality, necessarily by Dr. Myrdal, who believes in the well-nigh universal operation of circular cumulative causation. Besides the political implications of its recognition, this would also throw into relief some of the conceptual and statistical problems of inequality and its measurement.

in which the author's discussion is more clear-cut. Second, the selected issues will be recognised as being prominent in current discussions of under-developed countries, both in academic discussion and in proposals for policy.

(a) Dr. Myrdal subscribes to the familiar view that savings are unproductive if the proceeds are used for the purchase of land.¹ But if some people consume less than their incomes, they make available resources to the economy. Their contribution may be offset by the dis-saving of others, but this is unaffected by the nature of the assets bought by the savers, except on peculiar assumptions (which would need to be specified), especially of general unemployment of all types of productive resources which is clearly inapplicable to under-developed countries.

(b) Fluctuations in the export proceeds of primary producing countries are said to lend an inflationary bias to these economies.² In fact, both in Africa and Asia deflationary forces were prominent throughout the inter-war period, as well as in earlier times. The frequent inflations in Latin America over this period can be attributed more properly to a failure to control the supply of money rather than to the composition of their exports. The discussion of this subject seems to me confused. For instance, he argues that an increase in export proceeds sets up inflationary forces. But this does not happen when the supply of imports is elastic.³

(c) Balance-of-payments difficulties are regarded by Dr. Myrdal as chronic and indeed necessary in progressing under-developed economies, especially primary producing countries. This view is widely held at present. Yet it disregards the infrequency, or even absence, of such differences, both in the early history of the developed countries, and also in the comparatively recent history of rapidly developing under-developed economies or primary producing countries, including North America, Australasia, South-East Asia, West Africa, Hong Kong and many others.⁴ Neither in this context, nor in his discussion of the dollar shortage, does Dr. Myrdal consider or even mention the rate of exchange; and he mentions only most perfunctorily the relevance of monetary policy. Again, Dr. Myrdal urges priority in the national plan for exporting and for import-saving activities, without stating that this is economic only if the exchange rate is over-valued in some defined sense. Only then will the proceeds of exports (or the saving in imports) yielded by a given volume of resources and expressed at the official rate of exchange under-state their contribution to the national income compared to other uses. Characteristically, the principal discussion of this range of

¹ *International Economy*, pp. 183, 203.

² Especially *International Economy*, pp. 240-2.

³ With a fixed rate of exchange, an improvement in the terms of trade postulates a rise in local prices and incomes relative to import prices at that rate. But this is simply the monetary counterpart of a rise in real income and is not inflationary.

⁴ "... there must be something wrong with an under-developed country that does not have foreign exchange difficulties." *International Economy*, p. 270.

issues is entitled "The Foreign Exchange Front";¹ once more, the rate of exchange is not mentioned, and the discussion is essentially in terms of physical quantities.

(d) The discussion of the balance-of-payments and of foreign-exchange problems seems to provide an example of what one might term "price-less economics," that is of the treatment of supply and demand as physical quantities unaffected by price. In spite of general references to the value of the price mechanism as an efficient instrument of economic control, Dr. Myrdal frequently ignores the effects of prices and real rewards on supply and demand, or on the extension of the exchange economy, even where they are essentially relevant, notably in fiscal policy.² An example is the reference to the low level of investment in public utilities in Latin America, instanced by Dr. Myrdal as characteristic of unbalanced private investment, without mentioning that statutory rate fixing at uneconomic levels in inflationary periods has removed both the wherewithal and the incentive for it.³

(e) Dr. Myrdal repeatedly refers to the inelastic demands confronting the exports of under-developed countries. But the essential distinction is not drawn clearly between the elasticity of demand for a particular product and the elasticity of foreign demand for that product from a particular country.⁴ The elasticity of demand for a country's exports depends not only on the underlying elasticity of demand for the commodity, but also on the share of this country's exports in total exports of the commodity and the elasticity of supply of the commodity from all other resources. Many primary producers have lost heavily by overlooking the distinction.

(f) In the lengthy discussion of commodity stabilisation,⁵ the cardinal problem of distinguishing between fluctuation and a change in the trend is not discussed. This is fundamental, because neither private individuals nor Civil Servants can tell in advance whether a particular change is a fluctuation or a change in the trend. Moreover, at times Dr. Myrdal uses stabilisation interchangeably with monopolistic raising of prices, which is a familiar and possibly excusable practice of the spokesmen of primary producers, but is not easy to justify in serious discussion.

(g) The author insists that under-developed economies are very rigid.⁶ In support of this proposition he instances their reliance on agriculture in

¹ *International Economy*, pp. 273-5.

² These remarks are not intended to suggest that the price mechanism brings about an ideal allocation of resources. The very concept of a mechanism implies that it is neutral, and what comes out of it depends on what is put into it, which in this context depends among other factors on institutional arrangements, market structure, distribution of wealth, and government policies. Dr. Myrdal is right when he stresses the essential neutrality of the price mechanism, but this does not warrant the frequent neglect of its operation.

³ *International Economy*, pp. 168 and 343. He also notes (p. 64) the absence of foreign investment in recent years in Greece, where capital imports are badly needed. But he does not mention the relevance of the record of Greek governments in defaulting on their external obligations.

⁴ For instance, *Cairo Lectures*, p. 71.

⁵ *International Economy*, pp. 244-53.

⁶ Especially *International Economy*, Chapter XIII.

spite of low incomes and of unfavourable terms of trade. But there is no evidence that alternative forms of activity would have yielded higher returns. Moreover, producers in many under-developed countries respond readily to change, as is shown, for example, by the rapid growth of the output of certain crops, the changes in the relative importance of different types of crop over periods of a few years and the pronounced response of smallholders' production to price changes and differentials in India, Pakistan, South-East Asia and West Africa.

(h) The author suggests that ". . . all history shows that the cheap and often docile labour of under-developed regions does not usually attract industry."¹ This is truistic if it means that labour is not the only factor of production, and untrue (an example of price-less or cost-less economics) if it means that labour costs are irrelevant. Low labour costs have been a major factor in the rapid growth of manufacturing in Japan and Hong Kong; and in many western countries manufacturing interests are protesting against the exports produced by cheap labour.

(i) Dr. Myrdal asserts that all under-developed countries are over-populated, and that regularly (*sic*) about one-third to one-half of their agricultural population could be removed without affecting agricultural output, even without any special labour-saving devices.² This is unsubstantiated, and would be untrue for practically all African countries and many Latin American and some Asian countries.

These are only a few examples out of many; the preceding list could be extended greatly.

VII

The two major value premises of these books, equality of opportunity and of political democracy, are notoriously imprecise, and his discussion does nothing to give them precision. As a result, the chosen instruments of policy, the nature of which is less imprecise, can be used in pursuit of widely different, and indeed contradictory, directions and ends.

At times the discussion suggests that absence of discrimination, and substantial equality of income and wealth, are the essence of equality of opportunity, called integration.³ There is a fundamental difficulty here which needs to be made explicit. People differ widely in their capacities, inclinations and aims, they live in different climatic and geographical conditions, they are brought up by parents of widely different attitude and values, and they are subject to different religious, social and customary obligations. Thus absence of discrimination will not automatically bring about substantial equality of income and wealth. In fact, the two elements in integration, *i.e.*, absence of discrimination and substantial equality

¹ *Under-developed Regions*, p. 31.

² *International Economy*, p. 193.

³ For instance, *International Economy*, Chapters III and XI.

of income and wealth, can be reconciled only if the sources of differentiation are removed by compulsory standardisation. This can be achieved only by the over-riding powers of the state. The exercise of these powers over large areas of social life, or indeed social life as a whole, involves great inequality in power between the governors and the governed; and the wider differences in capacities and circumstances, the more extensive and intensive is the compulsion necessary for the required standardisation. This necessary corollary of the author's interpretation of integration seems to me to be ignored in these books.¹ This is the more surprising because he is largely concerned with world economic integration, and differences in capacities and conditions are very much wider on a global than on a national scale.

Political democracy, the other major value premise, is interpreted in these books as majority rule under universal suffrage.² But this condition is compatible not only with the over-riding of the wishes of a large minority but also with extreme tyranny, as is explicit in the title of J. L. Talmon's *The Origins of Totalitarian Democracy*. This is only one of several reasons why the inequality of power discussed in the preceding paragraph—the necessary corollary of equality of opportunity as generally interpreted in these books—cannot be rendered innocuous by the democratic process.

The national interest, the promotion of which Dr. Myrdal wishes under-developed countries to pursue, is another vague concept which he treats as if it were unambiguous and simple to interpret. Thus he frequently implies a simple cleavage of interest between the country and the rest of the world, or between the local population and foreigners. In fact, the relationship between groups is far more complex, and in the economic sphere it is often more nearly complementary than opposite or rival. He also suggests that in countries in which all are allowed to participate in the political process, the governments and the Civil Servants can both unambiguously discern the national interest, and unlike private individuals, will conform to it. This obscures both the vagueness of the concept and the reality of the possible cleavage of interest between administrators and their constituents.³

¹ Dr. Myrdal frequently states that the unchecked working of market forces leads cumulatively to growing inequalities; indeed he greatly overstates this influence. But he does not recognise that his proposals to deal with this involve more serious inequalities of power.

² For example, *International Economy*, p. 176; *Cairo Lectures*, p. 65; *Under-developed Regions*, p. 83. In one aside he refers to the maintenance of civil rights, but these would be defined by the political majority.

³ There are many other instances in these books of the use of imprecise general terms as if they unambiguously described specific analytical categories, types of situations, or aims of policy. They include usury, exploitation, land reform, economic colonialism, fair and equitable prices, and stabilisation. Again, general rejection of discrimination is meaningless, since all social and human intercourse is based on it, though different people may and do discriminate for different purposes and on the basis of different criteria. Such usage is misleading, and especially unwarranted in a book such as *An International Economy*, for which the author explicitly claims scientific objectivity.

VIII

The main instruments of development policy envisaged by the author are clear. He considers comprehensive development planning, in the sense of government determination and control of the direction of economic activity, including large-scale compulsory saving, as indispensable and presumably sufficient for the increase in output, which is the essence of economic improvement for the masses.¹ However, it is not stated why the particular pattern of productive resources envisaged by this proposal should break the vicious circle or otherwise accelerate economic growth. The discussion simply presumes what requires to be established.

Dr. Myrdal emphasises the benefits, made possible by development planning, of the disregard of market prices and costs, which he regards as fictitious.² He stresses the importance, especially in the context of industrialisation, of external economies, which he regards as a major ground for subsidisation of particular activities and enterprises, or for the establishment of state enterprise. But these measures imply a transfer of resources, and a corresponding diminution of activity and of external economies elsewhere. They promote economy of resources and growth only if the subsidised activities or the state enterprises yield goods or services and external economies exceeding in value those lost through the contraction of the taxed activities, allowing also for the announcement effects of taxation and the administrative cost. In spite of general admonitions on the importance of economy of resources, the author treats the assisted or officially sponsored activities as straightforward additions to total activity, without asking where the resources come from.³ This vitiates much of the discussion of compulsory

¹ For instance he writes: "All special advisers to under-developed countries who have taken the time and trouble to acquaint themselves with the problems, no matter who they are . . . all recommend central planning as the first condition for progress." *International Economy*, p. 201.

"However difficult the task, one thing stands out: the hope for economic development of under-developed countries depends very much on the state's being able to plan and to direct, and even to invest and to produce." *Ibid.*, p. 210.

² "The national plans cannot be made in terms of costs and profits for the individual enterprises; they can, in fact, not be made in terms of the prices in the markets . . . the whole meaning of the national plan is to give such shelter from the market forces to investment, enterprise and production that they become undertaken in spite of the fact that they are not remunerative according to private business calculations."

"It is here that the national state comes in as representing the common and long-term interests of the community at large. It senses the fictitiousness of the private business calculations in terms of costs and profits." *Cairo Lectures*, pp. 66 and 67. It will be noted that the actions of the government are here axiomatically assumed to represent the national interest.

These passages are almost identical with similar passages in *Under-developed Regions*, pp. 86-7.

³ Dr. Myrdal frequently gives the impression that he wants the state to increase the supply of almost everything without indicating the provenance of the resources. To quote two instances:

"If, as is assumed to be an urgent necessity in the under-developed countries, the movement towards industrialisation is to be pushed ahead, the state will have to intervene in the field of manufacturing industry too [as well as in agriculture, transport and public utilities] not only creating the external economies and supplying transport and power, but often also organising the marketing of the produce of the expanding industrial sector, providing facilities for training

saving for development. Moreover, a policy of compulsory saving cannot be framed sensibly if the repercussions on the private sector of both the collection and the expenditure of the funds are ignored, and if it is conceived in a framework of price-less economics.

The insistence on the rejection of market prices and costs goes much beyond the recognition of differences between money and real costs, or the importance of external economies, or the emphasis on growth compared to the allocation of resources. The author generally regards individual choice and valuation as irrelevant: the decisions of planning governments are the *criteria* as well as the *instruments* of development. For instance he writes:

"The plan and its targets, in other words, have to render the basis for deciding the criteria; the decisions themselves are political decisions reached in terms of national welfare, as this is determined by the political process."¹

On this argument again the actions of the government by definition accord with the national interest. Moreover, the criteria of development need not be related to peoples' wants, and can be set in terms of the growth of output deemed important by the government, or in terms of the output of particular industries or sectors only. Development so defined or measured can be wholly unrelated to living standards, especially those of the masses.² Both in the discussion of more specific economic issues and of broader social and political issues, Dr. Myrdal largely ignores the wishes of individuals and groups other than those of government, their executives and of political spokesmen.

IX

Although he is not sanguine about its early prospects, Dr. Myrdal envisages substantial international redistribution by means of inter-governmental transfers. He notes that an irreversible step in this direction has already been taken under United Nations auspices, especially by the Technical Assistance Programme. He regards such transfers as both moral duty

workers, foremen, and technicians on all levels, as well as business executives, giving managerial advice, making capital available, often subsidising or protecting new industrial enterprises, and sometimes actually establishing and operating them." *International Economy*, p. 209.

"The plan must determine this overall amount [investment] and must, in addition, determine the proportions of the capital to allocated in different directions: to increasing overall facilities in transport and power production; to constructing new plants and acquiring the machinery for heavy industries, and for light industries of various types; to raising the level of productivity in agriculture by long-run investment in irrigation schemes and short-term investment in tools, machinery and fertilisers; to improving the levels of health, education and training of the working people, and so on." *Under-developed Regions*, p. 81.

¹ *Cairo Lectures*, p. 73.

² A specific example of this attitude, which pervades these books, may be mentioned. Dr. Myrdal enjoins the developed countries to abstain from forcing the under-developed countries to accept imports which they do not want. *International Economy*, p. 292. But if the consumers did not want them the commodities would not be imported. There is here (as in many other places) an unwarranted axiomatic identification of the government with the population.

and political wisdom on the part of the developed countries; and he argues that they should take place through international agencies to assure their allocation in accordance with rational economic and moral criteria, rather than on political grounds. This exposition again misses the central problems:

(a) The transfer of such funds is essentially a political matter, because they represent resources compulsorily collected from the citizens of the transferor country. The funds are not the result of voluntary acts by individuals impoverishing themselves in order to assist their less fortunate fellow men. They can thus be reasonably required to be used to promote the interest of the donor countries.

(b) Dr. Myrdal implies that there is a moral obligation on the governments of advanced countries to tax their citizens for the benefit of the governments of the under-developed countries, regardless of the use of the funds and of the policies pursued, and regardless also of the attitudes, qualities, efforts and institutions of the recipients. Even if these policies and influences are wholly unsuitable to economic development, the moral obligations of the developed countries apparently remain unaffected.

(c) It is regarded as self-evident that no conditions should be attached to inter-governmental grants or loans. This is not obvious, since they represent funds in urgent demand throughout the world. Amounts made available to one country diminish those available to others. Moreover, the policies pursued by the recipients are among the factors (indeed the principal factor, in Dr. Myrdal's scheme of things) affecting their rate of development, and thus their need for further aid. Thus the policies pursued by the recipients are clearly relevant to eligibility for assistance.

(d) Though emphasising their importance, Dr. Myrdal nowhere specifies the moral and economic criteria in the allocation of funds. Should they go to the poorest countries, or those poorest in natural resources, or those which have progressed least or most over a specified period, or should some other tests be adopted from among dozens of possible criteria?

(e) The exposition assumes that the international agencies and their Civil Servants are completely disinterested. In fact, they have considerable interest in terms of prestige, position and power. Dr. Myrdal expressly refers to the need to keep technical assistance agencies free from the influence of private business interests. He does not recognise, or at least does not admit, the reality of the interests of those in charge of the agencies.

(f) Inter-governmental transfers affected through the international agencies are likely to be wasteful. The interest of the suppliers of

capital is generally a major factor assuring its productive use. It is for this reason that direct investment, especially by individuals or partnerships, in which there is close correspondence between the supplier of the capital and its user, is least likely to be wasteful. At the other end of the spectrum are the inter-governmental transfers through the international agencies under which the suppliers have little knowledge and no control of the use of the funds.

X

These books are, as I have shown, concerned with the promotion of policy rather than with the promotion of knowledge. It is not easy to reconcile either their argument or their presentation with the author's explicit objective, already noted, of getting "the facts straight and think things out stringently," or with his advice that an international agency should act as "a free and independent scientific agent, which approaches the problems and reaches and states its findings guided only by the inherited and established standards of the profession, without sideward glances at what would be politically opportune."¹

Although the books abound in sympathetic references to the plight of the under-developed countries, there is nevertheless a distinct ambiguity about the beneficiaries of Dr. Myrdal's proposals. He writes:

"Not merely to save the world, but primarily to save our own souls, there should again be dreamers, planners, and fighters, in the midst of our nations, who would take upon themselves the important social function in democracy of raising our sights—so far ahead that their proponents again form a definite minority in their nations and avoid the unbearable discomfort for reformers of a climate of substantial agreement."²

But the price to be paid by others may be too high for the salvation of even the most ardent souls, if inter-personal comparisons of utility and cost may be permitted for the moment.

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¹ Quoted from "Economic Essays in Honour of Erik Lindahl," *ECONOMIC JOURNAL*, March 1958, p. 119.

² *International Economy*, p. 322. This attitude is the opposite of the scientific which seeks to discern uniformities and to reach agreed generalisations.

REVIEWS

A Critique of Welfare Economics. Second Edition. By I. M. D. LITTLE.
(London: Oxford University Press, 1957. Pp. viii + 302. 30s.)

NORMALLY a reviewer's duty is to tell the reader what the book under review contains, and only after that is done to criticise those contents. But in reviewing this book the normal rule does not apply; for as the result of a series of unfortunate accidents the first edition of this famous book, which appeared as long ago as 1950, was never reviewed in the *ECONOMIC JOURNAL*. It would indeed be a waste of time to offer at this late stage to any economist who has any interest in the theory of economic welfare a brief sketch of what Mr. Little has said on the subject. The author has already through this book established his position as one of the few outstanding contributors to the subject, and his contribution is already incorporated into the thought of all those who study these problems.

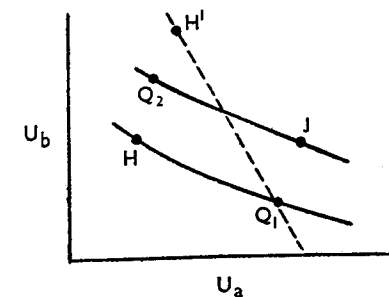
This book has indeed had a significant influence on economic thought. In the first place, the present fairly widespread realisation among economists of many basic difficulties involved in deciding whether a community is in any meaningful sense better off economically in one situation than another is largely due to Mr. Little's work. If consumers are not consistent in their choices, how can we say whether they are better off in the one situation rather than the other? If the community is made up of one set of individuals in one situation and another set in the other situation, in what sense can we say whether the community as a whole is better or worse off? If people's tastes and wants are affected by what other people consume, how can one tell whether they are better off in one situation rather than in another? If we cannot assume that all individuals know what is good for them, how can we construct a criterion for an increase in economic welfare in a non-authoritarian society? It has been much less easy to neglect questions of this kind since Mr. Little's book was first published.

There is a second way in which Mr. Little's work has radically changed the atmosphere in which welfare economics are discussed. By insisting as he did on the emotive and prescriptive nature of many judgments which in form are innocent statements of objective fact he has significantly increased the chances that in economic discussion disputes about the values attached to certain ends will be less easily muddled up with disputes about the objective economic effects of certain acts of policy. Certainly the present reviewer is not ashamed to admit that Mr. Little's book acted on him as a salutary reminder that he ought to avoid a misuse of persuasive language in making what purported to be judgments of fact and not judgments of value.

Thirdly, there is a basic element of balanced common sense in Mr. Little's

work which has done much to bring sanity into discussions about economic welfare. Having shown that it is impossible to use any exact and precise criterion of economic welfare except on simplifying assumptions that are certainly in many respects unrealistic, he does not rush to the conclusion that the whole analysis is a mere waste of time. For example, suppose that we can show that there would have been a very large increase in economic welfare if all consumers were consistent in their choices and if no new citizens had been born or old citizens had died between the two situations under consideration; in such conditions Mr. Little is prepared to say that the presentative citizen is in some meaningful sense probably better off, even though there have been some intervening shifts in tastes and some intervening births and deaths, but that we can attach importance to such a criterion only if it shows a really substantial change. This is certainly a healthy attitude; the criterion of economic welfare can be a useful pointer, but not an exact and precise measuring rod.

But the actual criterion of an increase in economic welfare which Mr. Little adopts cannot, in the reviewer's opinion, be regarded as acceptable. The choice of this criterion is the central technical economic issue in Mr. Little's book, and as Mr. Little has made more substantial changes in his second edition in this part of his work than in any other, the matter deserves closer attention. Let us state Mr. Little's criterion in his own words (p. 275): "an economic change is desirable if (a) it would result in a good redistribution of wealth and if (b) the potential losers could not profitably bribe the potential gainers to oppose the change." And let us illustrate this argument by the sort of diagram which Mr. Little himself uses in his book (Fig. VII on p. 104):



We consider a community made up of two individuals A and B, and we measure some ordinal index of A's utility along the horizontal, and of B's utility along the vertical, axis. We start at a position Q_1 , and some change of economic circumstances or of policy moves us to the situation Q_2 . Does this change, which has made A worse off and B better off, represent an increase or decrease in economic welfare? Let us suppose that we prefer the sort of distribution of real income between A and B which is represented by Q_2 to that represented by Q_1 . Suppose, then, that starting from Q_1 , we made a

simple redistribution of income from A to B by lump-sum transfers of money income from A to B, thus moving along a line such as Q_1H . (Little makes it clear that we need not assume that such a purely redistributive policy is at all practicable; we use the line Q_1H and the hypothetical point H merely as an indirect means for comparing actual Q_2 with actual Q_1 .) Now if the sort of distribution of real income represented by Q_2 is better than that represented by Q_1 , then H is better than Q_1 , because H is Q_1 modified *solely* by a straight lump-sum redistribution of purchasing power to make Q_1 more like Q_2 . But Q_2 is better than H because at Q_2 both A and B are better off than at H . *A fortiori*, therefore, Q_2 is better than Q_1 . The first part (a) of Mr. Little's criterion for an improvement in economic welfare is satisfied because a distribution like Q_2 and H is better than a distribution like Q_1 ; and the second part (b) of his criterion is satisfied because the line Q_1H passes to the left of Q_2 , which means that if, starting from Q_1 , A gave to B sufficient money income to make B as well off without the change of economic policy as B would be with the change of policy at Q_2 , then A would be worse off than he would be at Q_2 .

The first comment to be made on this is that Mr. Little's criterion as expressed in the passage quoted above is unduly restrictive. The second part (b) of it should read "and if (b) *either* the potential losers could not profitably bribe the potential gainers to oppose the change *or* the potential gainers could compensate the potential losers to accept the change." Suppose that the line Q_1H passes to the right of Q_2 , as in the case of Q_1H' in the diagram above; then the criterion as expressed in the passage quoted from Mr. Little's book does not tell us whether the change is an improvement or not; we know only that H' is better than Q_1 and Q_2 . But suppose now that having reached Q_2 we make notional lump-sum transfers from B to A, thus moving along the line Q_2J and suppose that, as drawn in the diagram above, the line Q_2J passes above Q_1 . This means that B (the gainer from the move to Q_2) could make a lump-sum payment to A of a kind which would leave A as well off as at Q_1 , with B remaining better off than at Q_1 . We can now say that a point like J is better than Q_1 , since both A and B are better off at J than at Q_1 ; and we can add that Q_2 is better than J , because Q_2 and J differ solely in the distribution of income and the kind of distribution at Q_2 is better than that at J ; therefore, *a fortiori*, Q_2 is better than Q_1 .

Mr. Little has in fact in the second edition of his book much improved the assumptions and analysis of this basic part of his argument. This part of his analysis is now based on the assumption that no lump-sum redistributions are going to be made so that H and J are purely notional reference points to help in making a comparison between the two points of actual choice Q_1 and Q_2 . If we compare the table on p. 105 of his second edition with the similar table on p. 103 of his first edition we find that the analysis now tells us that the change should be made if the distribution of income is improved and *either* the Kaldor-Hicks *or* the Scitovsky criterion is satisfied, whereas formerly we

were told that the change should be made only if, the distribution of income being improved, the Scitovsky criterion is satisfied. But this further enlargement of the criterion has not yet been absorbed into some other parts of Little's book.

Let us accept this enlargement of Mr. Little's criterion. There appears, however, to the present reviewer to be a flaw in the whole criterion. Let us consider once more the basic argument with the aid of the diagram above. A central feature of Mr. Little's analysis is the following type of argument: by a lump-sum transfer of money income we move from Q_1 to H ; we prefer distributions of a kind like Q_2 to those of a kind like Q_1 ; therefore, H is better than Q_1 , being Q_1 modified solely by a lump-sum redistribution which makes it more like Q_2 . This argument is a *non sequitur*. Let us remember that Q_1 is not assumed to be a point at which all the necessary marginal conditions for a Paretian optimum are satisfied. There may be all sorts of inefficiencies and divergences between marginal values and marginal costs throughout the economy. We then make a lump-sum transfer of money income from A to B. In one institutional and economic set-up we may reach H , if, for example, the goods which A ceases to buy are produced under conditions of increasing returns to scale and in a sector of the economy where there was a minimum of economic inefficiency, while the new goods which B buys are produced in conditions in which a limited specialised factor plays an important role, without any economies of scale, and in a sector of the economy where there are all sorts of institutional and other inefficiencies. In the opposite conditions we might move from Q_1 to H' . Whether or not there is any sense in the unqualified statement that in general one prefers distributions of the kind Q_2 to those of the kind Q_1 is not clear to the present reviewer; but it is clear to him that one might well say that one prefers H' to Q_1 (if a lump-sum transfer from A to B would make B much better off but A only a little worse off) while one prefers Q_1 to H (if the same lump-sum transfer would make B only slightly better off at the cost of making A much worse off). If we are going to have a notional line to help us to make a comparison between the actual points Q_2 and Q_1 we cannot use the line Q_1H , for the effect of a lump-sum transfer of income in fact depends upon the actual institutional and other economic conditions for production and exchange which are irrelevant to the present issue. We should have to use a line which plotted out the combinations of u_a and u_b between which we as priest or parliament or dictator (pp. 80-1 of Mr. Little's book) or as superman (Chapter VII of Mr. Little's book) or as policy-maker (as the reviewer has elsewhere expressed it) are indifferent. We cannot avoid the social-welfare function which Mr. Little criticises so severely in Chapter VII of his book. In crude and inaccurate language, we cannot separate the considerations of the "distribution" and the "size" of real income, but must weight the two together in a single one-part criterion.

Mr. Little in his book (cases 2 and 6 in Table II on p. 105) already

admits that there are cases in which his criterion cannot tell whether a change should be made or not. These are the cases where there is an unequivocal increase in real income (in the sense that Q_2 is north-east of H and J is north-east of Q_1) combined with an undesirable change in the distribution of income, or—conversely—in which there is an unequivocal reduction in real income combined with a desirable redistribution of real income. So long as a criterion is used which applies a “distribution” test and a “size” test separately, cases of this kind are bound to arise. And in real life they may be of the greatest importance. Thus there may well be a conflict in society between equality and incentives; less-progressive taxation might well sharpen incentives so that by any reasonable measure real income increased while the distribution of income changed for (what the reviewer would regard as) the worse. The policy-maker must in such cases decide in some sense how much increase in real income is worth how much worsening of distribution. If the present criticisms of Little’s criterion are accepted, this problem must in fact be faced in all, and not only in some, cases in which a policy change makes some people better off and others worse off.

If, however, we are prepared to use a social-welfare function but also to be somewhat more modest in the questions which we address to it, we can make some progress towards the solution of this problem beyond the rather obvious and sterile conclusion that we must simply decide whether we prefer Q_2 to Q_1 . Suppose that there are only two citizens A and B and only two goods X and Y , x_a representing the amount of X consumed by A and so on. Then we can write our social-welfare function as:

$$W = \{u_a(x_a, y_a), u_b(x_b, y_b)\}$$

Suppose, for example, that the change from Q_1 to Q_2 represents the change due to the introduction of a policy of freer trade in a country with a high uniform *ad valorem* duty on all imports. We no longer ask whether complete free-trade would be better than the present position; we content ourselves with the much more modest question whether a small reduction in the rate of import duty would make things better or worse. Let T be the rate of import duty. Then a small reduction in the rate of duty would improve matters if

$$\left(\frac{dW}{dT}\right) = \frac{\partial W}{\partial u_a} \cdot \frac{\partial u_a}{\partial x_a} \cdot \frac{dx_a}{dT} + \frac{\partial W}{\partial u_a} \cdot \frac{\partial u_a}{\partial y_a} \cdot \frac{dy_a}{dT} + \frac{\partial W}{\partial u_b} \cdot \frac{\partial u_b}{\partial x_b} \cdot \frac{dx_b}{dT} + \frac{\partial W}{\partial u_b} \cdot \frac{\partial u_b}{\partial y_b} \cdot \frac{dy_b}{dT} < 0$$

Now it is the first task of positive non-welfare economics to make the best possible estimate of the values of $\frac{dx_a}{dT}$, etc.; we must first know what will in fact be the significant effects of the tariff reduction on outputs and consumptions.

There remain the welfare weights $\left(\frac{\partial W}{\partial u_a} \cdot \frac{\partial u_a}{\partial x_a}, \text{etc.}\right)$ to give to these changes.

If citizen A is maximising his own ordinal utility index, then

$$\frac{1}{\rho_{x_a}} \cdot \frac{\partial u_a}{\partial x_a} = \frac{1}{\rho_{y_a}} \cdot \frac{\partial u_a}{\partial y_a} \equiv \mu_a$$

where ρ_{x_a} represents the money marginal revenue obtained by A from x_a if he is selling X or the marginal money charge incurred by A from buying another unit of X if he is buying X (and similarly for ρ_{y_a}), and where μ_a is the marginal utility of money to A measured in the arbitrary units of his ordinal utility index. In other words, A buys and sells what he does buy and sell until the marginal utility of 1s. to him is the same in his net outlays on everything he buys and in his net revenues from everything he sells. The evaluation of the ρ 's is the second great task of positive economics; we must know the market valuations which everyone puts on everything.

We can now write our marginal welfare criterion as

$$\rho_{x_a} \frac{dx_a}{dT} + \rho_{y_a} \frac{dy_a}{dT} + \frac{\omega_b}{\omega_a} \left(\rho_{x_b} \frac{dx_b}{dT} + \rho_{y_b} \frac{dy_b}{dT} \right) < 0$$

where $\omega_b = \frac{\partial W}{\partial u_a} \cdot \mu_a$ and $\omega_b = \frac{\partial W}{\partial u_b} \cdot \mu_b$.

It is now the task of the welfare policy-makers to allot the social welfare weights ω_a and ω_b . In other words, someone must decide whether in present circumstances we should give the same importance to an additional £1 of income to A as to B $\left(\frac{\omega_b}{\omega_a} = 1\right)$ or should give twice as great an importance to an additional £1 of income to A as to B $\left(\frac{\omega_b}{\omega_a} = \frac{1}{2}\right)$.

A special case of the above type of analysis is the old-fashioned welfare function in which the total utility of society is the sum of the cardinal utilities of A and of B ($W = U_a + U_b$). In this case $\omega_a = \mu_a$ and $\omega_b = \mu_b$; or, in other words, the social-welfare weights are simply the marginal utilities of money to A and to B. It was more or less in terms of this kind that the present reviewer analysed the problem in his *Trade and Welfare* referring to μ_a and μ_b as “distributional weights.” But, as the above analysis shows, the method of analysis can be extended to cover the case of a general social-welfare function, requiring from the policy-maker only the allotment of social-welfare weights to increments of money income for different classes in the community. Although such a method will deal only with marginal, and not with structural, problems, it would seem to the reviewer to be, in many cases, of greater promise for practical use than the more general structural analysis of Mr. Little’s book, which, if the above criticisms are accepted, boils down almost to the trivial conclusion that we must decide whether we prefer Q_2 or Q_1 .

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Monetary and Fiscal Policy under Uncertainty. By KARL-OLOF FAXÉN. (Stockholm: Almqvist and Wiksell, 1957. Pp. 212.)

THE depth and brilliance of the Swedish contribution to economics, from Wicksell and Davidson onwards, has been out of all proportion to the numbers of the Swedish nation. A form of economic analysis in which *time*, in at least two of its manifold aspects, lies at the heart of things, is their especial achievement. The imagined unfolding of events through future time we call expectation. In this interior mental exercise the individual is constrained by his sense of the possible, but by no means limited to a single track, still less is he guaranteed by any law of nature to hit on the one right track. He has to take decisions in face of uncertainty. The same is true of everyone, and so we have an economy composed of individuals, every one of whom must base his own plans on the unknowable reactions to those plans of all the other decision-makers. Seen in this light, it is no wonder that the face of the economic world has scared economists into taking, for their main advance, a somewhat easier line, and into assuming time away. The static analysis has had an almost miraculous success in securing valuable insight while throwing away so great a part of the apparent problem; it is surely one of the supreme examples in science of a successful intellectual short-cut. But it did not account for everything, notably the business cycle, and Wicksell and his many heirs were dissatisfied.

One problem of *dynamic* economics is to find a scheme of thought by which we can sort out the sequence in which one set of circumstances leads to another, through the disappointed expectations and the revised plans of policy makers in firms, governments and households. The other problem perhaps even more difficult, is to devise a means of insight into the inner nature of the decision-making process itself, to show what uncertainty is and how men sublimate it into incentives for action. Between these two problems there lies, like the safety-lanes in a motor-racing circuit another escape route: if the second problem seems too arduous, one can drive off the track after having covered the first part, the business of planning with expectations which, while not self-justifying, are treated by the individual as free from uncertainty. It is to the first problem that the Swedish economists, especially in the greatest days of the "Stockholm School" in the late twenties and the thirties, have chiefly made their contribution in the form of *sequence* or *period* analysis.

What Dr. Faxén has done in this admirable book is to carry forward the conception of sequence analysis by adapting to it the notion of "strategies" and replacing the idea of expectations held with certainty by that of expectations weighted with probabilities. These two concepts are combined into that of "partition trees," diagrams which display, first, the action-alternatives immediately open to a decision-maker; next, for each of these alternatives, his subjective picture of the bundle of various outcomes, each with its

probability adjudged to it by him, which could follow that immediate action; next, the further range of alternative actions which *each* outcome of *each* immediate action would offer him; the alternative consequences, each again with its probability, of each of those further actions, and so on up to some horizon; and finally, the "pay off" or summed-up valuation of each separate path of development, allowance made for the successive probabilities to be applied at each stage; since, of course, the decision upon the immediate course of action must be *based on* the picture of the alternative variously probable futures which this course of action or that could lead to, but it must be *taken* in the present, while all those alternative paths are still only probable or improbable in subjectively assigned degrees.

Such is the apparatus that Dr. Faxén has here described and explained with scholarly exactness, detail and care. Its nature and usefulness are, I would say, a little different from what he supposes. Whereas he presents it to us as a manual showing how Governments can succeed in a certain kind of game, it is in fact closer to an elaborate statement of the rules of the game: to understand *what sort* of insight the great chess-master must have is not the same thing as to be able to implant that insight itself into the mind of the learner.

Dr. Faxén's book lies centrally in the great tradition of the Stockholm school. That school has its own special, admirable and valuable *pietas*, whereby the method of advance is to examine as it were *from within* the body of doctrine, the theories and constructions devised by the illustrious predecessors and to modify, develop and evolve those models by a process of "immanent criticism." This is how Lindahl and Myrdal have worked in relation to Wicksell, and it is in full conformity with this idea that Dr. Faxén has sought to increase the analytical power and range of sequence-analysis. The sustained concentration of thought which the writing of this book has required, in synthesising the necessary analytical tools and applying them in many parts of the great field of the study of government action, and in explaining the methods and results of this analysis with patient, step-by-step exactness, will be evident to every economic theoretician who reads it. Only in one respect may the reader feel that the book does not quite do what its title implies. Uncertainty is present only as a skeleton which occasionally rattles the cupboard doors, but Dr. Faxén has not allowed these manifestations to shake his nerve, which is amply strong enough to let him lock the cupboard with some rather familiar-looking keys.

This book is of top intellectual quality, and fully worthy to be Number One in the New Series of *Stockholm Economic Studies*.

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The Falling Rate of Profit: Marx's Law and its Significance to Twentieth-century Capitalism. By JOSEPH M. GILLMAN. (London: Dennis Dobson, 1957. Pp. xi + 172. 25s.)

FOR almost two centuries a spectre has haunted economics—the spectre of a “law of the falling tendency of the rate of profit.” Among all the different versions of this law which have been put forward, that of Marx has roused political passions to the greatest extent, and for this reason if for no other has been the most hotly debated. Was Marx's law a mere rationalisation of contemporary obsessions with the stationary state, or did it succeed in explaining a real tendency observable in the development of capitalism? How far, if at all, did Marx's treatment of the problem contain insights which can help us in the analysis of present-day trends? These are essentially the problems which Dr. Gillman tries to tackle in this honest and stimulating book.

Marx's problem was to examine the effect upon the rate of profit of a secular increase in capital accumulation and productivity. The main point which he emphasised was that accumulation would normally be associated with an increase in the organic composition of capital (that is, roughly, the ratio of the amount spent on machinery, buildings and raw materials to the amount spent on wages). Since according to his basic theory it was only the amount spent on wages which reproduced more than its own value, it followed that the increasing organic composition of capital would exercise a strong downward pull on the rate of profit. At the same time, however, the heightened productivity associated with the increasing organic composition would tend to cheapen wage-goods (thus raising profit per man employed), and also to cheapen capital goods (thus retarding the rise in organic composition in value terms), so that “the same causes which bring about a tendency of the rate of profit to fall also check the realisation of this tendency” (*Capital*, Vol. III, p. 277). Marx evidently believed, however, that the effect of these (and other) offsetting factors would not be sufficiently strong to prevent the rate of profit from falling, at any rate in the long run.

Controversy over Marx's law has followed two main lines. First, it has been argued that the prevalence of capital-saving innovations in our own time has checked or even reversed the former tendency for the organic composition of capital to increase in value terms. This is a relevant and important argument, the validity of which Dr. Gillman's statistical researches tend to confirm. Second, it has been argued that Marx based his law on the assumption that the rate of surplus value (roughly, profit per man employed) remained constant as organic composition increased. This assumption, it is argued, is quite illegitimate, since the increase in organic composition will necessarily be associated with an increase in productivity in the wage-goods industries, a fall in the unit value of wage-goods, and

therefore a rise in the rate of surplus value. It seems to me that this second argument is something of a red herring, since Marx in effect dropped his preliminary assumption of a constant rate of surplus value after the first page or two of his exposition, and discussed at length the reasons why in his opinion the rising rate of surplus value which went hand in hand with a rising organic composition of capital could offset the effect of the latter upon the rate of profit only within “certain impassable limits” (*Capital*, Vol. III, p. 290). Dr. Gillman, however, like almost everyone else, accepts the notion that Marx assumed a constant rate of surplus value, and puts himself to a considerable amount of unnecessary trouble to show that this assumption is “generally valid on both theoretical and historical grounds” (p. 145).

In his attempts at the empirical verification of Marx's law, Dr. Gillman has to deal with somewhat intractable statistical material, and his results, as he himself recognises, have to be viewed with great caution. Broadly speaking, he concludes that the rate of profit, when worked out on the basis of Marx's original formula, tended to fall until about 1919, but that since 1919 it has tended if anything to rise. Does this mean, then, that Marx was wrong? No, says Dr. Gillman: it merely means that the traditional Marxist *formula* is too rigid to encompass the effects of certain new technological and organisational forms which now have a much greater influence on the rate of profit than they did in Marx's time. The terms of Marx's law must therefore be “redefined” so as to take full account of these new forms (p. 61).

The way in which Dr. Gillman goes about this task of “redefinition” is as follows: In his calculations up to this point, he has taken surplus value (s) to be equivalent to value added — (wages of production workers + depreciation), and has worked out the rate of profit by relating s to the stock of capital (c). Now he changes the basis of his calculations, reducing s by a factor u , which stands for “the salaries and wages of the unproductive workers and for all the sales, advertising and all other administrative expenditures as well as for taxes” (p. 89), and works out the rate of profit by relating $s - u$ to c . The figures computed on this basis, he claims, show a decline in the rate of profit during the period since 1919. He accordingly proposes “to reformulate the law of the falling rate of profit for the monopoly period as the tendency of $\frac{s - u}{c}$ to fall” (pp. 89–90).

In the pre-monopoly period, the operative factor in causing $\frac{s}{c}$ to fall was the rise in organic composition; in the monopoly period, the operative factor in causing $\frac{s - u}{c}$ to fall is the tendency for u -expenditures to eat further and further into s .

This is an interesting method of approach, but there are two comments which have to be made about it. First, the downward trend displayed by

the figures which Dr. Gillman calculates for $\frac{s-u}{c}$ is not really substantial enough to justify him in talking about a "general declining tendency" (p. 101) of the rate of profit in the post-1919 period, especially in view of the fact that his continuous series do not go beyond 1939. Second, even if a "general declining tendency" *could* be statistically established, so that it became plausible to speak in terms of the operation of Dr. Gillman's "law of the falling $\frac{s-u}{c}$," it seems doubtful whether it would be proper to describe this law as a "reformulation" or "redefinition" of the original law put forward by Marx. Dr. Gillman, it is true, does so describe it; but what he in actual fact does is to relegate Marx's original law, with its emphasis on the leading role of the increasing organic composition of capital, to the pre-1919 period, and, so far as the post-1919 period is concerned, to use the basic Marxist ratios merely as a sort of conceptual framework within which to examine the effect upon the rate of profit of certain new phenomena which Marx did not consider at all. This may well be all that *can* be done; but Dr. Gillman's attempt to link the two laws together is not entirely convincing, and seems to me to be up to a point otiose.

Dr. Gillman's book is interesting not only in itself, but also because it is a reflection of a new and important trend in Marxist economics. After a long period during which "vulgar" Marxism tended to predominate, a new period is now opening up in which many Marxists are beginning to re-examine the foundations of their theory and to test their theoretical conclusions against the new economic facts which have emerged since Marx and Lenin wrote. Non-Marxist economists, with the experience of a rather similar awakening only twenty years behind them, should surely look with understanding at the efforts of those like Dr. Gillman who are trying to put their Marxist house in order.

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The Design of Development. By JAN TINBERGEN. (Baltimore: The Johns Hopkins Press for the Economic Development Institute (International Bank for Reconstruction and Development) (London: Oxford University Press), 1958. Pp. viii + 99. 20s.)

Studies in Economic Development. By ALFRED BONNÉ. (London: Routledge & Kegan Paul, 1957. Pp. x + 294. 32s.)

Economic Development. By G. M. MEIER and R. E. BALDWIN. (New York: John Wiley (London: Chapman & Hall), 1957. Pp. xix + 588. 68s.)

Poverty and Capital Development in India. By D. K. RANGNEKAR. (London: Oxford University Press for the Royal Institute of International Affairs, 1958. Pp. xii + 316. 42s.)

THE gap between the standard of living of the industrially advanced countries and that of the "underdeveloped" economies cannot be wider

than the gap between the literature on growth theory and the actual planning practices for economic growth. Professor Tinbergen attempts to bridge this gap in his short book, where development policies and techniques of planning are discussed in the light of economic theory. After a discussion of the objects of development policy, Professor Tinbergen proceeds to point out the essential features of programming, from the preliminary macro-economic outline to the detailed micro-economic programme. This section is followed by a study of some methods of appraising public investment projects and a discussion of the "methods by which to explore, appraise and stimulate private investment." There are four annexes dealing with specific questions such as the use of capital coefficients and that of accounting prices.

Professor Tinbergen points out that in underdeveloped economies there are "fundamental disequilibria" in some markets and the market prices do not represent very much. Professor Tinbergen, therefore, calculates the contributions of investment projects to national product (or consumption) at "accounting prices" defined as the equilibrium prices of these markets obtained through "shadow programming." In reviewing this method of project evaluation one cannot escape the feeling that Professor Tinbergen is making a very difficult problem look a good deal simpler than in fact it is. Even if one leaves out the practical difficulties of obtaining these equilibrium prices through any kind of "shadow programming" in countries very poor in statistics, one cannot ignore the complex political and social questions involved in pricing decisions which market equilibrium may not take into account. The equilibrium prices of factors of production, which Professor Tinbergen calls "true values" or "intrinsic values," may not after all be of immense significance once one introduces income inequality, external effects, "irrationality" of savers and other perplexing questions of relative values. It is of course inevitable that in actual planning operations the problem must be simplified, but it does not appear that using market equilibrium prices will be the most economic or the most useful method of doing this. In fact, at one point Professor Tinbergen comes very close to an impossible situation. In some markets, he admits, there may be no equilibrium price at all (for example, "capital may not be sufficient to employ all unskilled labor, even at zero price," p. 83). Professor Tinbergen abandons the method in these markets and suggests a zero accounting price; he adds, however, "in all other markets the flexible equilibrium price is taken to represent the accounting price."

In spite of containing this element of over-simplified economic theory, *The Design of Development* is, it must be admitted, a very useful book. Professor Tinbergen draws our attention to some very important questions of planning methods involving both the internal consistency and the priorities of the design of development. His clear study of the essence of programming in Chapter II is particularly instructive. One may also add that the section entitled "Lessons To Be Learned from Failures" (pp. 67-9) should prove very useful to the planning authorities of some countries.

While Professor Tinbergen addresses economic planners, Professor Bonné's book is written for all economists interested in the problem of development. He discusses the existing economic conditions in many underdeveloped countries, particularly India and West Asia, and then studies the possibilities of a change in the conditions described. In doing this he compares and critically examines alternative theories of development and puts forward his "suggestions for a new theory." Professor Bonné points out that in contrast to the spontaneity of economic development observed during the industrial revolution in the West, an "implanted mechanism" is needed to initiate growth in underdeveloped countries. This is because the main factors responsible for Western economic development "do not obtain at all in underdeveloped countries or obtain only partially" (p. 253). This contrast is perhaps slightly overstressed, since some of these factors (for example, "availability of an expanding market," "tendency for a balanced ratio to be maintained between population growth and economic growth") were as much the result of industrial growth as the cause of it. The basic thesis, however, seems to be valid and Professor Bonné undoubtedly provides a most well-documented study of different aspects of this necessary "implantation." Rich in historical data and in rigorous economic analysis, the book will fill a very important gap in the literature on economic development.

Professors Meier and Baldwin have given their book the grand title *Economic Development*; the subtitle *Theory, History, Policy* does not make the field much narrower. On examination, however, one finds that this nomenclature is entirely fair, for the book is perhaps the most comprehensive one in the field. The authors discuss, in Part I, nearly every theory of economic development from Adam Smith to the most modern prophets of economic growth. This is followed by a historical study of the various stages of economic development in the West. The next part deals with the characteristics of poorer countries and the means of accelerating their economic development. In the last part the problem of *maintaining* development in rich countries is discussed. Nearly all the literature available at the time of writing has been used. The result, as one might expect, is a very useful book. It is also, unfortunately, a little confusing. The book is so comprehensive that it includes an abundant quantity of rather doubtful theories and the reader might feel somewhat lost unless he knew what he was looking for. Again, are there in fact so many different theories of underdevelopment? Can one, for example, treat the capital deficiency of underdeveloped countries and the economic backwardness of the labour force in those countries as two entirely different problems? Can one deficiency exist without the other? Again, are all the "vicious circles" discussed really different circles? In trying to present all theories of underdevelopment, the authors have made the field more crowded than it need have been. This crowding is, however, from another point of view, a merit of the book. It is useful to have a book where all the alternative theories are clearly and intelligently

presented, even though their interconnections are not shown to one's complete satisfaction. As a grand text-book in the full American tradition, the volume will make things easier for a great many students of economics.

Poverty and Capital Development in India is the misleading title of Dr. Rangnekar's book on the Indian economy. Not much is said in the book on the nature or causes of Indian economic poverty and estimates of money investment outlay without correction for price changes constitute the information on capital development. The author is mainly interested in studying the agricultural and the industrial structures of the Indian economy and in critically reviewing the Indian Five Year Plans.

Capital formation is, no doubt, an important field of investigation. It is not clear, however, what purpose is served by presenting undeflated annual figures of gross and net investment expenditure in different fields. A time series of real investment figures might have been interesting. A comparison of margin capital-output ratios over time, or of different industries, or of alternative techniques of production, could have been instructive. The author does not reveal much interest in any of these questions. Why he is at all interested in estimating investment expenditures is, therefore, not so obvious. He makes remarkably little use of his figures while discussing policy problems in the rest of the book.

The author disapproves of the present emphasis on the public sector in India and treats it as a self-evident truth that private enterprise is essential for India's economic development. Interestingly enough Dr. Rangnekar also points out that in India "private capital is nervous and is easily scared" (p. 241); but instead of asking whether that characteristic imposes some limitations on its role in economic development, he goes on to suggest that the State should be particularly careful not to scare the private sector in any way.

Dr. Rangnekar criticises the Second Five Year Plan for a relative neglect of agriculture. This, in view of the recent food shortage, seems fair. He also criticises the Indian policy of developing heavy industries, on the ground that such industries are "uneconomic" and "premature" in an underdeveloped country with surplus labour and capital shortage. This section of the book seems to be based on a number of current economic prejudices. In a country for the growth of which foreign exchange is a basic bottleneck, development of machinery-producing industries may have, depending on (among other things) the relevant capital-output ratios, a most favourable effect on its economic growth. In any case, any argument of this nature should be based not on simple *a priori* reasoning, but on concrete economic data about the alternatives involved. Dr. Rangnekar presents few data on this question, and in their absence, all we are expected to do is to have faith in his judgement. Dr. Rangnekar could have been more fair to his readers.

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Classics in the Theory of Public Finance. Edited by RICHARD A. MUSGRAVE and ALAN T. PEACOCK for The International Economic Association. (London: Macmillan, 1958. Pp. 244 + xx. 30s.)

It is always difficult to review a selection of classics. The articles or extracts, their good and bad points, are likely to be well known already; and the game of criticising the editors' selection is usually too easy to play to be worthwhile.

In this particular case, perhaps, these difficulties are not quite as pronounced as usual. The aim of this series sponsored by the International Economic Association is to provide translations of important works in foreign languages, and hence there is only one extract from Anglo-Saxon literature (Edgeworth on "The Pure Theory of Taxation") in this compendium. Although some of these translations are from celebrated continental authors (such as Wicksell, Lindahl, Wagner), others (such as Goldscheid, Ritschl) are not quite so well known, at any rate to the readers of British and American journals. Furthermore, the editors have made a deliberate attempt to concentrate on contributions to the theory of public expenditure rather than the theory of taxation.

The result of this selective process is an interesting volume, much of which is likely to be new reading even to specialists in the field of Public Finance. Perhaps the Swedish contribution in this, as in other spheres of economics, is the most interesting. A long extract from Wicksell's *Finanztheoretische Untersuchungen* (Jena, 1896) and a rather shorter one from Lindahl's *Die Gerechtigkeit der Besteuerung* (Lund, 1919) make up the selection. Wicksell argues firmly for the benefit rather than the ability to pay approach to taxation, but clearly sees the difficulty that "if the individual is to spend his money for private and public uses so that his satisfaction is maximised, he will obviously pay nothing whatsoever for public purposes" (p. 81), simply because he knows that the public services will be there anyway whether he makes any contribution to their cost or not. Therefore, in his view, decisions on the size and composition of the public sector must be regarded as a joint economic and political process. Lindahl, building on Wicksell's work, produced a link between the benefit and ability-to-pay theories and evolved a mechanism for determining the price of public services based on the same principles as those underlying price determination in the private sector.

Most of the other selections in this book are designed to illuminate either the German or Italian traditions of thought in this field of economics. Accordingly, we have contributions from (*inter alia*) Wagner and Sax, on the one hand, and a series of Italian authors such as Pantaleoni and Mazzola, on the other. Another contribution outside either of these traditions is to be found from the writings of the Dutch author, Cohen Stuart on Progressive Taxation; and yet another in an extract from Leroy-Beaulieu's *Traité de la Science des Finances*.

A collection of such writings together with an editorial introduction by two distinguished and well-known specialists in the field of Public Finance could hardly fail to be interesting and illuminating. Indeed, this is all the more so in view of the discussion which has been taking place in American journals in recent years on the pure theory of public expenditures. Nevertheless, one has to ask oneself whether some of the contributions to this volume are worthy of the careful dusting given them by the editors. Undoubtedly, the Anglo-Saxon tradition has been too insular. At the same time, outstanding contributions from the Continent usually have been noticed and appreciated (this is illustrated by the references by Edgeworth, as quoted in this volume, to Sax, Cohen Stuart and others). So perhaps not everyone will feel quite as strongly as the editors that all these ideas from the Continent have been unjustly neglected and that Anglo-Saxon economics has lost a great deal thereby.

A. R. PREST

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The Uniting of Europe. Political, social and economical forces, 1950-1957. By E. H. HAAS. (London: Stevens for the London Institute of World Affairs, 1958. Pp. xx + 552. 50s.)

NEGOTIATIONS for further European integration can be exasperating. After months of protracted discussions, any one government can always delay matters by re-stating its fear of others and desire for safeguards. The record of the 1950s suggests that every successful project must be negotiated twice. This is tiresome. Yet, considering Europe's old tradition of strife and that serious attempts at European integration are only a decade old, it is remarkable that Europe has already become more than a geographic designation—though its ultimate area and meaning are still uncertain. At the present juncture, when the European Coal and Steel Community is about to "over-spill" into the (twice negotiated) European Economic Community, it is useful to have this survey of the development of European supranationalism in E.C.S.C.

Mr. Haas distinguishes between the minimalist and the maximalist approach to Europe—Europe meaning E.C.S.C. The minimalists want to concede as little as possible, and concessions, once made, are interpreted as narrowly as possible. The maximalists desire the widest possible interpretation of existing supra-national powers and seek their extension. The minimalists normally include those who are doing well as it is. They fear further supranationalism might undermine their present political and economic importance. The minimalists include the High Authority itself, national governments, trade associations of strong industries before E.C.S.C. came into being, trade associations of weak industries after the establishment of E.C.S.C. and trade unions before E.C.S.C. came into being.

The High Authority's minimalism is due to caution: its power must not be compromised. It depends on the goodwill of national governments whose agreement is preferred to strict enforcement of the law. Disputes are taken to the Court of Justice sufficiently rarely to make Court decisions generally respected and obeyed. Normally, the effectiveness of E.C.S.C. depends on consensus in the Council of Ministers (which consists of members of national governments). Whatever the authors of E.C.S.C. may have had in mind, the record reads as if policy-making power rests with the Council of Ministers, while the High Authority is *de facto* the Council's executive. The Council does not, however, act entirely as guardian of national sovereignties against supranational encroachments. The Ministers meet frequently. Each knows that to-morrow he may want special consideration, so that it does not pay to be too awkward to-day. The Council seeks general consensus, and this can lead to broader rather than minimalist interpretations of the treaty.

Since the High Authority does what the Council of Ministers wants and the members of the Council of Ministers feel primarily responsible to their own national parliaments, the Common Assembly of Parliamentarians could easily have become a frustrated body. Instead, the Assembly tries to assert itself by demanding further extensions of supranationalism which it hopes to control. The Assembly is E.C.S.C.'s underdog. It shares with other continental underdogs a desire for more influence at Luxembourg. The other underdogs include trade associations of weak industries. These were at first fearful of E.C.S.C. and initially opposed it. Later they found that E.C.S.C. might help them when their own governments refused. Similarly, trade unions found that when their domestic demands were met as far as practicable, supranationalism provided them with a new outlet in demands for harmonisation of social conditions. Finally, political parties in opposition at home do not mind trying for more influence *via* a supranational forum. In general, the strong favoured the opening of economic frontiers and subsequently sought to confine E.C.S.C. as far as possible to the supervision of free trade. The weak initially feared and opposed E.C.S.C., but once it was established came to think of it as an additional instrument of protection. In practice, neither minimalists nor maximalists had it all their own way. The result to date is minimalism plus what is possible or maximalism minus the impossible, with the balance nearer minimalism. The growth of supranationality in the Six was less one of deepening of supranationalism in a narrow sector than its spread over wider fields, such as railway charges, labour conditions and the like, until members were finally prepared for its grand over-spill into the European Economic Community.

The chief merit of the book lies in its contribution to understanding of the political history of the Six in relation to E.C.S.C. The comparative sections on Britain are, however, too sketchy to be of much value. Economists will find their appetite often whetted, but never really assuaged by references to economic propositions. For instance, on p. 430 the author reports the view

of some continental socialists that Keynesian prescriptions can no longer be meaningful in the confinement of national states. Regret that this and other economic propositions are not analysed should not be regarded as adverse criticism of a book concerned with the political consequences of an economic institution rather than with economic theory.

F. V. MEYER

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The Theory and Practice of Exchange Control in Germany. By F. C. CHILD. (The Hague: Martinus Nijhoff (London: Batsford), 1958. Pp. viii + 241. 38s.)

THE system of exchange control which was adopted by Germany in the early 'thirties and which was later extended under Dr. Schacht's ægis has for long been the favourite example of the scope and functions of government regulation of foreign payments. Mr. Child's main excuse for taking up the topic again is that the modern theory of the effects of trade restrictions upon the gains from trade, as well as the application of Keynesian economics to international economic relationships, throw new light upon German pre-war experience. Moreover, his appears to be much the most detailed account of this part of twentieth-century economic history to have been published.

The book opens with a discussion of economic events leading up to the adoption of exchange control in 1931—the beginning of the Great Depression, the financial crisis in Austria, and the flight of both foreign and domestic capital from Germany. In the circumstances, as the author argues convincingly, Germany had no choice but to restrict the sale of German currency against foreign exchange, the alternative—devaluation—being ruled out, as it would almost certainly have stimulated adverse speculation. Moreover, as Mr. Child goes on to show, the exchange-control regulations were fairly liberal and in many ways non-discriminatory at the beginning, though as a result of both technical factors and the increasing over-valuation of the mark (due at least partly to the depreciation of sterling and other important currencies) there was a distinct tendency for the system to spread and to become more restrictive. Special exchange procedures—barter, bilateral clearing and the *de facto* devaluation of selected mark accounts—made their appearance in 1932, and when the National-Socialist Government came to power early in 1933, they found the basis for an aggressive foreign-trade policy already in existence.

At this point, the author interrupts his historical account in order to demonstrate the theory of monopolistic terms of trade. His analysis differs from the conventional treatment of this topic in that he employs the concepts of the demand for and the supply of foreign exchange instead of working with offer curves. This device allows him to introduce exchange control directly into the model, but apart from that advantage this proves to be a relatively

clumsy and sometimes rather confusing way of demonstrating well-known propositions.

The remainder of the book is devoted to the story of Germany's foreign-trade policy under the new masters. The three chief aims of the Government were to isolate the balance of payments from the effects of internal recovery, to reduce Germany's overall dependence upon international exchange and to increase the gain per unit of trade. In consequence of the pursuit of these objectives exchange control became, as Mr. Child shows, part of an exceedingly detailed supervision and regulation of economic relations with the outside world. Bilateral clearing superseded multilateralism as the accepted basis of trade, and currency manipulations such as the use of special account marks were the order of the day. The author recounts at considerable length the effects of this policy upon Germany's commercial relations with the different countries and regions, stressing the means whereby Germany exploited the weaker of her trading partners, though he clearly tries to avoid painting too black a picture of the German record. The best example of this is probably his contention that although many countries, particularly those in South-Eastern Europe, traded with Germany at exceedingly disadvantageous terms, the increase in exports tended to stimulate the internal economy and thus brought about a net gain for the "exploited" partner. It is a pity, however, that the author fails to state in clear terms the reasoning underlying this hypothesis; otherwise he would probably not have confused the reader by referring to the quite common case in which his argument is most likely to be justified—that of unrequited exports to Germany—as "double exploitation" ("double" because exports were sold to Germany at relatively low prices in exchange for blocked balances).

There is no doubt that Mr. Child's book has many merits. He has presented a detailed account of an often exceedingly complex issue, and many of his judgments appear to be soundly based. On the other hand, there are a number of important blemishes. Perhaps the most important one is that although the author has attempted to present a comprehensive study, many problems remain unanswered. We do not really learn very much, for example, about the detailed working of German trade controls (what exactly were the internal German arrangements for coping with the problem of the widely differing prices being paid for imports of the same commodity?), and some attempt should (and could) have been made to obtain quantitative evidence for the contention that Germany's terms of trade differed markedly from country to country. Finally, there are a number of confusions, including verbal contradictions, in the argument, which careful proof-reading would have eradicated.

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H. H. LIESNER

A History of the Dollar. By A. NUSSBAUM. (New York: Columbia University Press, 1957 (London: Oxford University Press, 1958). Pp. 308. 36s.)

MR. NUSSBAUM has produced a very interesting chronicle of the dollar. He has delved into the past and brought to light many fascinating episodes. He has faithfully recorded the leading events in the evolution of the dollar with scholarly care. At times he makes us share his interest in the format of coins or the imprint on notes, and he has illuminating legal passages. Towards the end he allows himself to show a certain pride, but in an entirely discreet and inoffensive vein, in the emergence of the dollar, after its checkered history, as an international medium of settlement of foremost importance in the world. He is entitled to that pride.

On the other hand, he does not appear to be deeply interested either in the theory of coinage as such, embodying the central principle of Gresham's Law and all that flows therefrom, or in the interaction between monetary and trade-cycle theory. Sometimes he is content to account for an outflow of metal by an adverse balance of trade. A keen theorist is apt to regard such an adverse balance as posing, rather than solving, the problem. May it not be due to some internal monetary disorder, which itself needs explaining? Or to some disparity in mint ratios of a kind that played so dominant a part in coinage history?

An example of this is that, in referring to the decline of silver prices "to an extent never seen before" after 1873, he does not mention the rupture in that year of the bi-metallic par for the first time for some five centuries; that par had been the dominating influence on the fate of gold and silver coinages, in America no less than elsewhere. No one sensitive to coinage theory could just have slid over that event.

Your reviewer may perhaps be permitted to take up a point, where he is referred to in Mr. Nussbaum's text. I stated that the gold value of the dollar was sustained in the thirties, after the dollar ceased to be gold-convertible for individuals, by the operation of the gold bullion market in London. He seems to find in this an implied slight upon the United States—which was by no means intended!—and rather heavily observes, "the gold-value standard of the dollar as established by the presidential proclamation of 1934 was maintained by the United States itself, whose gold reserves amounted to four or five times those held by the United Kingdom." A mere proclamation that a currency is worth so much cannot alone maintain its value, if that proclamation is not reinforced by a willingness to convert it for individuals. The American willingness to convert it for other central banks was essential, but not sufficient, for the maintenance of its value. The point is that the London gold bullion market was in daily touch with the British authorities, on the one hand, who could get gold for dollars at the shipping parity, and with the individuals throughout

the world, on the other. This meant that the top sterling price of gold in the bullion market was governed day by day by the sterling price of the dollar in the foreign-exchange market, which floated, and this meant that the whole world of individual dollar holders could get gold for dollars via sterling, if they so wished, at par plus shipping cost. After the War this mechanism was no longer operating, and the dollar fell to a heavy discount, as Mr. Nussbaum narrates. How was that? If it was the President's proclamation and the large size of the American gold reserve that upheld the dollar in the thirties, why did they fail to do so in the forties? The answer is that in the latter period there was nowhere in the world a gold bullion market operating in daily touch with a monetary authority able to get gold for dollars at par.

Mr. Nussbaum thinks that gold itself is now losing importance, and that "a kind of international dollar-gold standard seems to have emerged." If this went well, all would welcome it; but its success is by no means clearly assured at present. It could succeed if the Americans were willing to allow external dollar balances to grow in line with world trade, which would mean that they would soon far exceed the gold reserves of the United States, valued according to the proclamation of 1934. Alternatively, it could succeed if the Americans altered the value of the dollar set out in that proclamation. If they are not willing to do either, then gold is likely to come to the forefront again, regardless of the dollar, but only after a painful process of transition. There are certain awkward, hard-edged facts about money. Mr. Nussbaum's theoretical appreciation is rather too vague for him to be a good guide about these.

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The Structure of British Industry, A Symposium. Edited by DUNCAN BURN.
(London: Cambridge University Press for the National Institute of
Economic and Social Research, 1958. Vol. I, Pp. xvii + 403, 45s.;
Vol. II, Pp. xii + 499, 50s.)

SINCE some criticisms are made below, let it be said at the outset that the National Institute of Economic and Social Research has put all teachers, undergraduates and students of economic affairs deeply in debt to it, by conceiving and executing this work; and so have the administrators of the Conditional Aid programme for economic research, who financed the preparation of this landmark.

Previously, every seeker after knowledge of the organisation or functioning of British industry has had to hew his way through the vast primeval forest of White Papers and journal articles, confronted with infuriating inconsistencies in statistics between one source and another, and forced, for lack of even bibliographical guidance, to zigzag myopically from one source

to the next. Now the forest is mapped, a wood has been created from the trees, and for those who wish still to ferret in the undergrowth, there is an organised system of signposts in the bibliographies which complete each contribution.

It goes without saying that the standard of the contributions is uneven; though they are all competent. Fortunately, the sheer size of the work (nineteen studies, covering industries responsible for 40% of the net output of manufacturing, plus agriculture, building, transport and coal-mining) relieves the reviewer from the invidious task of drawing up a league table of contributors, and from the idle pretence that his own knowledge is encyclopaedic enough for him to essay such an enterprise. It seems more useful instead to consider some general questions which the work provokes.

The general conception of the symposium is indicated by its title; it is concerned with the structure of a group of British industries. It is taken for granted that it is appropriate to define an industry in terms of the standard industrial classification. This reviewer happens to believe that there are good reasons for so doing, and the gap between theoretical and applied economics in this field is no doubt too long standing for much discussion to be needed. But, given the degree to which the traditional theory of the firm is based on a product classification, one might have expected some explicit justification for the activity classification here adopted.

This is perhaps only a particular example of a general deficiency in the symposium; namely, the absence of clear guiding principles for the contributors. The distinction between structure and function has long established scientific warrant. But the body economic is a far more complex organism than is the body physical. There is the more need therefore for a study of structure to be guided by some prior notion of what is important from a morphological point of view. Indeed, since change is a fundamental feature of industrial structure, it is at least arguable that the changing structure of British industry would have been a more rewarding subject. It is said in the introduction to the study that the object was to examine the effect of structure on economic performance. This is all right so far as it goes. But economic performance is still largely an undefined term, particularly as applied to an industry rather than a firm.

This absence of a general guiding principle for contributors is undoubtedly a reflection on the present state of economic thought rather than on the organisers of the study. But it helps to explain why a certain lack of direction is apparent in some of the essays. Not to put too fine a point on it, some of them must be characterised as inordinately dull; even after due allowance has been made for reviewer's indigestion.

A work of reference is dull almost by definition. But these contributions were not intended to be merely that. And Mr. Reddaway's chapter on the chemical industry is a most stimulating example of what can be done by someone who approaches his subject with a clear set of important questions

in mind. There are some other essays which come near this standard. But in some cases the line of inquiry is by no means clear. Indeed, one has the impression that some contributors may have been inhibited by the emphasis on structure from trying to answer some of the more obviously important questions which naturally suggest themselves. For example, the reader who approaches the study of inland carriage by road and rail with the idea that he will find there set out the conditions under which the railways could hope to pay their way on a commercial basis is in for a disappointment.

Perhaps, however, all that these criticisms amount to is a plea that some one, a giant in conceptual ability and industry, should now produce the general treatise which the editor's concluding Retrospect explicitly does not attempt. Certainly, anyone sufficiently daring to take on this task has here a rich store of material on which to draw.

Mr. Reddaway provides him with a discussion of the concept of an industry, in terms of common "know-how and facilities," which would have filled admirably the gap in guidance to contributors referred to above. He gives also some illuminating insights into the question of goodwill, a factor of prime importance, since it is this which guards the markets for industrial products from the instability suffered by primary producers, but one which has received little attention. The importance of "regularity of supply or reciprocal loyalty between producer and customer" must have become all the greater in the post-war era of full-capacity working.

There will be many to question Mr. Reddaway's conclusion that "One of the advantages of having production largely in the hands of a very few big firms is that they assume a sense of responsibility to the public interest which is probably more valuable than any formal regulations or agreements could be in such a complex and changing industry." Indeed, Professor Carter, while recognising the dilemma that, in the building industry, "competition, while a spur to efficiency, makes its attainment more difficult because it prevents co-operation in the planning of the world," sets his face squarely against the rationalising or nationalising solutions which have been proposed. But, wherever the truth may lie, these are the big issues for debate.

Professor Carter himself provides less of a source book than do most of the other contributors, but I am full of admiration for the neat way in which he has reduced this "untidy, shambling giant" of an industry to order in less than thirty pages. He succeeds in asking most of the right questions—much the most difficult activity—even if not all of them are answered. Amongst other things, his contribution illustrates very clearly why generalisation is so difficult in the theory of competition.

And finally, to end this very small sample of the stimulating ideas to be drawn from the symposium, there is Mr. Silberston, boldly seizing the old and sturdy nettle which Professor Galbraith has recently christened the dependence effect. Since the use or misuse of tail-fins and chromium plate

has become the leading symbol of the effect, a study of the motor industry could scarcely avoid discussing it. Nor is it surprising, in the present state of knowledge, that Mr. Silberston should present his conclusions on competition and efficiency somewhat uneasily. But the discussion is full and fair. And there is some comfort, morally dubious though it may be, in the implication of his export-performance criterion that, a trade-based economy having to take the world as it finds it, any lead for dealing with the effect must originate in the land of its father.

J. DOWNIE

London.

American Investment in British Manufacturing Industry. By JOHN H. DUNNING. (London: Allen & Unwin, 1958. Pp. 365. 35s.)

THE impact of United States capital in British industry and the extent of its penetration is largely virgin territory, and Dr. Dunning's book is all the more important for that reason. Faced with the relative paucity of published material, he has succeeded in obtaining data by interviewing 160 companies and corresponding with 45 others. These 205 companies accounted for 90–95% of the total labour force of American financed manufacturing units in the United Kingdom, and for that reason, as well as Dr. Dunning's obvious concern to avoid unsupported generalisations, his conclusions on the advantages and disadvantages of American participation in British industry can be treated with respect.

There is no doubt in Dr. Dunning's mind that the advantages far outweigh the disadvantages. The advantages have been direct in reducing the dollar import bill and increasing United Kingdom exports. The magnitude of the contribution on both sides of the account is impressive. In 1954 about £500 million worth of goods and services produced by American-controlled firms were consumed in the United Kingdom, but Dr. Dunning estimates that had the equivalent quantity of goods been imported from the United States the cost would have been as much as £600–£650 million. In the same year, exports of these companies totalled £275 million, or one-eighth of total United Kingdom manufactured exports. Indeed, in the case of the newer industrial products—those which have been commercially produced only during or since the 1930s—the American-financed companies accounted for one-third (and if cars are included, two-fifths) of total United Kingdom exports of these products.

The other advantages have been less tangible but equally important, since they accrue from the example that the United States companies have set their British counterparts in management techniques, research and marketing methods.

"The directness of these associations and the contact which United Kingdom industry has been given with American industry has increased

their receptiveness to new ideas, and has enabled this country to adapt herself to her changing economic environment more quickly and with less frictional disturbance than would otherwise have been the case. Productivity has been raised in many spheres of industry and commerce, American managerial practices have been both publicised and assimilated, and the general tempo of industrial development advanced."

As far as research is concerned, most of it is done by the parent companies in the United States. Only one-fifth of the firms undertook basic research on their own; nearly three-fifths did some applied and development research while relying on their parents for fundamental know-how; and the remainder maintained no separate research department. It is a sobering thought, however, that in real terms the parent companies of the hundred most important American-financed firms manufacturing in this country devote more to research and development each year than the whole of British industry combined.

Dr. Dunning stresses that the traffic in research and development is not entirely one-way: new knowledge has been passed back in respect of diesel engines, turbines, harbour radar installations, phenolics, laminated plastics, materials for cinematic equipment and mechanical handling techniques. In addition, nearly half of the companies make some specific payment for research knowledge, but the author is right to emphasise that "too much reliance on the research and development department of the American concern could well mean that the British company refrains from independent research," and by restraining the demand for pure scientists and technologists tends towards "the stifling of initiative and the development of this country's industry in its own right."

The greater part of this study deals with post-war developments, and quite rightly so, for American investment has increased most rapidly during this period. In 1955 it stood at \$1,420 million: 70% more than in 1950, 175% more than in 1943 and 195% more than in 1929. This post-war growth is attributed to the dollar shortage, which induced American firms to produce in Britain and sell their products abroad for sterling; to the growing demand for American designed and styled goods; and to Britain's need of specific industries, such as oil-refining, in which American expertise was most important. But Dr. Dunning does not neglect the two earlier waves of United States investment in this country: the first from 1870 to 1914, and the second during the inter-war years, when import duties and quota restrictions were imposed on the products of their parent companies.

Dr. Dunning has made the most of his material, and the result is a book which is a competent and thorough analysis of the growth, consequences and implications of American investment in British industry.

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Economics and Agriculture. By ANNE MARTIN. (London: Routledge & Kegan Paul, 1958. Pp. viii + 169. 21s.)

MISS MARTIN here sets out briefly and clearly the basic theory of markets, demand and supply with special reference to the conditions of agriculture. Market prices, the relationship between incomes and prices and between costs and prices, the nature of profits and of economic rent, that elusive concept economic efficiency, the reasons for and methods of State control of agriculture, the problems of land tenure are severally discussed in nine chapters.

Miss Martin writes easily, she draws her illustrations from a wide knowledge of agricultural problems in many countries, and many beginners in agricultural economics will find this book useful. But the more practically minded students working for an agricultural degree may be discouraged by the first chapters, whose matter is far from immediately relevant to the world they know. The chapter on economic rent, for interest, largely ignores the market for land or for houses as it operates to-day; it seems likely only to confuse the beginner by presenting him with concepts of negative economic rent and negative pure profit, abstractions which have only a tenuous link with the profits and rent actually earned in farming.

Again, Miss Martin includes in her exposition of production planning those imaginary animals the iso-cost and iso-revenue lines, whose main function is to provide easy questions for the examiner of final-year honours students. In my experience, the undergraduate studying agriculture does not find such concepts of any use in thinking about economic problems. And the author's contention (p. 37) that land and capital cannot be distinguished is belied by the last chapter, which discusses separately their separate markets and conditions of supply.

For the students who are prepared to plunge into economic analysis, or for those who have already dabbled at the shallow end, this book will be a welcome addition to the meagre list of text-books on agricultural economics.

EDITH H. WHETHAM

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The British Paper Industry, 1495-1860. By D. C. COLEMAN. (London: Oxford University Press, 1958. Pp. xvi + 367. 55s.)

THIS book is sub-titled *A study in industrial growth*. Written almost entirely from the angle of economics, it sets out not only to describe but also to explain the development of the paper-making industry from the end of the fifteenth to the middle of the nineteenth century. It starts from the growth in the demand for paper and goes on to analyse the major influences operating from the side of supply—raw materials, capital and labour. The interactions of supply and demand, the effect on the industry of changes in fiscal policy, the changing locational pattern, the firm structure and the course of

technological change are carefully woven into the narrative so that in each phase of its history we are made aware of why things happened the way they did and what exactly was making the industry "tick." Only the chapters on industrial relations, excellent as these are in themselves, appear to stick out a little as loose ends.

It is not easy to illustrate the combination of historical narrative and economic analysis which is the central merit of the book. We are, for example, given a picture of rising demand in the eighteenth century being met by the introduction of the Hollander, with consequent changes in the scale of production until progress is halted by rising rag prices, labour difficulties and tariff policy. Pressure built up, and this appears to have resulted in the nineteenth-century "breakthrough"—the paper-making machine, rising productivity, fiscal reform, falling prices, increasing demand and so on. One is not left with a feeling that all this was inevitable. The concurrent rise in raw-material supplies on which so much depended was largely fortuitous. One is left to speculate whether a new source of raw material (wood-pulp) would have been tapped more quickly had things been otherwise.

It seems almost churlish to offer minor criticisms of such a first-rate piece of work. The economic approach should not lead economic historians into a belief that sensible economic reasons should be found for everything. The factors governing the location of the industry at the end of the seventeenth century (pp. 34–7) may have been less important than the chance that some local person would interest himself in paper manufacture. There must have been very many places in England where sufficiently suitable conditions could be found.

The present reviewer, as a true son of Gwent, was horrified to see Monmouthshire described as a remote area *circa* 1851 (p. 220) with the implication (p. 221) that it was remote from coal and from canal and rail transport. So far as Mr. Coleman's account goes, the decline in the number of mills at work in Monmouthshire 1816–51 takes a bit of explaining away, though we may surmise that clean water may have been difficult to come by in 1851. Another minor criticism arises in connection with the late introduction of steam power. Mr. Coleman attributes this chiefly to the antiquity and remoteness of the mills (pp. 112–13). It seems more likely, however, that power could be applied only to a process which accounted for a small proportion of total costs.

But these are very minor blemishes on a most impressive product of the new school of economic historians. It is a very good book which should interest specialists in industrial organisation as much as the historians.

Aberystwyth.

A. BEACHAM

The Efficiency of the Coal Industry. An application of linear programming. By J. M. HENDERSON. (Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. Pp. xii + 146. 36s.)

DR. HENDERSON'S monograph deals with the following limited but important question. If the United States be divided into eleven coal-producing areas (in each of which open-cast production is distinguished from mining) and fourteen consuming areas, how in any given year should production be allocated among the twenty-two producing units, and which coal should be sent where? He assumes perfectly inelastic demands and constant marginal costs of extraction up to a capacity limit for each area—reasonable enough assumptions given the short-run nature of the problem.

Dr. Henderson formulates this problem in terms of a linear programming model, which he applies to data for 1947, 1949 and 1951, to calculate for each year an optimal pattern of production levels and deliveries. By comparing these patterns with those observed, he is able to obtain measures of the efficiency of the coal industry in solving this particular problem. (The measures can, of course, throw no light on the efficiency of the industry in allocating production among the 800 or so pits in each area, or in solving long-period problems.)

The obvious and most significant measure, ignoring differences in factor prices, is one based on comparison of the total costs incurred by the actual and the optimal patterns. It turns out that total costs were 8–12% higher than they need have been, this excess being composed of extraction costs about 5% above the optimal and transport costs from 13 to 26% higher. The "dual" programming problem enables him to investigate the distribution of the costs of inefficiency between consumer, operator and deposit owner, and to conclude that they fall mostly on the last-named. It seems that, once again, inefficiency is due to a failure to charge full economic rent. Dr. Henderson also computes a measure of efficiency showing the proportion of total output that is produced in the right areas; this is clearly inferior to the cost measure, as it gives no indication of the magnitude of the loss caused by each "misallocated" ton of coal.

So long as he is applying his linear programming model, or discussing its results, Dr. Henderson exhibits an unusually high level of technical competence. But in Chapter VI he yields to a temptation to discuss the long-run efficiency of the industry, and to offer suggestions for improving it. His empirical work on the short-run problem offers no guidance here, and his discussion is both superficial and naïve. To take but one example, he ignores capital costs, which, though not relevant in the short-run, are important in the long-run—there is, after all, a considerable difference between a coal deposit and a coal-mine.

We must also register some doubts about his suggestion in Appendix B that the same technique can be applied to agriculture. Differences in

quality and season mean that a very large number of commodities must be distinguished by the model, as they certainly are by the consumer. This is likely to produce a programming problem of quite unmanageable magnitude. In addition, for many agricultural products it is not plausible to assume an inelastic final demand, even in the short run.

But with these specific qualifications, we must congratulate Dr. Henderson on an excellent book, and one which is a model of what applied economics should be. He combines, moreover, considerable expository skill with a clipped American style, reminiscent of Hemingway, which makes a most refreshing change from so much of economic writing.

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M. J. FARRELL

Introduction to Modern Statistics, with applications to business and economics. By W. Z. HIRSCH. (New York and London: The Macmillan Company, 1957. Pp. xiv + 429. 45s. 6d.)

How should we teach statistics? At the elementary level the choices are clear-cut: we can either show a fledgling economist in his first or second year at the university how to use statistical data to answer economic problems, keeping statistical methods—sampling, regression analysis—for the specialists in their final year; or we can spread the teaching of statistical methods over two years, allowing experience in handling data to accumulate by way of exercises and time. Both methods can be made to cater for the bright student; only the first is suitable for all types of students—providing that we grant that it is an expensive luxury actually to teach an economist when to hire a statistician. The second question is: in scrutinising data, how much statistical equipment does a student need? The philosophy of social accounting, some theory of index numbers, the techniques of decomposing time series, together with instruction in averaging, errors and graphical and tabular presentation—these will be generally agreed upon, and if they are combined with exercises on the main sources (in Britain: the Bluebook, *Monthly Digest and Census of Production*) it will also be agreed that they provide plenty of work and scope for a non-specialist course in the first or second year.

What texts can be recommended? Devons and—more recently—Marris cover parts of such a course. But both are intentionally selective. Successful texts on methods such as Allen, or Mills, provide the rudimentary equipment, but it is surrounded and at times embedded in methods more suitable for the specialist. From this point of view, Professor Hirsch's "Introduction" is for the specialist. It is well organised, easy to read, with welcome dashes of common sense and humour in the examples. At the elementary level it deals very satisfactorily with averages, index numbers

and the decomposition of series. At the other extreme it deals—again very satisfactorily—with confidence interval estimation and decision theory.

If it is correct to judge this text as a text for specialists the one substantial criticism is that its emphasis is wrong. While the methods presented are up to date, the particular branches of statistical theory are those traditional subjects which provide little assistance to the economics student finding his way around the econometric literature. (It is only fair to say that this does not apply to the industrial statistician or the market researcher.) Professor Hirsch limits himself to a brief statement about multiple correlation (one page and a half). Considering that he takes the students such a long way into decision theory, control charts and the like, this seems an odd arrangement. There is no doubt that recent advances in statistical theory have left the middle-aged economist gasping, and it is valuable to have some of these developments distilled into a very readable text-book. But there are other developments in stochastic theory which are more suited to the interests of the general economist. Alas, no one with Professor Hirsch's gifts as a distiller has as yet turned to them.

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Survey Methods in Social Investigation. By C. A. MOSER. (London: Heinemann, 1958. Pp. xiii + 352. 35s.)

MR. MOSER quotes an estimate of the annual expenditure on market-research sample surveys in this country, which in 1956 was some £3½ million. It will be higher now, and if we add to this the cost of the government Social Survey, expenditure by academic research bodies, to say nothing of the effort of many, often small political and social organisations, we have survey work adding up to an economically sizeable activity. It is therefore timely that a comprehensive book like Mr. Moser's should appear, setting out to provide an introduction to survey methodology for the social science student, for the *ad hoc* surveyor, and—very important—for the users of survey results.

Here one finds discussion of most of the problems encountered in the design, execution and analysis of large-scale social surveys. Alongside an extremely clear description of the technical aspects of the work (sampling theory, mechanical methods of analysis and the like) the author does not eschew common-sense advice on such important matters as ensuring that the survey is not overlapping with others, or on the writing up and presentation of conclusions. He has been at considerable pains to refer to the specialist literature and there is an extremely useful bibliography with nearly 300 references.

There is no doubt, however, that the preparation of a text-book on this subject at the moment presents considerable difficulties, because of the

unsatisfactory state of our knowledge over much of the field. Work on the sampling of human populations has now reached a point where there is a set of scientifically based and well-defined principles available. As the author remarks, future developments here should be mainly refinements. When it comes to data collection, however, the method of field work, the design of questionnaires, the role of interviewers, response and the like, we must agree with Mr. Moser that our knowledge is primitive. There is a good deal of experience available (although not as much as there might be if investigators would only make more widely available the results of their trials and errors), but the most cynical would view this as a body of folklore, and the most kindly disposed as providing only a very general framework of reference.

I find it surprising therefore that the author has not adopted more widely a technique which he uses in his discussion of sampling. Here, after three chapters of sample theory and design, he devotes a fourth to the description of a national random sample design for a specific inquiry. Thus the reader sees the general applied to the particular, and although he may never himself be faced with the same problem, he can learn much of practical value. Why not similarly, then, with other aspects of a survey? If, for instance, we could have had a description of the stages through which a particular questionnaire passed, the form and order of the questions, its length, the amendments made after pilot experience and finally a discussion of mistakes and omissions, then many of what the experienced practitioner knows to be sound words of advice but which may appear to the uninitiated as vague generalities, would take meaning from such a concrete setting. Teachers using this as a text-book can supply their own illustrations, but the book is intended for a wider audience.

Indeed, it is to be hoped that it will be read by many of those now working in the survey field, who may then take note of the plea it contains, for more experimentation both at the pilot and main survey stages. It is clear from a comprehensive study, such as Mr. Moser's, that the area for investigation is vast. But surveys are costly, and while their results are in such demand, it is often difficult to justify expenditure for purely methodological research alone. The advance of knowledge must therefore depend upon the conscious co-operation of all those responsible for the planning and execution of social surveys.

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DOROTHY COLE

Financial Intermediaries in the American Economy since 1900. By R. W. GOLDSMITH. National Bureau of Economic Research Studies in Capital Formation and Financing No. 3. (Princeton, New Jersey: Princeton University Press for the National Bureau of Economic Research (London: Oxford University Press), 1958. Pp. xxxv + 415. 68s.)

THIS is a broad descriptive study of financial concerns in the United States essentially based on balance sheets for eight benchmark years between 1900 and 1952. These include such different institutions as federal reserve banks, government pension funds, investment trusts and personal trust funds; but of course the commercial banks and the insurance companies are the largest groups. This diversity makes it difficult to weigh up the significance of changes in the totals. But there is an even greater difficulty. This sector of the economy—as the word “intermediary” suggests—is far less independent than, say, the steel industry: its whole shape and size and growth is intimately connected with the development of the economy as a whole. Thus it is far harder to explain changes within this financial sector without writing a detailed economic history of the whole economy.

Thus, to say that Dr. Goldsmith succeeds in presenting a memorable description of the main features of this financial sector and indicating the reasons for the outstanding changes that have occurred over the past fifty years is to pay high tribute to his wealth of knowledge and skilful exposition. But this remains in some ways a difficult book. For, as is pointed out in the foreword and stressed by the author throughout, this branch of economics is in its infancy. This is a collection of building blocks, a collection that will certainly prove most valuable to many future workers in this field. As yet we cannot clearly see how they will fit together into an analytical structure explaining the interdependence of economic and financial growth and specialisation.

Some features, however, emerge clearly already. We can see that this financial sector has grown faster than the economy as a whole. This is common knowledge, and one valuable service performed by this study is the correction of exaggerated popular impressions of the dominance of financial units. Thus it is misleading to compare the holdings of financial concerns of all assets, tangible and intangible, with the total of national wealth; the true comparison with the total of national assets shows that the percentage held by financial intermediaries increased from 11% in 1900 to 22% in 1952. Since 1929 the most striking rate of growth has been in public intermediaries such as government insurance and pension funds. For the period as a whole from 1900 to 1952, adjusting for price changes and the growth of population, the rate of growth per head of assets of insurance organisations was over 4%, as compared with only 2% for the banking system. This relative growth of institutions which are mainly long-term lenders as compared with the banking system is reflected

in the funds provided for non-financial corporations by this financial sector. In the period 1901-12, 45% more was provided by acquisition of corporate bonds than by commercial bank loans: in the period 1946-52, 130% more. Financial intermediaries have become a slightly more important source of external funds for corporate business, but the change has been much smaller than many of us would have guessed. They provided 36% of external funds in the period 1901-12, 48% in 1950-52. In the total finance of corporate business the growing importance of retained profits has meant that the proportion of funds provided by financial intermediaries shows little change. But large manufacturing corporations relied more on financial intermediaries as a source of funds from 1946 to 1952 than in the 1920s or 1930s.

Again it is valuable to be reminded of the relative importance of housing and consumer durables, as the latter is so much more spectacular. Thus non-farm households are estimated to have provided \$723 billion for saving and capital-consumption allowances over the period 1901-49 and to have received \$70 billion from external sources, of which \$41 billion was in the form of mortgages and only \$20.1 billion notes and accounts payable. As Dr. Goldsmith says, "There is no question that the acquisition, holding and improvement of residential real estate has been far the most important single purpose for which nonfarm households require external financing."

The relationship of the main forms of saving through financial intermediaries to personal disposable income is another important aspect of their role in the economy. Such saving rose from a third of total personal saving in the first thirty years of this century to rather over half in the next decade.

Another virtue of this study is the clarity with which it draws attention to the deficiencies in the data and so suggests fields where further research is required. The finance of non-corporate business is one such large field.

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Economic Concentration and the Monopoly Problem. By E. S. MASON. Harvard Economic Studies, Volume C. (Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1957. Pp. xvi + 411. 48s.)

THIS book consists of numerous papers written by Professor Mason during the last twenty years. In Part I the author examines the problem of industrial concentration, mainly, although not entirely, in an American context. He analyses the different senses in which the term is used (concentration of control in the economy as a whole and concentration within particular industries), and he considers the economic significance of each type.

He then reviews the evidence about changes in the degree of concentration during recent decades and he concludes that in the United States' economy concentration has probably diminished during the last quarter of a century. This Part also contains useful papers on the price and production policies of large firms. Part II is mainly concerned with the relation of competition and monopoly to price policies and to problems of wages and employment. The author's treatment of price inflexibility is of particular interest. He also analyses the differences between the kind of monopoly power exerted by trade unions and that exerted by great firms or cartels, and he brings out the importance of these distinctions for public policy. Part III consists of papers on American resources and access to raw materials, in relation to the requirements of strategy, political security and economic growth. In Part IV the author deals with anti-trust policy, chiefly in the United States. He distinguishes between the attitude of lawyers and economists towards this policy, and he discusses the significance of recent trends in economic thinking about it.

The book, by the nature of its construction, lacks the unity of a systematic treatise; here and there it suffers from repetition; and Part III has only slender connections with the main theme. Nevertheless, the book is surprisingly well articulated, and taken as a whole, it makes a valuable contribution to the subject described in the title.

Professor Mason's treatment is eclectic, as good sense and realism require. The theories of monopolistic and imperfect competition, which once seemed so full of promise for the investigation of situations in the actual world, have provided, as he says, "a beautifully logical and consistent set of propositions without direct operational applicability." More recently, attempts to inject realism into this branch of inquiry by the introduction of the concept of "workable competition" have still left us without criteria easily applicable to actual situations. Professor Mason comments that the study of these problems and of price behaviour in particular has presented "the dilemma—too general in economics—of a choice between an institutional gathering of irrelevant data and a theoretical formulation of unanswerable questions." Although he does not pretend to have discovered the path to reconciliation, he at any rate suggests the type of questions that are relevant to an examination of many business situations and he discusses with insight the major issues that arise in this sphere of economic policy.

The author is especially interesting when he calls attention to the striking differences in the price policies of firms with apparently similar degrees of dominance in their several industries, and to the conditions that determine these contrasts, among them "the stage of economic development of the industry, the size of buying units, the nature of the distributive channels that confront the industry." Many of his readers will agree with him in doubting whether any "useful generalisations can be made regarding the price and production policies of large-scale enterprise without further specification of

the market situations which confront such firms." In dealing with anti-trust problems, he is critical of the traditional American suspicion of structures and practices that offend the ideal of pure competition, but he is by no means content with the formulations of Schumpeter, Galbraith and Lilienthal, whose complacency in the fact of massive concentrations of economic power he does not share. He considers that public policy towards monopoly has been guided by excessively narrow objectives, while economists and others who have sought to establish the principles of such a policy have ignored the complexities of the real world of business. An attempt to create a market structure and a set of business practices "such that the scope of action of individual firms is severely limited by the action of rival firms in the economy" (that is, the establishment of a condition of workable competition) is in itself not enough. At the same time, the efficient use of economic resources, or the adequacy of long-run business performance in regard to such matters as innovation and growth, is also, in isolation, an unsatisfactory objective. The tests "both of workable competition and effective business performance have merits and demerits; they must be used to complement rather than to exclude each other." Professor Mason offers suggestions about how these tests might be applied, and to this as to the other matters discussed in his book he brings wise judgment and an admirable knowledge of fact.

G. C. ALLEN

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Economics of International Migration. Edited by BRINLEY THOMAS. Proceedings of a Conference held by the International Economic Association. (London: Macmillan, 1958. Pp. 502. 45s.)

THE work under review comprises the proceedings of a conference of the International Economic Association on emigration held in 1955. The twenty-four papers consist of a group of three which attempt to develop analytical tools, followed by groups concerned with individual countries of emigration and immigration, intra-European migration, the Far East and social aspects of migration. They were well planned, and though of varied quality, most were interesting. Their usefulness in print to the student of migration would have been enhanced had they all been given the excellent bibliographic backing provided alone by Professor Spengler in his paper on the economic effects induced by immigrants. Professor Douglas Hague's report of the discussion is heroic, clear and extensive, though he could have made it more interesting had he censored irrelevance more vigorously.

The conference clearly owed most to the Chairman of its Programme Committee, Professor Brinley Thomas, who edited the volume and whose paper on migration and international investment provided the conference with its point of departure. The Thomas hypothesis, as readers of his

Migration and Economic Growth will know, finds the key to the great overseas migrations of the nineteenth century in the connection between movements of labour and capital. In the Atlantic economy migration synchronised almost exactly with international investment; long swings in the economic development of Britain and the United States were inverse to one another, periods of expansion in the American sector being accompanied by high rates of westward-moving migration and investment. There was general agreement about the bold outlines of this hypothesis which, indeed, had clearly influenced several of the other contributors, notably Professors Spengler and Rubin in writing of the United States, Professor Timlin of Canada and Mr. Isaac of Great Britain. Criticism and doubt appeared in discussion only on the refinements of the theory. There was some initial confusion between business cycles and long-term twenty-year cycles postulated by the hypothesis, some scepticism about the weight to be placed upon the statistical series involved and, more specifically, about whether bursts of migration induced bursts of capital movement or vice versa, and doubt about shifts of resources in Britain from construction to exports demanded by the theory; but on the whole it appears to this reviewer that the hypothesis stood up well to criticism. In particular, Professor Thomas defended himself in advance against the criticism which had been levelled against it elsewhere by Professor Lewis and Mr. O'Leary that because immigration and capital flows were statistically a very small proportion of American population and gross national product respectively, they were of little significance. What is important, says Professor Thomas, and here the historian assessing more qualitative evidence must bear him out, is the key industry or sector which sets the pace of change, the foreign funds which allowed the doubling of American railway mileage between 1866 and 1873, for example. It is curious that discussion of this free movement of resources dwelt on the benefits to the receiving country in terms of capital, labour and skills, rather than the benefit, which Professor Robinson almost alone stressed, to the countries of emigration of the resultant yield in cheap foodstuffs and raw materials.

The second part of Professor Thomas' hypothesis, and the major attention of the conference as a whole, was concerned with the period roughly since the Second World War when free migration is at an end and international investment has profoundly changed its character. The old inverse relation has disappeared. American private investment is not performing the old British role in under-developed countries, either in relative amount or in fructifying character. Instead American government investment in Europe for the time being has allowed European countries to maintain their traditional patterns of labour and capital exports; but the old nexus between migration and investment has lost its force and the conference turned to consider whether in the new circumstances migration is still an aid to economic development.

It became clear, especially from the "country" papers, that there is a good deal of controversy with governments and economists on different sides of this question. Australia has a long-term immigration policy, the Netherlands an emigration policy, the United States and Canada are in varying degrees severely restrictive of immigration; and yet Professor Hofstee argues that the Dutch drive to emigrate to Canada is social and has no economic justification, whereas Professors Rubin and Timlin respectively for the United States and Canada believe a more liberal policy would still benefit the North American economy; and Professor De Avila can pin his faith on immigration as the key to Brazil's emancipation from economic colonialism. The officials of the Intergovernmental Committee for European Migration continue to assume the need for emigration, not only from Italy and Greece but also from the Netherlands and Germany, whereas Professor Edding believes there is no economic justification for German emigration and Professor Zubrzycki, that even if there were no iron curtain the West would never again see a great migration from eastern Europe. Finally, an interesting group of papers dealing with eastern countries emphasised the contrast between the empty, under-developed countries of the nineteenth century and these new under-developed countries, which are also over-populated, where, apart from the infiltration of special skills, migration is irrelevant and growth the result of indigenous factors such as mass purchasing power.

The predominant tone of scepticism about the value of migration for future economic growth was underlined by the concluding, ably argued paper by Professor Ellis. In general, the participants are to be congratulated in having aired all the major economic considerations. So long as attention was focused on the criterion of increasing income *per capita*, discussion was enlightened and definitive. It only faltered when the conference shifted its attention to the discussion of two papers on the social aspects of migration; but this was a very different kind of terrain.

F. THISTLETHWAITE

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Canadian Economic History. By W. T. EASTERBROOK and HUGH G. J. AITKEN. (London: Macmillan, 1957. Pp. 581. 50s.)

"THE story of Canadian economic development . . . is essentially the story of how a group of scattered colonies organized themselves into a national economy equipped to provide the industrialized areas of the world with the materials and foodstuffs they required." In these words Messrs. Easterbrook and Aitken, in a remarkable collaboration, summarise the history they have so boldly and successfully told of what is virtually the Canadian nation. The account comes after research by a generation of brilliant Canadian

historians such as Innis and Creighton whose works comprise the end-of-chapter reading lists; but the sum is more than its parts. The book is a splendid synthesis which cannot fail to be of value to all, historians and political economists alike, who are concerned with the economic questions behind the rise and fall of nations. Though running to nearly 600 pages and, as befits its formal status as a text-book, covering every aspect of Canadian economic history from the most narrowly provincial to the most expansively continental, the account follows a few massive and well-conceived contrapuntal themes which enable the authors to pursue the particular to extremes without losing the reader's sense of relevance.

The first set of themes are the great staples which have determined the course of Canadian development. Fish, furs, timber and wheat are each shown, at length and in fascinating detail, to have provided the successive thrusts behind the expansion of British North America. From those by-boat keepers who were seasonal settlers on Newfoundland in the seventeenth century, to the *coureurs de bois*, pursuing their beaver westwards by way of ever more remote water-courses, the lumbermen logging the red and white pine of the St. Lawrence, and the homesteaders of Saskatchewan, all were occupied in producing staples, the fluctuating demand for which in Europe determined the fortunes of the Canadian economy.

These staples were a precarious basis for growth; and the second set of themes is concerned with the external and alternating market pulls, British and United States, to which the Canadian economy was exposed and which constantly threatened to rend asunder the national fabric. No sooner, it seemed, had Canadians adjusted, in the generation after the American Revolution, to their exclusion by the Hudson's Bay Company from the western fur trade, and their new role as purveyors of squared timber to Britain within the mercantile system, than the British timber and wheat duties were abolished and the commercial empire of the St. Lawrence had to seek new markets in the United States; after the period of Reciprocity, when the power of the American economy appeared to justify the United States assumption of a Manifest Destiny to take over Canada, Canadians returned by means of the tariff and railways to the development of a new, wheat staple for European markets. The authors bring out strikingly the ways in which the dynamic American economy to the south has chronically threatened to disrupt Canada's lateral communications, of which the building of the Erie Canal is only the most obvious example, and the counter-measures, from canals and railways to the St. Lawrence Seaway, which the Canadians have had to take not only to capture for the St. Lawrence a share of the United States mid-western trade but, in some cases, to preserve their very identity as a nation.

Particularly valuable is the comparison of the rates of growth of the United States and Canadian economies and the complex of reasons, geographic, political and social, for the spectacular emergence of the United States

as an industrial giant and the persistence into the twentieth century of a lagging Canadian economy based on an agricultural staple.

However, perhaps the most refreshing aspect of the book relates to the third set of themes. While by no means neglecting the more abstract concepts of current analysis, the authors give full weight to the human factors, individual and communal, without which Canadian economic history might have been no more than an episode in the history of the United States. It is not simply that the entrepreneurs—whether they be merchants, railway promoters, bankers, civil servants or politicians—are portrayed as creative decision-takers. The authors are right to describe their story as “how a group of scattered colonies organized themselves into a national economy,” because the organisation, will and drive were central to Canadian development. This is essentially a study in political economy, with the central role played, not by innumerable individual entrepreneurs spread over half a continent, as in the United States, but by a few big men operating in, or with, Government, from Montreal or Toronto, in accordance with a strategy of planned expansion, in canals, railways, land settlement, the tariff, banking and investment, and with strong central institutions, political, financial and commercial, which alone could preserve Canada’s cerebral being from absorption by the United States. The authors point out revealingly that unlike the United States experience, there is in Canada the concept neither of the Frontier nor of cutting loose from Europe; instead there is lateral continuity and control; the authors are to be congratulated in bringing out so clearly the uniquely *Canadian* experience in North American continental expansion.

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The Development of the Soviet Budgetary System. By R. W. DAVIES. (London: Cambridge University Press, 1958. Pp. xxii + 373. 45s.)

DR. DAVIES's study is a very detailed and well-documented one, the general scope of which is wider than its title might be held to imply. Since in the Soviet system so much more is channelled through the budget than in capitalist countries (for example, the major part of investment), the work embraces almost the whole field of planning in its financial aspect—what is officially termed ‘the unified financial plan’ (although the budgetary system proper does not include investment financed directly from industrial profits, or before 1938 social insurance payments, and thus is somewhat narrower than the unified financial plan). Among the topics covered, for example, are financial incentives to industrial enterprises, the system of bank-credit and credit control, and general price-policy.

Over the years there have been many books on ‘Soviet finance’ or the

financial system’; most of them descriptive of machinery and not always noted for their display of an historical sense. It is one of the virtues of Dr. Davies’s painstaking and erudite study that he has firmly placed it in an historical setting and that from an historian’s training he has derived a lively appreciation of the relation of particular devices and institutions to time and place. His book does not only describe machinery and accounting classification but discusses the functioning of machinery and the economic role of particular financial instruments as well as the problems associated herewith. He deserves congratulation for the way in which he has successfully organised a wealth of detail and subordinated it to the answering of interesting questions. It will stand for some time as the best standard work for students of the subject; but it should also serve as a source-book for economists interested in comparative economic systems or the problems of planning.

After a brief account of the pre-revolutionary financial system, Part 1 gives an account of the early financial measures of the revolutionary government after October 1917 and of the fate of finance during the inflation and demonetisation of the civil war years. Part 2 deals with the period of NEP in the ‘twenties when planning was still in its infancy and much of the traditional tax system and credit mechanism was retained. With Part 3, comprising nearly two-thirds of the whole, we come to the extensive financial changes of the early ‘thirties, with their much closer integration of the budgetary and the planning system, and the development of this system also its modification, especially quite recently) between then and 1955.

Inevitably there are certain points which readers with special angles of interest may wish to see more fully elucidated. The reviewer would have liked to hear more about the ‘exceptions’ where turnover tax is levied on producers’ goods, and is inclined to think that these exceptions may be somewhat more important than the author implies. Some may wish for more light on price-policy in its relation to output-policy. But available evidence about this is scanty, and the recent discussion on price-policy was for the most part subsequent to the writing of this book. On the whole, however, the general balance of the questions selected for special comment and elucidation is beyond reproach, as is the clarity with which they are handled. Among such questions are the variation of turnover tax rates for different goods or varieties of a good and the extent to which these variations are aimed at adjusting supply and demand; control of investment and the extent to which discretion is left to lower levels as to ‘decentralised investment’ and minor repairs; the device of ‘profit equalisation’ between enterprises with different costs by means of differential prices at the *glavki*-level; the conflict between the ‘planned output’ motive and the efficiency motive (to each of which is harnessed a special form of incentive-payment to the enterprise) and the consequences that followed from giving too much weight to the former, especially when output-targets were defined in terms of gross output,

or alternatively in terms of weight in some cases; the basis of transfer-prices between State enterprises, and the method of allocating raw material and fuel supplies; and finally "the successful containment of inflation" in the post-war years.

The book has a good index, an extensive bibliography, some fifty tables and a number of convenient charts or figures. Its general appearance is a tribute to the typography and printing of the Cambridge University Press.

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The Wealth of Nigeria. By G. BRIAN STAPLETON. (London: Oxford University Press, 1958. Pp. ix + 228. 16s.)

THIS book is a modest, but welcome addition to the growing, but still inadequate literature on the Nigerian economy. I say modest, because it has kept well within the bounds set by authoritative works that will be familiar to the specialist; it is welcome, because it provides particularly for Nigerian readers a sensible corrective to over-optimistic assessments of Nigeria's future economic expansion.

In the Preface, Mr. Stapleton tells us that he is "an economist who has specialised in Geography, and in particular in that branch of Human Geography which is termed Economic Geography." It is clear as the book proceeds—from a catalogue of Nigeria's "resources" to a group of chapters on "development"—that the author's interests range widely rather than run deep. The first third of the book, comprising five chapters, conducts the general reader round the subsistence crops, one by one, the cash crops, the livestock products, minerals, fuel, population and public services, including transport and communications. Then comes a transitional chapter in which the gist of the Report on Nigeria's national income of 1950/51 is reproduced, quite faithfully, but not altogether intelligently (the "explanatory Appendix" to this section could have been considerably extended with great benefit to the uninitiated). Nigeria's output per head is contrasted (with a very proper emphasis on the dangers of this kind of international comparison), with that of other nations both great and small; and there is a useful chapter on the industrial development of the major Western and Eastern nations. This central part of the book will give the African student much useful background information against which to set Nigeria's economic problems in better perspective. The treatment of Nigeria's agricultural and industrial development is modelled closely on the Report of the International Bank Mission in 1953, and the importance of a balanced or "integrated" programme covering the whole Federation is stressed. Finally, there are three short chapters describing the structure of Nigeria's international trade, the possible means of financing her development plans,

and giving a series of very brief observations on the probable effects that political changes may have on economic progress.

While the treatment is for the most part kept at a level that should make for easy and informative reading, there are, perhaps inevitably in a book of this kind, several points which do invite criticism. For a start, the title must be given a charitable interpretation, since the treatment of capital formation is inadequate and incomplete even for an introductory account. The view that Nigeria's economy is growing fairly rapidly is accepted without question, although indicators of agricultural output, whose overwhelming importance the author correctly emphasises, still remain a matter for conjecture. On Nigeria's trade, the statistics and the discussion stop at 1953, since when a surplus on current account has turned into a deficit; there is also the impression in this section (p. 179) that trade "ought" to balance between Nigeria and other countries, taken individually. Moreover, there is discernibly a tendency to treat physical resources as being of somewhat greater economic importance than the rendering of services of various kinds; and readers should be warned against this kind of wrong emphasis. However, these and other minor blemishes should not prevent the book from having a wide appeal, particularly in West Africa.

IAN G. STEWART

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Economic Survey of Europe in 1957. Prepared by the Research and Planning Division of the Economic Commission for Europe. (Geneva: United Nations, 1958. Pp. 390. 28s.)

WHAT a very good thing it is to be presented every year with such a volume as this! One must express admiration for the efforts which go into this remarkable compilation, with its unique mixture of painstaking detail and stimulating generalisations. The present volume is fully up to the high standard which one has come to expect from Geneva. It has, amongst other things, an unusually full discussion of the problems of the Eastern European economies and of the Soviet Union. Long and meaty chapters on these contain a mass of statistical data not otherwise available to the non-specialist (or the linguistically limited) economist, the official figures being supplemented by an intelligent use of a variety of sources and where necessary by the secretariat's own rough estimates. At the same time there is an account of the most important changes in economic policy and structure, accompanied by a valuable discussion of unsettled problems. Thus, for example, we learn about the debates on the role of prices and the market mechanism, especially in Poland, and also of the attempts made by economists to bring some kind of order into foreign-trade calculations. This last problem arises acutely where one finds both an unrealistic exchange rate and

irrationally inter-related internal prices. Both in Hungary and in Poland this has led to cautious experiments with notional multiple exchange rates, as a guide to planners, but within the present setting the problem is hardly soluble. Until recently, as the Survey rightly says, foreign-trade decisions were largely *ad hoc*; it was a matter of finding goods which could be spared, with which to pay for imports of goods which were physically short. However, the trend is now towards a more rational specialisation within Soviet bloc countries, and therefore towards a search for some objective criterion of relative efficiency. The discussion of East European trade is illustrated by a whole series of most valuable statistical tables. Another subject given full and extremely interesting treatment is the problem of manpower and employment in the Soviet Union and its group of countries. This is all the more important and timely because labour supply is becoming a major bottleneck, especially in the U.S.S.R.

Here and there one may find statements, generally of a detailed character, which appear to be questionable or mistaken. Thus, whatever may be the ultimate effects of the Soviet reorganisation of industrial planning, an "equation of marginal cost with price" is hardly likely to be one of them, though it is true that the new regional authorities will seek where possible to eliminate enterprises which persistently make losses. There is also a tendency, perhaps inevitable in an international organisation, to accept certain official claims which could be treated with greater scepticism. Thus the Soviet milk and meat output statistics are not internally consistent and appear to be somewhat exaggerated. Nor is it likely that the real income of Soviet peasants has risen by 68% in the period 1950-56, as is alleged in Chapter 7. Indeed, some other Soviet statements suggest that the real figure is perhaps half as great, though this, too, represents an impressive achievement. In Chapter 1 it is alleged that Soviet urban housing in 1956 provided an average of 1.6 persons per room, though this is not even claimed by the Russians! Properly deflated into "living space," the Soviet official housing figures show something of the order of 5 square metres, say 50 square feet, per head, which would mean over three people in a room 13 feet square. But there are remarkably few points where even the most carping critic can quarrel with the Survey's admirable chapters on Eastern Europe.

The sections on Western Europe provide ample grounds for thought and argument. The authors are evidently out of sympathy with "sound" anti-inflationary restrictionism. While conscious of the need to bring wage rises into line with increases in productivity, they do not fail to point out that such increases are improbable if output is not expanding. The United Kingdom does not show up well in any of the numerous comparative tables. Thus our "share of productive investment in total resources" appears lower than that of any other country except Ireland, Greece and Portugal. Only Denmark and Ireland show lower growth rates in the period 1953-56. On the other hand, the very useful table on "indicators of inflationary pressure"

United Kingdom (with France) as having the biggest increases in wages in this same period, while export prices rose by more than those of any other major country, and by much more even than those of France, because French output rose much more rapidly. It is not a very encouraging picture.

In the treatment of the economic problems of Western Europe, the Survey is, of course, very useful. It is useful to have an organisation capable of transcending the limitations imposed by frontiers. Thus, while each country taken in turn may have congratulated itself on expanding exports, we are reminded here that much of this expansion has taken the form of supplying other European countries with consumers' goods, that much of the effort has gone into sectors which make more consumers' goods, that the basic industrial structure and economic viability of the entire area have not benefited. This was a consequence of the inflationary policy which made sales to other "booming" West European countries profitable. It is not so clear how this could be prevented, save by deflationary policies which tend to damp down the process of growth, and which are justified for other reasons by the authors of the Survey. However, we can only commend them for not pointing a way out of this dilemma. We would like to know what it is!

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School of Economics.

NOTES AND MEMORANDA

MR. HUNTER ON COUNTERVAILING POWER:
A COMMENT

LIKE a child, an idea does not much benefit from a too protective parent, for such a posture confesses, even if it does not induce, a certain lack of vitality in the offspring. Moreover, in the six years and something more since I put forward the notion of countervailing power as a partial solution of the problem of market regulation, though the idea has been repeatedly destroyed, I have been able to contrive some comfort from the fact that the essays devoted to this end continue and, if anything, get longer each year. For these reasons I hesitate to comment on Mr. Hunter's lengthy article in a recent (March 1958) issue of this JOURNAL. But his determined, incisive and lucid negativism merits attention and not alone from those who share his view that irrelevance is a small price to pay for stability in economic ideas. Let me refer to some of Mr. Hunter's principal propositions.

(1) At the outset he ascribes to me the contention that countervailing power "promotes, invariably, lower retail prices." In fact, the final chapters of the book in question are devoted to showing how countervailing power, in a context of high demand, contributes to higher prices and inflation. I want to be generous to Mr. Hunter, so I am willing to assume that, in his critical zeal, he could not pause to finish the book. Yet, though we may certainly countenance intellectual passion, I wonder if we should altogether condone it in an exercise of this kind.

(2) Mr. Hunter asserts, partly on the "weight of material and example devoted to retailing" and partly on his own authority, that the "crucial section" of my thesis is that concerned with distribution. I think this part important; I gave this manifestation of countervailing power a good deal of space, at least in those chapters Mr. Hunter read, because I thought this part of my argument required the most demonstration and proof. But I also consider the application of countervailing power to the factor markets, especially to the labour market, as of crucial social importance. In basing his case on distribution Mr. Hunter was, in fact, suiting his convenience. It would have been more candid to say so.

(3) It is central to his case against countervailing power in the distributive trades that Mr. J. B. Jefferys, a distinguished student of marketing, has not much noticed the use of bargaining power by buyers. This means, evidently, that the theory of markets may not be modified without prior

sanction from the marketing experts. Let me add that, while I am unacquainted with Mr. Jefferys, the singular feature of many American marketing experts is their inability to notice anything.

Let me add, further, that I would not argue that deployment of bargaining power is the only reward to size in marketing. The efficiencies of scale are symbiotic with the rewards to bargaining power. And I wonder if Mr. Hunter, his resistance to novelty notwithstanding, is really willing to argue that the wielding of this power has no appreciable effect on the market solution.

(4) In suggesting that countervailing power has little empirical support from the American context, Mr. Hunter cites as evidence the views of Professor Stigler. The conventional view has no more skilled defender than Professor Stigler; his abilities as distinct from his conclusions command the deepest admiration. But citing Professor Stigler in opposition to an innovation is precisely on a par with the recurrent American discovery that the National Health Service is a failure on the evidence of a fashionable Harley Street physician.

(5) Mr. Hunter's chief contention is that countervailing power is a hodgepodge—"a series of propositions on such a level of generalisation and applied to such a mixture of situations that they are not susceptible to a uniform analytic treatment and cannot yield precise results." This seems to me the triumph of negative myopia. For the same argument can be made with equal effect against the use of the notion of competition—this concept must contend with oligopoly, oligopsony, the various types and conditions of product differentiation, the approaches to pure competition and the several combinations of these. What a hodgepodge! No one would ever contend that competition is susceptible to "uniform analytic treatment" or that such treatment as we have yields "precise results." But Mr. Hunter's frame of mind and method allows and even compels him wholly to overlook this.

(6) Mr. Hunter is right in suggesting that my concern for social harmony—as compared with a single-minded preoccupation with efficiency—creates problems. I can attest personally to the difficulties and frustrations of trying to deal with them. But Mr. Hunter assumes that the existence of such problems is a compelling reason for avoiding them. He is surprised (p. 102) that any theorist should disagree. Is the case for evasion really so strong?

(7) Mr. Hunter concludes that because I devoted a book to the subject there must be *something* to the idea of countervailing power. Contemplating the books that come out each year, even this seems a dubious conclusion. Then, although agreeing that countervailing power makes some contributions to social understanding, Mr. Hunter states that the "thesis has no solid claim to theoretical status." What these words mean no one can know, and unhappily Mr. Hunter does not say. One thing alone Mr.

Hunter makes clear. This subjective and personal test that he applies in such sanguinary fashion has nothing to do with an effort to advance social comprehension.

JOHN KENNETH GALBRAITH

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INDUSTRIAL CAPITAL STOCKS AND ENERGY CONSUMPTION

ECONOMISTS have frequent occasion to estimate the growth of the total or industrial capital stock.¹ Unfortunately, little is known about the capital stocks growth: Most reliable estimates are of very recent origin even for the United States and for the United Kingdom. For most other economies, developed as well as underdeveloped, virtually no estimates exist at all. It has been suggested that an index of energy consumption might be substituted for that of capital stock growth where the latter is unavailable.² The argument is that energy-using equipment, which bulks large in the capital stock and even larger in the industrial capital stock, should increase at rates roughly similar to the increase of the capital stock itself. The use of energy, the argument continues, should in turn be related to the growth in capital-using equipment. Therefore, energy consumption, it is suggested, should be a good indication of the growth of capital stocks and an even better indication of the growth of industrial capital stocks. This line of argument may be easily challenged, however; the share of energy-using equipment in the total capital stock may well change in the course of an economy's development; and, in any case, the equipment is likely to become a more efficient user of energy.³ Forsaking additional deduction, I have turned to the evidence, which may be summarised by correlation coefficients (r) between energy consumption and industrial capital stock indices of 0.9995 for the United States and 0.9893 and 0.9619 for the United Kingdom.

The American experience between 1880 and 1948 is summarised in Fig. 1 and Table I. The first six columns present as many separate indices of the industrial capital stock in the United States. The indices are derived by myself from estimates prepared by the National Bureau for Economic Research, Goldsmith, Kuznets and Douglas. Although the coverage of each index is not precisely the same, I have tried to select the components

¹ See, for instance, Gregory Grossman, "Some Current Trends in Soviet Capital Formation," in National Bureau of Economic Research, *Capital Formation and Economic Growth* (Princeton, 1955).

² *Ibid.*, pp. 171-82.

³ See, for instance, the related argument by Norman Kaplan, who comments on the previously cited study by Grossman in *ibid.*, pp. 201-7.

of each index so as to approximate "Capital in Manufacturing." Since the indices differ among each other, and since each index does not contain an estimate for each of the ten years studied, I have in Column 7 constructed a composite index, weighting each of the six others equally. Column 8 represents an index of energy consumption based on the Department of Commerce index, "Annual Supply of Mineral Fuels and Water Power," published in the *Historical Statistics of the United States*. The sources of energy included are coal, hydro-electric power, gas and petroleum. Exports and imports of these energy sources were sufficiently small to permit use of the

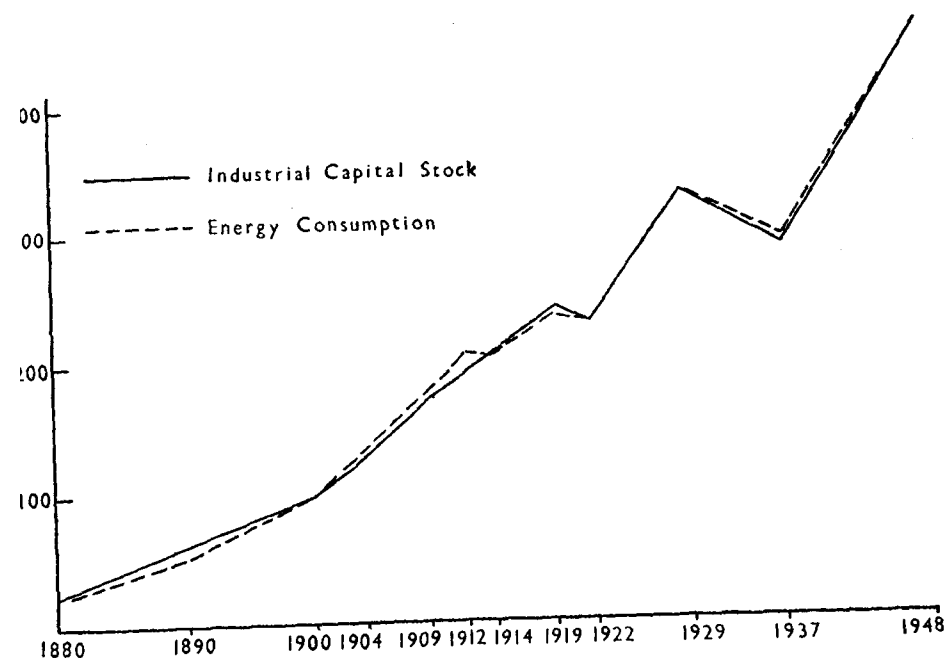


FIG. 1

index of energy supply as an index of energy consumption without alteration. The correlation coefficient (r) between the composite index of industrial capital and that of energy consumption was found to be 0.9995.

Because differences in industrial structure may render the experience of one economy irrelevant to that of another, it would be desirable to examine the evidence from other economies as well. Unfortunately, however, reliable evidence from elsewhere is hard to come by. Until additional studies become available for other economies, we shall have to be satisfied with additional evidence from the United Kingdom only. Even that evidence is somewhat inconclusive, because the available estimates of capital-stock increases measured in current prices differ from among each other, and suitable price deflators differ even more. The existence of these differences

would permit construction of a substantial number of alternative indices of equal or similar reliability. Therefore, Fig. 2 and Columns 1 and 2 of Table II present from among them only the two indices which are most favourable and least favourable respectively, to the demonstration of a correlation between the increase of energy consumption and capital stocks. The first index, derived from Giffen and Stamp, refers to all capital. The second index, derived from more recent estimates by Douglas, refers to industrial capital only. To show that the differences between the two indices is not very likely to be due to differences in coverage, a third index, also by Douglas,

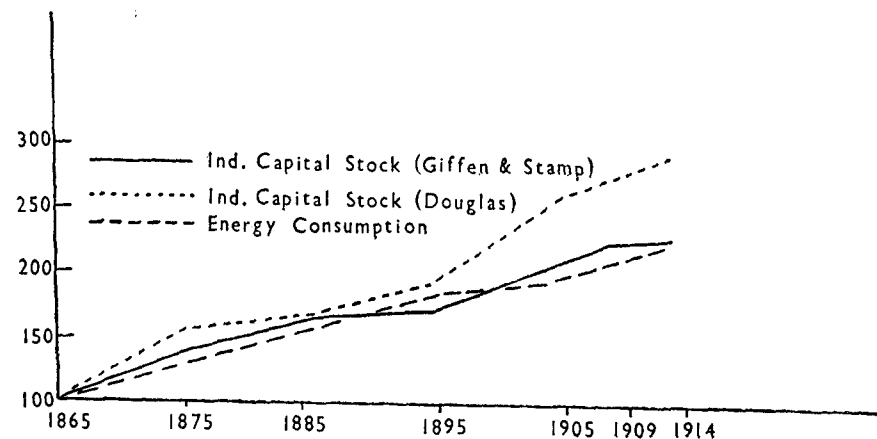


FIG. 2

is presented. The movement of this index is very similar to that of Douglas' index of industrial capital stocks, but since it refers to all capital, its coverage is approximately the same as that of the Giffen and Stamp index. The energy-consumption index was constructed from data on coal and oil consumption (production minus export of coal plus import of oil) from the *Statistical Abstract for the United Kingdom*. The correlation coefficient (r) of the energy consumption index with the "more favourable" Giffen and Stamp index was found to be 0.9893 and that with the less favourable Douglas index 0.9619. The evidence from the United Kingdom may not be conclusive because of the differences among equally reliable capital-stock estimates. But at least the British evidence does not challenge the significance of the very high correlation between the growth of energy consumption and industrial capital stocks in the United States.

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APPENDIX

TABLE I

United States Industrial Capital Stock and Energy-consumption Indices

1900 equals 100

Year.	N.B.E.R. manufacturing.		Goldsmith non-farm.		Kuznets manuf- acturing, etc.	Douglas manuf- acturing.	Compo- site.	Energy supply, actual.
	Total.	Fixed.	Structure and equip.	Equip. only ^a				
1879-80	(1) 27	(2)	(3)	(4)	(5) 30	(6)	(7) 28	(8) 25 ^b
1889-90	62	59			67		62	55
1900	100	100	100	100	100	100	100	100
1904	129	136	122	127		129	130	134
1909	175		152	164		187	175	179
1912			166	182	188	215	194	210
1914	204		177	195		232	210	209
1919	256		197	234		366 ^c	245	239
1922			204	235	232	406 ^c	233	234
1929	350	357	255	289			332	335
1937	307	309	230	255			290	297 ^d
1948	458	492	288	441			463	463

Notes

(a) Includes non-agricultural capital other than manufacturing, mining, etc., and hence rises less fast than the other indices.

(b) Mineral fuels only.

(c) Omitted from the composite index because values differ too much from other indices.

(d) Index cited is 1936-38 average to adjust for the 1937 revival induced temporary rise of the actual index to 328.

Sources and Explanations

All indices are computed by myself from the following data:

- Column 1 National Bureau of Economic Research, Unpublished, Table C: "Total Capital, 1929 Values, Manufacturing, 1880-1948."
- Column 2 Same as Column 1; Table B: "Fixed Capital Excluding Land, Total Manufacturing."
- Column 3 R. W. Goldsmith, *A Study of Saving*. Vol. III. (Princeton, 1956), p. 20: "National Wealth by Type of Asset, 1929 Prices, 1896," "Reproducible Tangible Assets," including: (1) non-farm, non-residential; (2) mining structures; and (3) producer durable equipment (Table W-3, Part 1, Columns 6, 7 and 12).
- Column 4 Same as Column 3, except that only producers' durable equipment (Column 12) is included.
- Column 5 Kuznets, S., *National Product Since 1869* (New York: N.B.E.R., 1946), pp. 218-19, Table IV, 5 and 6, "Value of Real Estate Improvements, Census Dates, 1880-1922, 1929 Prices," the average of the two sets of estimates shown for Mining, Manufacturing, Other Industrial, and Electric Light and Power (Lines 2, 3, 4 and 14) or p. 231, Table IV la, Lines 4, 11 and 15.
- Column 6 Douglas, P., *The Theory of Wages* (New York, 1934), p. 121; "Cumulative Total Capital as Expressed in Terms of Cost and 1880 Prices, 1899-1922" (Table 6).
- Column 7 Average of all columns except Column 3, and omitting the high 1919 and 1922 indices of Column 6.
- Column 8 U.S. Department of Commerce, Bureau of the Census, *Historical Statistics of the United States 1789-1945* (Washington, D.C., 1949), p. 155; "Power—Annual Supply of Mineral Fuels and Water Power; 1819 to 1945" (Water power measured at constant fuel equivalent); except 1948, which is from *Statistical Abstract 1951*, p. 515.

TABLE II
United Kingdom Industrial Capital Stock and Energy-consumption Indices
1865 equals 100

Year	All capital, Giffen and Stamp.	Industrial capital, Douglas.	All capital, Douglas.	Energy consumption.
	(2)	(2)	(3)	(4)
1865	100	100	100	100
1875	140	154	148	130
1885	164	170	186	159
1895	174	198	218	187
1905	213	261	250	202
1909	229	277	265	215
1914	234	296		232

Sources and Explanations

1. Stamp, J., *British Incomes and Property* (London, 1920), p. 406. The estimates for 1865–1909 are Stamp's citation from Giffen's *The Growth of Capital*. The same estimates may be found as well in Douglas (below), p. 661.
2. Douglas P. H., "An Estimate of the Growth of Capital in the United Kingdom, 1865–1909" *Journal of Economic and Business History*, Vol. 11, August 1930, p. 675. Reference is to "Industrial Capital Owned by Residents of the U.K." at 1865 prices.
3. *Ibid.*, pp. 679–81. Reference is to "Capital in the U.K. at 1865 prices."
4. The Index is constructed from coal-consumption (production minus export) and oil-consumption (imports) data in the *Statistical Abstract for the United Kingdom*. Oil was converted to a coal equivalent at 1 ton of oil = 1.54 tons of coal (English tons). The share of oil did not exceed 1.3% of the total of coal and oil combined in any year.

THE MARGINAL EFFICIENCY OF CAPITAL: A FURTHER NOTE

IN the issue of this JOURNAL of September 1958, J. D. Pitchford and A. J. Hagger maintain¹ that there may be several values for the marginal efficiency of capital consistent with a given series of yields and a given replacement cost for a capital asset, *if some of the yields are negative*. From their equation (2):

$$\sum_{i=1}^n Q_i p^i - S = 0$$

where $p = \frac{1}{1+r}$, it is clear that, irrespective of the signs of the Q_i , there must be n roots, and hence n possible values of r . Some of these roots can be ignored as irrelevant; those which are less than zero or are complex. The problem really resolves into the conditions under which there will be one and only one root to the equation greater than zero, or, alternatively, one and only one value for r greater than -1 . Pitchford and Hagger

¹ "A Note on the Marginal Efficiency of Capital," pp. 597–600. This note should really be considered as a development of Pitchford and Hagger's, but, owing to the difficulties of contact between Australia and South Africa, I was unable to communicate its contents to them before their own note was accepted for publication.

have given a sufficient, but not necessary, condition that this should be the case, namely that all the estimated yields should be positive. However, this does not preclude the possibility of negative yields giving the same result.

First, to ensure that there *can be* one positive root to the equation, the *final yield*— Q_n in the equation—must be positive. This is a necessary condition but not sufficient, because a positive Q_n will ensure that there will be an *odd number* of positive roots, whereas a negative Q_n would imply an *even number* of positive roots, or even no positive root at all. Positive Q_n is by no means unreasonable, since presumably an entrepreneur would discard final yields which were negative when considering an investment, and would plan to discontinue the investment in some year which gave an estimated positive yield.

Second, with the assumption of positive Q_n , one may confidently assume that there will be at least one positive root, say p_j . The problem now resolves itself into the conditions for which zero is an upper bound to the roots of the equation:

$$\frac{\sum_{i=1}^n Q_i p^i - S}{p - p_j} = f(p) = 0$$

These conditions are that $f(0)$, $f'(0)$, $f''(0)$, . . . should all be positive.¹ $f(0)$, which equals $-S/-p_j$, is positive. The first, second, etc., derivatives can be shown—by initial differentiation and then mathematical induction—to be:

$$f^k(0) = \frac{k! \left[S - \sum_{i=1}^k Q_i p_j^i \right]}{p_j^{k+1}}$$

In order that these can be consistently positive, then:

$$S > \sum_{i=1}^k Q_i p_j^i \text{ for all } k = 1, 2, \dots, n-1.$$

These then are the conditions under which there can be one and only one positive root to the original equation.

Thus, if there is one positive root to the equation, and it discounts the first k yields, where k varies from 1 to $n-1$, to something less than the replacement cost of the investment, then it is the only positive root. Since we have assumed that Q_n —or the final yield—is positive, there must be at least one positive root. The conditions could be plotted as in Fig. 1, where

$\sum_{i=1}^k Q_i p_j^i$ is graphed against k . There is one, and only one, positive root if the graph is consistently below the horizontal line a distance S above the

k -axis until $k = n$, where, according to the original equation, the graph must rise to cut this line.

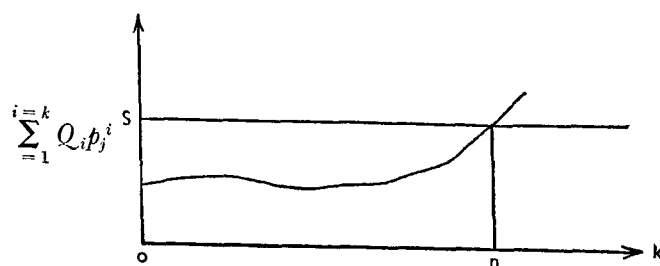


FIG. 1

However, from any set of n yields, chosen at random but with Q_n positive, there seems to be no reason why we should not get a graph such as that in Fig. 2, where $\sum_{i=1}^k Q_i p_j^i$ exceeds S at some value or values of k .

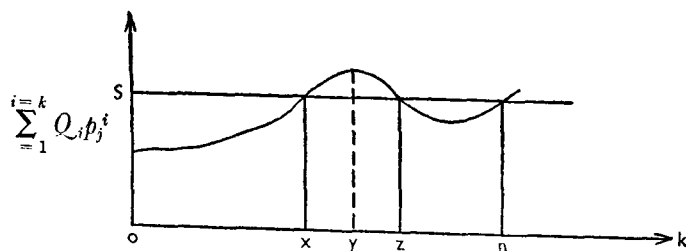


FIG. 2

In this case, there will be as many positive roots as the number of times the graph cuts the horizontal line at S . This occurs three times in Fig. 2, namely where k equals x , z and n . The graph reaches a maximum when $k = y$. If all yields above Q_y were discarded then a smaller value of p than p_j would reduce the discounted sum of yields from Q_1 to Q_y to equality with S . This smaller value of p would be the one, and only one, positive root to the equation:

$$\sum_{i=1}^y Q_i p^i - S = 0$$

Since it is a smaller value of p than p_j , it represents a larger value of r . It will be found that this is always true; namely, that if the given set of yields provides several positive roots to the discounting equation, then an r -value greater than that corresponding to any of these roots will be found by discarding more distant yields until the remainder reduces to a set which provides a discounting equation with one and only one positive root. If the investor is aiming at a maximum rate of profit on his investment—a

maximum r —then this will always cause him to discriminate between different “lengths of life” for the investment, choosing that length which includes the maximum number of consecutive yields which are still consistent with a discounting equation with one, and only one, positive root.

The confusion in the Pitchford and Hagger note derives from ignoring the “life” of the potential investment as a possible variable in the maximising process: assuming, as I am sure has so often been done, that the life is, in some sense, technologically “given.”

The conditions given above allow us to determine the restrictions on the negativeness of any yield. For example, no Q_r may be so negative that, although the sum of the discounted yields up to $k = r$ is less than S , that up to $k = r - 1$ is greater than S . However, this is hardly relevant, since the maximising process, by restricting the series of yields, will eliminate those which are too negative; thus the problem of the negativeness of yields really falls away.

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MR. WORSWICK ON THE PRODUCTION POSSIBILITY FUNCTION

1. MR. WORSWICK has recently offered a simple proof of the proposition that the production possibility function (P.P.F.) is convex from above.¹ Unfortunately his demonstration is inconclusive. In this note we first briefly summarise his analysis, drawing attention to a particular assumption made in it. We then argue that this assumption contradicts certain of the conditions of his model. Finally, we use this argument first to show how the contradiction renders his proof invalid, and then as the basis for an alternative proof of the convexity of the P.P.F.

2. There are fixed total amounts of labour and land available, which can produce OA if all devoted to clothing and OB if all devoted to food (Fig. 1). The production of each commodity is subject to constant returns to scale. It is possible, but not necessarily optimal, to move along the straight line AB if the labour-land ratio for each commodity is always kept equal to the ratio of total labour to total land. Mr. Worswick points out (p. 749) that if the ratio of the marginal physical products (M.P.P.) of labour and land is the same in food as in clothing at any point on AB , it must be the same in food as in clothing at all points on AB . Hence the P.P.F. is either AB itself or lies wholly above AB except at A and B .

¹ G. D. N. Worswick, “The Convexity of the Production Possibility Function,” *Economic Journal*, December 1957, pp. 748–50.
No. 273.—VOL. LXIX.

therefore lie above it, and so on an isoquant representing a higher level of output than P''' or Q''' . It follows that:

$$F(L_Q, T_Q) + F(L_R - L_Q, T_R - T_Q) < F(L_R, T_R) \quad (2)$$

Clearly if the labour-land ratio were the same at Q'' as at R , so that P' , Q'' and R all lay on the same ray, or if isoquants were straight lines, relation (2) would be false, and equation (1) would hold.

An exactly similar argument for clothing would show that, given conditions (a) and (b):

$$C(\bar{L} - L_R, \bar{T} - T_R) + C(L_R - L_Q, T_R - T_Q) < C(\bar{L} - L_Q, \bar{T} - T_Q) \quad (2')$$

5. We now return to Mr. Worswick's argument. The point M in Fig. 1 represents the output of (L_Q, T_Q) in food and $(\bar{L} - L_R, \bar{T} - T_R)$ in clothing. It follows from (2) that $(L_R - L_Q, T_R - T_Q)$ will produce not MR food but only, say, MR' , and from (2') not MQ clothing but only, say, MQ' . Mr. Worswick's "down-the-line" possibility function is therefore not QR but $Q'R'$. The existence of points on or above the latter does not imply the existence of points on or above the former.

We can, however, use the argument of Section 4 to give an alternative proof of the convexity of the P.P.F. Suppose that we could use $(L_Q + L_R)$ labour and $(T_Q + T_R)$ land in food and $[(\bar{L} - L_Q) + (\bar{L} - L_R)]$ labour and $[(\bar{T} - T_Q) + (\bar{T} - T_R)]$ land in clothing. The resulting output of each commodity would be greater than the sum of its outputs at Q and R . Suppose now that we were to use $\frac{L_Q + L_R}{2}$ labour and $\frac{T_Q + T_R}{2}$ land in food and $\frac{(\bar{L} - L_Q) + (\bar{L} - L_R)}{2}$ labour and $\frac{(\bar{T} - T_Q) + (\bar{T} - T_R)}{2}$ land in clothing. (This would exactly use up all the available labour and land.) By constant returns to scale, the resulting output of each commodity would be greater than the arithmetic mean of its outputs at Q and R . The resulting point in Fig. 1 would lie above the straight line QR . Since Q and R are any two points above AB on the P.P.F., this establishes its convexity.

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RAILWAYS OR ROADS?

ON November 6 Sir Brian Robertson, speaking for the Transport Commission, said that British Railways would probably show a deficit of £85 million for 1958. What is the cause of this financial failure? It cannot be put down to less travelling or a smaller movement of goods than in 1913,

when the accounts of the old Railway Companies showed a profit of £50 million in gold. As I see it, the change is due solely to the competition of road motor transport; for road transport, offering conveyance from door to door, is obviously more convenient than railway transport from station to station. But this does not explain how the owners of motor lorries and coaches can quote lower charges than British Railways.

No one can suggest that driving rubber-tyred lorries and coaches up and down hill on macadamised roads requires less man-power and motive power (or means less wear and tear) than conveying the same amount of goods, or number of passengers, in steel-tyred trucks and passenger trains along graded tracks with steel rails. How many men and lorries or coaches are required to carry as much goods or as many passengers as one railway train? No General or Contractor, having to move large quantities of materials or large bodies of men, builds a road and runs rubber-tyred lorries along it. He lays down rails, and runs trucks along them.

In all forms of transport the governing factor is the cost of the "Permanent Way." This explains the cheapness of water transport, water does not wear out, although canal banks and locks may need repair; and ocean transport requires lighthouses and ports. In the case of land transport the track is more important than the vehicle, except over hard level deserts, or over snow. Most people, when they travel by railway, or send parcels, think of the train as the thing they pay for. In fact, the train is only the last-but-one (the arrival station) of a series of services offered by the railway-stations, tracks, signals, lights, police and so on. In the case of road transport the lorry or coach is the thing paid for; the owner of the vehicle finds his permanent way—the road—constructed, repaired, lighted, signalled and policed by the rate-payers. That is why he can charge less than British Railways.

Another financial point must be mentioned. Railways, unlike other property, have been rated on their ascertained profits; in the days before nationalisation the railway shareholder was often spoken of as the biggest rate-payer. It was equally true that the rate-payer was or might be, the biggest railway shareholder, since part of his Council's revenue depended on the profits of the railway running through his town. Possibly Sir Brian Robertson might claim a lower assessment in Cambridge City and County, on the ground that the Eastern part of British Railways is not making a profit. Tramways, too, are rated on their tracks; so it seems odd that London Transport should have torn up its rails and is running a motor omnibus service in place of the old trams, which were so notable a feature of the Thames Embankment. No doubt London Transport is saving the cost of maintaining its tracks and of paying rates on them; but this means that part of the cost is being borne by the London rate-payer.

It is now the declared policy of the Transport Commission to close railways which "do not pay," especially the smaller branch lines. That would

have seemed reasonable enough in the days of the old Companies. But surely it is part, and an important part, of the case for "Nationalisation" that you consider the benefit of the community first and profit-making a bad second, or even sordid? What would people say if the Postmaster General announced that he was giving up some rural collections and deliveries because they do not pay (which is evident)? There must be many areas where the services of water, electricity and gas do not pay.

In the old days railway charges for goods were based on the principle of "charging what the traffic will bear." Sometimes this was represented as a form of extortion, or bleeding the customer. In fact, it was more like the old plan by which doctors asked high fees from patients in big houses and charged low fees from patients in small houses; in the same way the stalls in a theatre pay more in order that the pit and gallery may pay less.

There can be no suggestion that British Railways are "over-capitalised." Any "water" was squeezed out in 1921, and property which originally cost £1,100 million in gold and yielded a revenue of £50 million in 1913 was acquired by Dr. Dalton for an annual payment of £43½ million in paper money. It is an odd fact that the licence duties charged for goods vehicles are actually graduated on the *degressive* principle, *i.e.*, 1 ton £15, 3 tons £35, 4 tons £50. Surely the Income Tax principle of *progression* should be adopted, so that each ton pays more than the last! The damage to roads increases faster than the weight, so that the duties ought to rise in proportion; indeed, the heavy lorries and oil tankers ought to be taxed off the roads. Cars and motor cycles should also be taxed on the progressive principle.

How far the petrol duty paid by motorists of all kinds ought to be regarded as payment for the use of the roads is a difficult question. Our large imports of oil mean a heavy drain on our dollar resources or balance of payments. It is sometimes contended that the persons who pay this oil duty have a special right to decide how the money should be spent, and want it to be diverted to road upkeep and improvement. Other persons also use imported oil, British Railways included, and the new diesel-driven trains are popular.

So far I have said nothing about road casualties, but they are undoubtedly a part of the cost of road transport, and have been estimated at £150 million a year. If rules for the safety of the public, which Parliament imposed on the old railways, were imposed on road traffic, road transport, as we have it now, might come to an end.

Cambridge.

J. E. ALLEN

CURRENT TOPICS

THE Council and Fellows of the Royal Economic Society offer their warmest congratulations to Sir Roy Harrod on the conferment of a Knighthood in the New Year Honours List. In addition to all his notable contributions to economics, he has served as one of the joint editors of this JOURNAL since the resignation of Lord Keynes in 1944.

THE Council and Fellows of the Royal Economic Society will wish us to express their very sincere congratulations to Professor Robbins, who more recently received the honour of a Life Peerage. He has served on the Council of the Royal Economic Society since 1931 and was President in 1954-56. His contributions both to the work of the Society and to the study and teaching of economics are so well known that we need not attempt to list them.

IN the same announcement of Life Peerages we were delighted also to see the name of Sir Edwin Plowden, who, after studying economics in Cambridge and a period of activity in the City, played a central part in the organisation of war production and subsequently, as head of the Central Economic Planning Staff, in the framing of economic policy in the critical years after the War. More recently, as Chairman of the Atomic Energy Authority, he has continued to carry heavy responsibility for one of the most important branches of economic activity.

WE record with great regret the death on January 14, 1959, of Professor G. D. H. Cole, formerly Chichele Professor of Social and Political Theory in the University of Oxford. He was a notable contributor to the study of social history and of labour problems and a great stimulus to economic thought and discussion in those and many other fields, both in Oxford and in wider national circles. He served on the Council of the Royal Economic Society from 1942 to 1955.

WE record with great regret the death of Mr. F. A. Burchardt, Fellow of Magdalen College, Oxford, and Director of the Oxford Institute of Statistics. Mr. Burchardt first came to England in 1935 as a refugee from

Nazi oppression in Germany, where he had made a great reputation as an independent liberal thinker. In Oxford he made a second and comparable reputation as a teacher and as Director of the Institute of Statistics, and the continued success of the Institute in recent years has owed much to his guidance.

PROFESSOR B. R. WILLIAMS, of the University College of North Staffordshire, has been elected to the Robert Ottley Professorship of Economics in the University of Manchester. Mr. J. Johnston has been elected to the Chair of Econometrics in the same university.

THE Council of the Royal Economic Society has decided to reduce the price to Fellows of the edition of *Jeremy Bentham's Economic Writings*, edited for the Society by Dr. W. Stark. The three volumes will in future be available to Fellows at the price of 30s. a set, or 12s. 6d. for individual volumes. Application, including a remittance, should be made to the Secretary, Royal Economic Society, 21 Bentinck Street, London, W.1.

AN unbound stock of Cantillon's *Essai sur la Nature du Commerce en Général*, edited by Henry Higgs and published for the Society originally in 1931, has recently come to light in the hands of the printers. It has been decided to put these on sale through Messrs. Frank Cass, 91 Southampton Row, London, W.C.1, at a price of 25s. to members. An application form for this and for Dr. Stark's edition of Bentham is included in this issue.

Corrigendum: In Professor Kurihara's article in the September 1958 issue of this JOURNAL, the sentence beginning in line 28 of p. 480 should read:

"Thus the author of the *General Theory* proclaimed his sympathy with Gesell's book, the purpose of which 'as a whole may be described as the establishing of an anti-Marxian Socialism.'"

List of Fellows and Addresses. A new list of Fellows and their addresses is in course of preparation. The Secretary will be most grateful if all Fellows will inform him as soon as possible if the address to which this copy of the ECONOMIC JOURNAL was posted was not the present permanent address which they would wish to be included. May any corrections be sent to The Secretary, Royal Economic Society, 21 Bentinck Street, London, W.1?

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- No. 1-2, 1958. *Papers Read at the Third Congress of the Union of the Yugoslav Association of Economists, Zagreb, 29-31 May 1958. The Factors of Production and the Production Orientation of Yugoslavia:* N. COBELJIĆ. *The Optimum Rate of Economic Growth:* R. STOJANOVIĆ. *The Place of Population in the Future Production Orientation of Yugoslavia:* M. MACURA. *Scientific and Methodological Questions on the Choice of Production and Investment Variants:* A. ORTHABER. *The Optimum Investment Volume Within National Income:* A. BAJT. *The International Division of Labour as a Criterion of Production Policy:* P. MIHAILOVIĆ. *Internal and External Production Policy Criteria:* V. PERTOT. *Personal Consumption as a Criterion for Production Policy:* V. TRČKOVIĆ. *Discussion. Some Aspects of the Effects of Planning and the Market upon the Yugoslav Economy:* B. JELIĆ. *The Problem of the Planned Guidance of Accumulation in Yugoslavia:* J. SIROTKOVIĆ. *Economic Development and Price Policy:* B. PILIĆ. *Some Problems of the Insufficiently Developed Regions:* K. MILJOVSKI. *Methods of Financing the Development of the Undeveloped Regions of Yugoslavia:* A. GRLIČKOV. *Discussion. Other Proceedings of the Conference.*

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- No. 7, 1958. *Against the Revisionists' Misrepresentation of the Economic Role of the State in a Popular Democracy*: V. RAUSSER. *A Mighty Weapon in the Struggle against Revisionism*: I. ADAM. *The Bankruptcy of the Bourgeois Theory of Crisis-proof Capitalism*: V. PERLO. *Many-sided Development—the Main Road Towards the Economic and Organisational Consolidation of Collective Farms*: C. COJOCARU and P. AVRAMESCU. *Grain Output in Our Country and Its Prospects*: S. HARTIA. *Some Problems in the Development of the Food Industry*: C. TEODORU. *On the Economic Category of Obsolescence and the Amortisation of the Means of Labour*: C. NEGULESCU. *Some Aspects of the Economic Efficiency of Industrial Investments*: R. SÁVEANU and H. BERCU.

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NEW BOOKS

British

Agricultural Policy in the European Economic Community. PEP Occasional Paper 1. London: Political and Economic Planning (16 Queen Anne's Gate, S.W.1), 1958.

Pp. 26. 2s. 6d.

[This is the first of a series of Occasional Papers which PEP is now publishing from its study, funded by the Ford Foundation, of Britain and the European Market. It describes the present agricultural policies of the six common market countries and the provisions relating to agriculture in the Rome Treaty. Possible effects on other countries are reserved for treatment in a later paper.]

AHMAD (NAFIS). *An Economic Geography of East Pakistan*. London: Oxford University Press, 1958. 9½". Pp. xiv + 361. 50s.

[This is a well-written, well-produced work, with fifty-six maps and fifty-nine tables. In Part I physical setting and geographical background is described. There is a historical survey of agriculture, industry, trade and transport in the second part and, in the third, the economic geography of the present day is discussed. The historical section is too brief to be penetrating, but on post-war economy there is a great deal of valuable information. The dominance of agriculture, of modern industry and pressure of population are well illustrated. The author has produced a descriptive study of merit. He is little concerned with economic analysis or policy.]

BERNAL (J. D.). *World Without War*. London: Routledge & Kegan Paul, 1958. Pp. xi + 308. 25s.

[To be reviewed.]

BROWN (LUCY). *The Board of Trade and the Free-trade Movement, 1830-42*. London: Oxford University Press, 1958. 9". Pp. 245. 30s.

[The subject of this scholarly monograph is the part played by Board of Trade officials in the free-trade movement up till Peel's reform of the tariff in 1842. The leading officials at the Board were the movement up and made full use of their position to advance their views. Miss Brown gives an interesting and detailed account of the political environment in which the Board worked and of the personalities involved. She then describes the day-to-day activities of the Board and the collection of statistics and in commercial negotiation. Finally, she discusses the part played by the Board's officials in the attack on the tariff. This part was most prominent in the organisation of the well-known Select Committee on Import Duties in 1840 and the dissemination of its findings. The main interest of Miss Brown's work is as a study in administration and the history of opinion, and much useful and well-documented material on British overseas trade during the period is also provided.]

DENMAN (D. R.), SWITZER (J. F. Q.) and SAWYER (O. H. M.). *Bibliography of Rural Land Economy and Landownership, 1900-1957*. A full list of works relating to the British Isles and selected works from the United States and Western Europe. Cambridge: Cambridge University Department of Estate Management, 1958. 10". Pp. xii + 412. 35s.

[The aim of this bibliography is to include all relevant items published in Britain from 1900 to 1957 and a selection of books and articles published elsewhere. The main body of the work consists of a classified list, arranged under the following general headings, with subdivisions: general works; rural economy; land economy; estate economy; research and education; and reference works. This is followed by an unclassified list of the same items arranged alphabetically by authors.]

DOBB (M. H.). *Capitalism Yesterday and Today*. Socialism Today Series, No. 1. London: Lawrence & Wishart, 1958. 7½". Pp. 63. 2s. 6d.

[In this pamphlet, which is the first of a new series, Mr. Dobb sets out to give a non-technical account in Marxian terms of the nature and origin of capitalism and to discuss certain of its present-day features. The most interesting part of the pamphlet is perhaps that in which he discusses what are the causes of cyclical depressions under capitalism and whether post-war experience should be taken to imply that these forces have ceased to operate. Also interesting is his brief treatment of the relation between Marxian forecasts and recent statistical findings regarding long-term trends in profits, wages and capital accumulation.]

DONNITHORNE (A. G.). *British Rubber Manufacturing*. An economic study of innovations. Industrial Innovations Series. London: Duckworth, 1958. 9". Pp. 159. 25s.

[To be reviewed.]

EINZIG (P.). *The Economic Consequences of Automation*. London: Secker & Warburg, 1956. 9". Pp. vii + 226. 21s.

[Dr. Einzig is a whole-hearted advocate of automation who holds that it is of vital importance that we should not lag behind the U.S.S.R. in adopting these new techniques. He puts forward the claim in this book that if there is not an Automation Economics, there ought to be. He stresses in particular the pressure on firms that have adopted automation to keep output up in the face of a fall in demand. The consequence of this may be a steeper fall in prices or, alternatively, a larger rise in inventories than would occur in cases where a reduction in output raises fewer problems. Dr. Einzig takes a broad view of his subject, and considers the impact of automation on, and the impact on automation of, business cycles, monetary policy, wages policy and the problems of under-developed countries. The analysis is very general, but most of Dr. Einzig's conclusions are sensible ones. It is difficult, however, to ascribe to automation as important a role as Dr. Einzig does. The reader might have been more convinced if the book had contained a hard analysis of some of the points that Dr. Einzig merely touches on. In particular, Dr. Einzig's argument that vast increases in production could be brought about by the rapid adoption of automation should have been balanced by a sober assessment of the costs involved. Similarly, his assertion that failure to adopt automation will lead to ruin in the balance of payments should have been the subject of a much more detailed analysis than it in fact receives.]

FRIIS (A.) and GLAMANN (K.). *A History of Prices and Wages in Denmark, 1660-1800*. Vol. I. London: Longmans for the Institute of Economics and History, Copenhagen, 1958. 11". Pp. xv + 350. 50s.

[This work represents the results of a major programme of research that has been carried on since the end of the War. This first volume contains an extended treatment of Danish currency, weights and measures during the period, and data on the assizes of Copenhagen and on Copenhagen market prices. The tables are accompanied by discussion of the institutional background.]

GILL (A. H.). *Notes on the Life and Use of Certain Farm Machines*. Department of Agricultural Economics, University of Reading, Miscellaneous Studies No. 14. Reading: Department of Agricultural Economics, University of Reading, 1958. 10". Pp. 23. 2s. 6d.

[This brief paper, based on a sample survey, illustrates an interesting application of certain questions about optimum utilisation of capital to the case of agricultural machinery. It is shown that for the small farmer wear and tear is a relatively unimportant consideration, because of the low intensity of use, and obsolescence is what effectively determines the life of a machine. This is not true for larger farmers. On the other hand, it appears that it is more important for the larger farmer than for the small one to have an efficient and up-to-date machine, because the greater intensity of use increases the importance of running costs relatively to the overhead cost of the machine. The author briefly considers various arrangements by which the prevalent tendency for farm machinery to be under-utilised might be lessened.]

HAGENBUCH (W.). *Social Economics*. Cambridge Economic Handbooks. Welwyn: Nisbet; London: Cambridge University Press, 1958. 7½". Pp. xvi + 320. 12s. 6d.

[To be reviewed.]

HENRY (H.). *Motivation Research*. Its practice and uses for advertising, marketing and other business purposes. London: Crosby Lockwood (26 Old Brompton Road, S.W.7), 1958. 9". Pp. 240. 30s.

[The author is Director in charge of research at McCann-Erickson Advertising Ltd., London. The aim of his book is essentially practical. It describes the nature and commercial uses of motivation research clearly and with much down-to-earth common sense. The brief discussion of the broader economic and ethical issues in the concluding chapter is not profound, but the author has given abundant material for reflection on these aspects in the main part of his exposition.]

HUNT (K. E.) and CLARK (K. R.). *The State of British Agriculture, 1957-8*. Oxford: Agricultural Economics Research Institute, University of Oxford, 1958. 13". Pp. 171. 10s.

[Like its predecessors in recent years, this publication provides a convenient assembly of statistical data relating to British agriculture and its economic environment. The statistics for the last three or four years are given in tables, and longer-term movements are shown in charts. There is also a certain amount of explanatory text and comment, but particulars of legislation are not covered. Information on government agricultural policy is given in the Institute's other publication, *The Agricultural Register*, which is complementary to the present volume.]

INTERNATIONAL BANKING SUMMER SCHOOL. *The Future Organisation of Banking*. A series of lectures delivered at the Eleventh Session, St. Andrews, September 1958. Edinburgh: Blackwood for the Institute of Bankers in Scotland, 1958. 8½". Pp. 161. 20s.

[The lectures printed in this volume include "The Prospect for Banking," by Sir John J. Campbell, "Forms of Bank Credit," by Professor A. D. Campbell, "Banking in Developing Countries," by Professor A. K. Cairncross and "Training of Bank Specialists in the U.S.S.R.," by I. V. Chernov, Deputy Chairman of the State Bank of the U.S.S.R.]

JACOBSSON (PER). *Some Monetary Problems—International and National*. Edited by E. E. JUCKER-FLEETWOOD. Basle Centre for Economic and Financial Research, Series B, No. 4. London: Oxford University Press, 1958. 9". Pp. 374. 42s.

[To be reviewed.]

JOHNSON (H. G.). *International Trade and Economic Growth*. Studies in pure theory. London: Allen & Unwin, 1958. 9". Pp. 204. 25s.

[To be reviewed.]

KELF-COHEN (R.). *Nationalisation in Britain*. The end of a dogma. London: Macmillan, 1958. 9". Pp. x + 310. 25s.

[To be reviewed.]

LANDES (D. S.). *Bankers and Pashas*. International finance and economic imperialism in Egypt. London: Heinemann, 1958. 9". Pp. xvi + 354. 30s.

[To be reviewed.]

LEE (SIR FRANK). *The Board of Trade*. The Stamp Memorial Lecture. London: The Athlone Press, University of London (distributed by Constable & Co., 12 Orange Street, W.C.2), 1958. 8½". Pp. 33. 3s. 6d.

[In this lecture the Permanent Secretary of the Board of Trade gives an informal sketch of the varied activities of his Department and of the aims which it pursues.]

LEIGHTON-BOYCE (J. A. S. L.). *Smiths the Bankers, 1658-1958*. London: National Provincial Bank, 1958. 10". Pp. xiii + 357.

[This is a history of the group of banks controlled by the Smith family, which were amalgamated into the Union of London and Smiths Bank in 1902 and then into the National Provincial in 1918. The original business was founded in Nottingham in 1658, and other branches, which in due course became largely independent of each other, were later set up in London, Lincoln, Hull and elsewhere. Parts of Mr. Leighton-Boyce's history are of mainly family or antiquarian interest, but much useful information on banking practice is also included, especially with regard to the eighteenth century. By the end of the nineteenth century, certain branches of the business were showing signs of going the way of Marshall's trees in the forest; but other branches remained

vigorous and enterprising, and members of the family have continued to play a very prominent part in banking and in other City institutions. A direct descendant of the Smith who founded the original Nottingham bank in the seventeenth century was Chairman of one of the Big Five seven generations later—a truly striking exemplification of the dynastic principle in the higher reaches of the British banking system.]

MARSH (D. C.). *The Changing Social Structure of England and Wales 1871–1951*. London: Routledge & Kegan Paul, 1958. 8½". Pp. xiv + 266. 28s.

[To be reviewed.]

MARTIN (ANNE). *Economics and Agriculture*. London: Routledge & Kegan Paul, 1958. 9". Pp. viii + 169. 21s.

[Reviewed in this issue.]

MEREDITH (H. O.). *Economic History of England*. A study in social development. Sixth edition by C. ELLIS. London: Pitman, 1958. 9". Pp. viii + 440. 20s.

[The parent of this book was reviewed by Sir John Clapham in this JOURNAL, September 1909. Since then there have been several reprints and six editions, but, apart from bringing the later parts up to date, the editors have made little change. Most of Clapham's objects of criticism stand unaltered. On p. 181, for example, it is still stated that "the success of British arms during the Seven Years War . . . must have advanced the Industrial Revolution by half-a-century." Cunningham and Dicey remain the author's chief intellectual creditors and one would have liked more incorporation of the results of recent research. The value of a book which covers the whole of English economic history in less than 450 pages must remain doubtful, but, of its kind, Meredith's is surely a classic.]

Nationalisation. A report by the Federation of British Industries. London: Federation of British Industries (21 Tothill Street, W.1), 1958. 8½". Pp. 68. 5s.

[This statement of the case against further nationalisation gives much weight to the efficacy of competition in what is referred to throughout as "free industry" (as contrasted with State-owned industry). This, it is claimed, is adequate to prevent the various alleged deficiencies in the private enterprise system which nationalisation is designed to eliminate.]

POUNDS (N. J. G.) and PARKER (W. N.). *Coal and Steel in Western Europe*. The influence of resources and techniques on production. London: Faber & Faber, 1957. 9". Pp. 381. 45s.

[This book is concerned only in the last chapter with the European Coal and Steel Community; its major intention is to provide a historical industrial background to that development. The first two-thirds of the book is by a geographer, Professor Pounds, and he is concerned with the history of the iron and steel industries in the eighteenth and nineteenth centuries. He traces the influence of resources and the spread of techniques from the point of view of their effect on regional concentration. The approach is therefore, as might be expected, primarily geographical, and the disadvantage is its inevitable bittiness. Professor Pounds brings into his purview at least summarily not only what his colleague understands by "Western Europe" but also Great Britain, Silesia, Russia, Scandinavia and other countries. In places the text is little more than a catalogue of dates and names, and as such it is not easy reading. The last third of the book is by an economist. Professor Parker has set himself an easier task in tracing "the response to economic opportunity" of the Western European coal and steel industries as a whole and in the twentieth century only. He has a theme in the trend of growth and the influences on it, and he is an enthusiast, and all this makes his section of the book much more readable. His work on the size distribution of firms, patterns of plant integration and cartel organisations is interesting. As a whole, however, this book is disappointing. Its different sections are not integrated, and it is difficult to see what class of reader it is aimed at. It is in very summary form throughout; the real scholar in this field will want to go back to the many valuable references—and their extent shows the immense amount of work behind this book. At the same time it is not a book to appeal to the general reader.]

ROBINSON (JOAN) and ADLER (S.). *China: An Economic Perspective*. Fabian Tract No. 314. London: The Fabian Society, 1958. 8½". Pp. 20. 1s. 6d.

[This is based on a visit which the authors paid to China in the autumn of 1957. It draws on their personal impressions and on the statistics which they were able to collect. It provides a vivid and informative picture of recent developments in China. These are discussed in relation to the problems of economic growth and planning generally, with due regard also for social implications. Issues discussed include the pace of development, the organisation of agricultural co-operative and of industry, urbanisation and population growth.]

SKILLICORN (M. D.). *The Measurement of Efficiency in Co-operative Retailing*. Co-operative College Papers, No. 5. Loughborough: Education Department, Co-operative Union, 1958. 9". Pp. 68. 1s.

[The main part of this paper consists of a study of sales, costs and profits in the dry-goods trade of a particular retail Co-operative Society. Stress is placed on the reduction in retailing cost per unit brought about by an increase in the volume of sales. This, it is suggested, may indicate a need for Co-operative Societies to reconsider their pricing policies in some departments. The main study is followed by briefer treatment of efficiency in the food trades and by suggestions for further research.]

WILLIAMS (SHIRLEY). *The Common Market and its Forerunners*. Fabian International Bureau Research Series 201. London: Fabian Society (11 Dartmouth Street, S.W.1), 1958. 8½". Pp. 41. 3s.

WILLIAMS (SHIRLEY). *Britain and the Free Trade Area*. Fabian International Bureau Research Series 202. London: Fabian Society (11 Dartmouth Street, S.W.1), 1958. 8½". Pp. 33. 3s.

[These two pamphlets were written before the recent breakdown of the Free Trade Area negotiations. In the first, the author describes the earlier developments of the European idea in the economic sphere, paying particular attention to the experience of the European Coal and Steel Community. She then proceeds to a critical discussion of the provisions of the Rome Treaty. In the second pamphlet she outlines the aims of the Free Trade Area proposal and the difficulties it involves. Her point of view is that although socialists may find much that is not to their taste in the European movement in the form it has taken to date, they should not on that account retreat into nationalism but should rather accept the need for a breaking down of national barriers and should try to pursue their aims in the new wider context.]

WILSON (C. H.) and READER (W. J.). *Men and Machines*. A history of D. Napier & Son, Engineers, Ltd., 1808–1958. London: Weidenfeld & Nicolson, 1958. 10". Pp. xi + 187. 35s.

[This book shows how a business history, when skilfully done, can be of interest both as a story in itself and as a study in economic organisation. The life of the firm of Napier, as described by Mr. Wilson and Mr. Reader, has fallen into a number of rather distinct phases. In its early days it produced printing-machines, bullets, weighing-machines and other precision engineering products. In the second half of the nineteenth century the firm moved away from printing machinery, and by the 1880s the eccentricities of the second-generation Napier who controlled it caused a shrinkage in the volume of its business as a whole. Then in the twenty years before the First World War it turned to a completely new line, motor cars, and for a while was one of the leading firms in the industry. Its orders for cars were already showing some signs of decline by 1914, and did not recover after the War. During the War, however, the firm had been induced, without much enthusiasm at first, to take up the construction of aero-engines, and this thereafter became its principal activity. The fortunes of the firm and the nature of its business have reflected very markedly the personalities of the three generations of Napiers who ran it. When the third of these died in 1931 no successor was at hand within the family, or indeed within the firm at all. After a period under the control of outside financial interests, the firm passed in 1942 into the English Electric group, which could provide the resources needed in the new circumstances of the aircraft industry. If any criticism may be made of the highly readable history which Mr. Wilson and Mr. Reader have written, it is that in view of the extreme importance of personalities in the firm's history, it would be interesting to be told more about why in the second and third generations control came to pass to J. M. Napier and M. S. Napier respectively, neither of whom was an eldest son, rather than to another member of the family.]

WINCH (P.). *The Idea of a Social Science, and its Relation to Philosophy*. Studies in Philosophical Psychology, edited by R. F. HOLLAND. London: Routledge & Kegan Paul; New York: Humanities Press, 1958. 7½". Pp. 143. 12s. 6d.

[To be reviewed.]

Australian

AUSTRALIAN AGRICULTURAL ECONOMICS SOCIETY. *Proceedings of the Conference of Agricultural Economists, Held at Sydney on February 20 and 21, 1957*, edited by P. C. BRUCE. Sydney: Australian Agricultural Economics Society, 1958. 9½". Pp. 110.

[The contents of this are as follows: "Agriculture in the Balance of Payments," by J. G. Crawford; a Symposium on Contemporary Agricultural Economics in Australia; "Uses and imitations of Farm Surveys," by H. P. Schapper; "Resource Productivities and Production Functions," by F. G. Jarrett; "Quality in Relation to the Marketing of Primary Products at Home and Abroad," by T. H. Strong; "Capital Formation in Australian Agriculture," by F. H. Gruen.]

No. 273.—VOL. LXIX.

New Zealand

Food and People. A series of articles on world population, food production and consumption. Wellington: Reserve Bank of New Zealand, 1958. 10½". Pp. 44. 3s. 6d.

[These articles are reprinted from the Bulletin of the Reserve Bank of New Zealand. They are concerned with trends in world supply and demand for food and their implications for New Zealand. They bring together much valuable information. As the authors point out, the many uncertainties involved make it impossible to draw any firm conclusions as to the best way in which production in New Zealand should in the future be divided between exports and import-substitutes. The need for New Zealand to broaden her markets and diversify her exports, however, emerges clearly.]

Canadian

HAWTHORN (H. B.), BELSHAW (C. S.) and JAMIESON (S. M.). The Indians of British Columbia. A study of contemporary social adjustment. Toronto: University of Toronto Press, and the University of British Columbia (London: Oxford University Press), 1958. 9½". Pp. ix + 499. 80s.

[This massive work is the outcome of a large-scale field study. It covers all aspects of the social and economic life of Indians in reserves in British Columbia. The aim throughout is to use the findings as a basis for policy recommendations. The first two of the authors named are anthropologists, the third an economist.]

MACFARLANE (D. L.) and BLACK (J. D.). The Development of Canadian Agriculture to 1970. Montreal: Macdonald College, McGill University, 1958. 9". Pp. 48.

[This contains projections of demand (including export demand) and supply of agricultural products in Canada, in aggregate, by commodity and by region. In estimating future productive potential, the authors stress particularly the effects on the supply of labour and other resources to agriculture of expansion in income and earnings in the non-agricultural sector of the Canadian economy.]

TRIMBLE (W.). Understanding the Canadian Economy. Toronto: Copp Clark, 1957. 8". Pp. vii + 207. \$3.00.

[This is a short introductory text-book designed for the use of first-year students in Technical Institutes in Canada. The subject-matter is largely theoretical, though the case of the Canadian economy is the one always in mind. The author's clear and lively style and his eye for what is most important at this level of exposition makes the book in many ways an excellent general introduction to economics. Unfortunately the standard of accuracy leaves something to be desired at a number of points; for example, grave confusion is introduced on pp. 75-7 by treating the forces affecting V in the Quantity equation in terms of the propensity to save.]

Indian

BASU (S. K.). The Managing Agency System, in prospect and retrospect. Calcutta: The World Press, 1958. 8½". Pp. vi + 222. Rs. 16.00.

[The prospect for the Managing Agency System in India is a gloomy one. Indeed, many people now regard it as an unfortunate institutional relic of British rule, although it has become largely Indianised. The 1956 Indian Companies Act has seriously curbed the powers of the Managing Agencies, and increasingly state intervention in industry may lead to their demise. Dr. Basu is as critical as most writers, and he provides here a useful description of the system's merits and defects, with chapters on earnings, finance, the 1956 Act and integration. He has little to say on the origins of the system, and a definitive study of its history remains to be done.]

BASU (S. K.). A Survey of Contemporary Banking Trends. Calcutta: The Book Exchange (217 Cornwallis Street), 1957. 9". Pp. 574. Rs. 16.00.

[This book is the author's *Recent Banking Developments* (1951) reorganised and brought up to date. The canvas is too wide to do some of the individual subjects justice. It describes not only central and commercial banking developments in India, but attempts a comparative study with Britain and other countries. There are chapters, also, on the International Monetary Fund and the International Bank.]

BHOGENDRANATH (N. C.). Development of the Textile Industry in Madras (up to 1950). Madras: University of Madras, 1957. 10". Pp. 264.

[There are three parts in this work; first, a historical survey, mainly from 1900, second an analytical study of profits, stock-exchange prices, size of mills, ownership and control, and third a review of raw material and labour conditions. There is much interesting information in this book, but it lacks conciseness and is very badly organised, as the author more or less admits in his introduction. He advocates decentralisation and the revival of the village industry, but provides little economic justification for his proposals.]

CHAND (GYAN). The New Economy of China. Bombay: Vora, 1958. 8½". Pp. xiv + 429. Rs. 16.00.

[Professor Chand has boldly attempted in this long work to give a picture of recent economic conditions in China. The descriptive information is interesting enough, and there is no doubt that India can learn something from Chinese efforts to tackle similar problems. A quantitative analysis of Chinese achievements is much more difficult. The author uses a great deal of official statistics, but admits that he has had "no opportunity of examining and assessing them with reference to their sources, methods and technique of collection." In view of this, how can one agree with his statement that "the limitations of the available statistics do not, to any significant extent, vitiate the conclusions based on them"? It is unfortunate that comparisons between Indian and Chinese conditions and policies in recent years have been explicitly omitted, although lack of comparative statistics would again hinder study here. This book lacks the rigorous research and penetrating analysis which would make it a major contribution to our knowledge of China.]

DESAI (M. B.). Report on an Enquiry into the Working of the Bombay Tenancy and Agricultural Lands Act, 1948 (as amended up to 1953) in Gujarat (excluding Baroda District). Bombay: Indian Society of Agricultural Economics, 1958. 9½". Pp. viii + 96. Rs. 4.00.

[This report provides statistical information on the extent and types of tenancy, the caste composition, literacy and indebtedness of tenants, patterns of cultivation and agricultural improvements. The failure of the Act to improve conditions substantially is attributed mainly to "the inferior social status and the low standards of literacy of the tenants."]

DESAI (N. B.). Report on the Administrative Survey of the Surat District. Bombay: Indian Society of Agricultural Economics, 1958. 9½". Pp. xxv + 336. Rs. 12.00.

[The Indian Planning Commission has stressed the need for the reorganisation of both Government and District Administration if the objectives of the Plans are to be fully implemented. This survey examines the evolution and present set-up of the Administration in one district of Bombay State. There is an historical survey, a detailed analysis of the methods of revenue collection and a description of the organisation and working of various departmental agencies dealing with agriculture, co-operation, health, irrigation and so on. The author makes many recommendations, including more decentralisation, the strengthening of village administration and, above all, greater liaison between the villager and Government officials.]

GHOSH (A.). Indian Economy, its Nature and Problems. A New Look Indian Economics. Calcutta: World Press, 1958. 8½". Pp. xx + 464. Rs. 10.00 or 21s.

[This is a revised and enlarged edition of the 1957 book which gives a panoramic view of the Indian economy, past and present. This edition is divided into two books. The first is on the *Nature of the Indian Economy* and deals mainly with problems of underdevelopment applied to India and the first and second five year plans. The second book is entitled *Problems of the Indian Economy*, and contains chapters on national income, agriculture, population, industry, trade unions, finance and banking. There is a great deal of factual information and statistics, and it will be a useful work of reference. The subjects covered are too wide to allow much depth in the analysis, but the author has made a brave attempt to get away from the hackneyed descriptions of the Indian economic scene and to approach it from the angle of growth and development. There is a bibliography but no index.]

GHOSH (O. K.). Problems of Economic Planning in India. Allahabad: Kitabistan, 1957. 8½". Pp. viii + 159. Rs. 5.50.

[There is a surfeit of books on Indian planning. This one has the merit of being written by an ex-member of the Planning Commission. There is more realism than one often finds in similar works, but the whole is discussed at too superficial a level to be of much value to the serious student.]

GORWALA (A. D.). Not in our Stars. Through the eyes of an Indian critic. Bombay: Jaico Publishing House, 1958. 6½". Pp. 303. Rs. 3.00.

[This provocative little book is a mixed journalistic bag of articles which have mainly appeared in Indian newspapers on subjects which include Congress funds, the Communists, the cattle problem, caste, language, Russia and the new China.]

INDIAN CHAMBER OF COMMERCE. A Handbook on the Companies Act, 1956. Calcutta: Indian Chamber of Commerce, 1956. 10". Pp. v + 330.

[The Indian Chamber of Commerce has recently published a number of studies intended to help business-men to interpret and understand the complex language of Indian tax and company legislation. In this book the main provisions of the 1956 Act are indicated, and advice is given on the correct procedure for, *inter alia*, incorporating companies, issuing shares, entering contracts, issuing debentures and dealing with managing agents.]

LAIDLAW (A. F.). Extension Work in the Co-operative Movement. Bombay: Central Committee for Co-operative Training, Reserve Bank of India, 1958. 8½". Pp. 46.

[Co-operative extension work has a vital role to play in Indian economics, especially in agricultural development. This pamphlet is a guide for extension workers, giving advice on their conduct, training, type of work and aims.]

LAKDAWALA (D. T.). Taxation and the Plan. Bombay: Popular Book Depot, 1956. 8½". Pp. 211. Rs. 5.0.

[The financial problems which have arisen during the currency of the second plan are now well known. This book was written before some of them arose, and is in parts therefore somewhat dated. It contains a detailed review of the Taxation Enquiry Commission's Report (1954) and a very able analysis of the relative merits of different forms of taxation in Indian conditions. For example, the need for a substantial increase in land revenue is pleaded. There is a critical note on Mr. Kaldor's report.]

LAUD (G. M.). Co-operative Banking in India. Bombay: The Co-operators' Book Depot, 1956. 8½". Pp. xii + 718. Rs. 27.8.

[Mr. Laud describes and analyses the history of co-operative banking in India, its effect on the money-lending system, the structure of rural credit, the organisation of village banks, people's banks, industrial banks and mortgage banks. In the last fifty years the movement has made considerable progress but the majority of the peasants are still indebted to the money-lender for their credit. This book would be much improved by drastic pruning.]

PAREKH (H. T.). The Future of Joint-stock Enterprise in India. Bombay: Jaico Publishing House, 1958. 9". Pp. x + 192. Rs. 17.50.

[This is a broad survey of industrial organisation and finance since the war, with chapters, *inter alia*, on self-financing, taxation, tariff policy, finance institutions, management and managing agencies, foreign capital and state enterprises. There is a useful statistical appendix on joint-stock company finance.]

RAO (A. V. RAMAN). The Economic Development of Andhra Pradesh 1766-1957. Bombay: Popular Book Depot, 1958. 8½". Pp. x + 384. Rs. 18.75.

[This is a detailed chronological account of the economic history of the region, admirably documented from original sources. There is a wealth of interesting information on agriculture, handicrafts, trade and finance. Unfortunately the chronological treatment and lack of general conclusions make it difficult for the reader to discuss long-term trends. There is no index.]

RESERVE BANK OF INDIA. Review of the Co-operative Movement in India, 1954-56. Bombay: Reserve Bank of India, 1958. 9½". Pp. v + 240. Rs. 3.00.

[This is the seventh review of this subject. It contains a full statistical analysis of all features of co-operation with comments on the achievements and problems of the movement.]

RESERVE BANK OF INDIA. Trends and Progress of Banking in India during the Year 1957. Bombay: Reserve Bank of India, 1958. 9½". Pp. vi + 122. Rs. 4.0 or 7s. 6d.

[This is a statistical survey of Indian banking during the year, with introductory comments on the general state of finance, banking legislation and organisation and the relation of the banking system to the plan.]

SAMAJ (B. S.). Slums of Old Delhi, 1958. Delhi: Atma Ram, 1958. 8½". Pp. 240. Rs. 7.50 or 15s.

[Indian cities are notorious for their slums but descriptions of these have usually been left to sensational travel writers. Here we have a serious study, the results of the "Socio-Economic Survey of the Slum Dwellers of Old Delhi City." Statistical and qualitative evidence is presented on population, congestion, housing, sanitation, health, incomes and employment. The result makes depressing reading and emphasises the enormity of the problems facing Indian town planners.]

SHAH (C. H.). Problems of Small Farmers. A report on an enquiry into the problems of low-income farmers in Kodinar Taluka. Bombay: Indian Society of Agricultural Economics, 1958. 9½". Pp. ix + 166.

[This is one of the many excellent regional studies sponsored by the Society. Among the topics discussed and illustrated statistically are underemployment, land utilisation, crop production and farm practices. Smallness of holdings and scarcity of capital for the purchase of seeds, manures and equipment mainly account for the lack of improvements and low yields on the small farms.]

SHENOY (B. R.). Problems of Indian Economic Development. Madras: University of Madras, 1958. 9½". Pp. 236. Rs. 10.00.

[Here are seven lectures delivered at the University of Madras on finance in the plans, the stabilisation of agricultural prices and the gold problem. The author is highly critical on the scale of deficit finance in the second plan, and maintains that the plans targets were unrealistically high. Events since then have partly justified his pessimism, although foreign aid has come to the rescue. He is no more optimistic about price-stabilisation schemes in farming, and also makes a brief plea for the devaluation of the rupee.]

WATTAL (P. K.). Population Problem in India. A census study. New Delhi: Minerva Book Shop, 1958. 8½". Pp. iv + 228. Rs. 10.00.

[Any book on Indian population is bound to be compared with the excellent study by Kingsley Davis, sponsored by the Princeton Office of Population Research. Mr. Wattal has produced a readable little book, based almost entirely on Census data, but, in comparison with the other, it suffers severely. It has neither the scope, the original research nor the presentation of the American work.]

Pakistani

HUSAIN (A. F. A.). Human and Social Impact of Technological Change in Pakistan. Report of a survey conducted by the University of Dacca and published with the assistance of UNESCO. In two volumes. Vol. I: Pp. xix + 404, 12s. 6d.; Vol. II: Reports of Case Studies: Pp. viii + 345, 7s. 6d. Dacca: Oxford University Press, 1956 (London: Oxford University Press, 1957). 8".

[This is the report on a survey conducted by the University of Dacca, with the assistance of UNESCO, into problems arising from the increasing industrialisation of East Pakistan. The sources are official statistics and the results of a large number of case studies which form the second volume. There is interesting information on attitudes to work, housing, class, health and similar socio-economic facts. A reading of the case studies provides a valuable insight into the way of life of the villagers and factory workers.]

American

ALLEN (R. L.). Middle Eastern Economic Relations with the Soviet Union, Eastern Europe, and Mainland China. Charlottesville, Virginia: Woodrow Wilson Department of Foreign Affairs, University of Virginia, 1958. 9". Pp. 128. \$1.00.

[The author describes recent relations between the countries of the Middle East and the Soviet bloc by way of trade, bilateral commercial agreements, credit and technical assistance, and arms transactions. He concludes by assessing the aims of the Communist leaders in these dealings and the consequences for western political and economic policies in the region.]

An Appraisal of the 1950 Census Income Data. *Studies in Income and Wealth*, Vol. XXIII, by the Conference on Research in Income and Wealth. Princeton, New Jersey: Princeton University Press for the National Bureau of Economic Research (London: Oxford University Press), 1958. 9". Pp. x + 450. 80s.

[The contributions to this conference fall into two main groups. The first group consists of methodological papers containing analyses of the relations and consistency between Census income data and other sources of information on incomes. The second group deals with substantive findings from the Census data. Certain statistics on income size-distribution from the Census are included that are not elsewhere available.]

BARUCH (B.). *My Own Story*. New York: Henry Holt, 1957 (London: Odhams Press, 1958). 9". Pp. 288. 21s.

[This first part of the entertaining reminiscences of the American financier and statesman is concerned mainly with his business experiences before the First World War. It contains also his reflections on certain social and political problems of the present day.]

BAUM (W. C.). *The French Economy and the State*. A RAND Corporation Research Study. Princeton, New Jersey: Princeton University Press (London: Oxford University Press), 1958. 9½". Pp. xvi + 391. 60s.

[To be reviewed.]

BERNSTEIN (M. H.). *The Job of the Federal Executive*. Washington, D.C.: The Brookings Institution, 1958. 8½". Pp. x + 241. \$3.50.

[This study of an aspect of public administration in the United States is based on a series of conferences in which a group of present and former federal officials took part. The topics discussed include the nature and environment of the work of those engaged in the federal service, the relations between political and career executives and problems of recruitment.]

BIRR (K.). *Pioneering in Industrial Research*. The story of the General Electric Research Laboratory. Washington, D.C.: Public Affairs Press, 1958. 9". Pp. 204. \$4.50.

[To be reviewed.]

BOURNEUF (ALICE). *Norway: The Planned Revival*. Harvard Economic Studies, Vol. CVI. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. 8½". Pp. xiv + 233. 40s.

[To be reviewed.]

BUCHANAN (J. M.). *Public Principles of Public Debt*. A defense and restatement. Homewood, Illinois: Irwin, 1958. 9". Pp. x + 223. \$5.00.

[To be reviewed.]

BUSHAW (D. W.) and CLOWER (R. W.). *Introduction to Mathematical Economics*. Homewood, Illinois: Richard D. Irwin, 1957. 9½". Pp. ix + 345. \$7.00.

[This work is the result of collaboration between a mathematician and an economist. Part I on Economics makes use of Part II on Mathematics. The latter, while covering a useful range of topics, is presented very formally and will certainly prove hard going for anyone doing more than refreshing his existing knowledge. Part I is written in a stimulating and illuminating style. Considerable emphasis is placed on the necessity of considering both stocks and flows in market analysis. The matters discussed are somewhat limited in scope, being confined to the older and more conventional departments of mathematical economics, e.g., stability and dynamics of markets, consumers' behaviour and the theory of the firm. The book should prove a useful guide to students half-way to Samuelson's *Foundations of Economic Analysis*; but surely activity analysis and its associated linear algebra ought now to be treated at an elementary level in any introductory course.]

CHANG (KIA-NGAU). *The Inflationary Spiral*. The experience in China, 1939-1950. New York: The Technology Press of the Massachusetts Institute of Technology and John Wiley (London: Chapman & Hall), 1958. 9½". Pp. xvii + 394. 80s.

[To be reviewed.]

COLM (G.) and GEIGER (T.) assisted by HELZNER (M.). *The Economy of the American People*. Progress, problems, prospects. Planning Pamphlet, No. 102. Washington, D.C.: National Planning Association, 1958. 9". Pp. 167. \$2.00.

[This provides a well-informed factual survey of the present state of the American economy together with a balanced discussion of some of the chief problems arising in it: fluctuations, distribution of income, anti-trust policy, international economic relations, etc. It is written from the liberal-minded business point of view characteristic of the National Planning Association.]

ECKSTEIN (O.). *Water Resource Development*. The economics of project evaluation. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. 1½". Pp. xiii + 300. 52s.

[To be reviewed.]

GRABILL (W. H.), KISER (C. V.) and WHELPTON (P. K.). *The Fertility of American Women*. In the Census Monograph Series. New York: John Wiley, for the Social Science Research Council and the Bureau of the Census of the U.S. Department of Commerce (London: Chapman & Hall), 1958. 9½". Pp. xvi + 448. 76s.

[This formidable collection of findings is based on tabulations of the Bureau of the Census, supplemented by certain material drawn from other sources. It is concerned with trends in reproduction since Colonial days and with differentials in fertility according to geographic and urban-rural residence, colour, age, education, occupation, income, etc. It will undoubtedly become a major source-book for students of fertility.]

HAMILTON (D. C.). *Competition in Oil*. The Gulf Coast refinery market, 1925-50. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. 1½". Pp. xiv + 233. 60s.

[The purpose of this monograph is to assess the effectiveness of competition in the refinery section of the oil industry on the Gulf Coast. There is a considerable degree of vertical integration between crude oil production, refining and distribution, but the non-integrated part of the industry is sufficiently large to create an independent market in which refiners may buy and sell. They are in fact in a relatively favourable position in this respect, since the capacity of the non-integrated refiners is much smaller than the capacity of the non-integrated producers of crude oil. The structure of the refinery market is moderately oligopolistic, and entry is difficult but by no means impossible. As far as structure is concerned, therefore, effective competition is feasible but not inevitable. Examination of the actual performance of the industry leads the author to the tentative conclusion that competition has in fact worked fairly well. There has not been any general tendency to excess capacity, and profits have been low and the processing margin (measured in constant dollars) has shown a slight secular decline.]

HICKMAN (W. B.). *Corporate Bond Quality and Investor Experience*. National Bureau of Economic Research, Studies in Corporate Bond Financing No. 2. Princeton, New Jersey: Princeton University Press for the National Bureau of Economic Research (London: Oxford University Press), 1958. 9½". Pp. xxix + 536.

[This is the second volume in the National Bureau's large-scale research project on corporate bonds. A massive collection of data is provided on life-span yields, defaults rates, promised yields and price behaviour of bonds, studied in relation to agency ratings, market ratings, date of issue, size of issue and other characteristics.]

HINSHAW (R.). *Toward European Convertibility*. Essays in International Finance, No. 31. Princeton, New Jersey: International Finance Section, Princeton University, 1958. 9". Pp. 33.

[This is a description and interpretation of the various moves towards and away from currency convertibility that have been made since the War. It was, of course, written before the recent establishment of non-resident convertibility. It includes an interesting discussion of the European Monetary Agreement.]

KENNEDY (W. F.). *Humanist Versus Economist*. The economic thought of Samuel Taylor Coleridge. University of California Publications in Economics, Vol. 17. Berkeley and Los Angeles, California: University of California Press, 1958. 10½". Pp. 96. \$2.00.

[In this essay Coleridge and Bentham are taken as the fountain-heads of two opposing streams of social thought. Coleridge's views on economic questions are first considered in relation to his philosophy as a whole. The conflict in the writings of later economists between the clear but narrow Benthamite philosophy and the humanistic ideas represented by Coleridge is then traced out, with special reference to Mill, Marshall and Keynes. The author maintains that the addiction

of later nineteenth-century economists to a narrow Benthamite view of man led them to over-rate the importance of economic needs and activities in life as a whole. Mill and Marshall both attempted to take a rather broader view, but neither, in the author's opinion, had at all a firm grasp of Coleridgean humanism, and neither succeeded in bridging the gap between the economists and the men of letters who became the main repositories of the humanistic approach to social problems. Of the three economists named above, Keynes, in the author's opinion, came nearest to freeing himself from the shackles of Benthamism.]

KRUTILLA (J. V.) and ECKSTEIN (O.). *Multiple Purpose River Development. Studies in Applied Economic Analysis.* Baltimore, Maryland: Johns Hopkins Press (London: Oxford University Press), 1958. 9". Pp. xiv + 301. 36s.

[To be reviewed.]

McKEAN (R. N.). *Efficiency in Government Through Systems Analysis, with Emphasis on Water Resources Development. A RAND Corporation Research Study.* New York: John Wiley (London: Chapman & Hall), 1958. 9½". Pp. x + 336. 64s.

[To be reviewed.]

MANSERGH (N.) and others. *Commonwealth Perspectives.* Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1958. 9½". Pp. viii + 214. 34s.

[To be reviewed.]

MIKESELL (R. F.) and BEHRMAN (J. N.). *Financing Free World Trade with the Sino-Soviet Bloc. Princeton Studies in International Finance, No. 8.* Princeton, New Jersey: International Finance Section, Princeton University, 1958. 9". Pp. viii + 109 + tables.

[This provides a useful analysis of the organisation of trade and payments in transactions with the Communist bloc. At present countries signatory to trade and payments agreements carry on about 70% of total East-West trade. The authors analyse the terms and working of these agreements and the extent of the moves that have been made towards more multilateral means of settlement. They also provide a useful discussion of the motives that have operated on both sides in negotiating bilateral agreements.]

MORLEY (F.) (Ed.). *Essays on Individuality.* Philadelphia, Pennsylvania: University of Pennsylvania Press (London: Oxford University Press), 1958. 8½". Pp. 270. \$5.00.

[This contains twelve papers originally prepared for delivery at a conference convened to analyse "the problem of man's freedom in the face of modern society's seemingly irresistible urge to socialize and regiment the thought and action of the individual." The contributors include Milton Friedman and Friedrich A. Hayek as well as a number of exponents of other social sciences and literary men.]

PHILLIPS (J. D.). *Little Business in the American Economy. Illinois Studies in the Social Sciences, Vol. 12.* Urbana, Illinois: University of Illinois Press, 1958. 10". Pp. 135. \$2.50.

[The word "little" in the title is used to distinguish the very small businesses which are the subject of this monograph from the larger concerns that are commonly ranked as small in studies of business size. The distinguishing feature of little businesses in the author's usage is the absence or near-absence of hired labour. Statistical data are provided on the place of such businesses in the American economy outside agriculture. Their total number is, of course, very large, but they are mainly concentrated in a few sectors, chiefly retailing and service trades. The author discusses the factors affecting the fortunes of these businesses and emphasises the largely nominal character of their independence. His general approach is similar in many respects to that expressed in J. Steindl's well-known book, *Small and Big Business*. He does not believe that any governmental measures can arrest the tendency for the place of little businesses in the economy to decline, save at prohibitive cost. At the same time he puts forward cogent reasons for rejecting the commonly expressed view that independent small proprietors form a socially indispensable ingredient of the middle class.]

ROSS (R.) and VAN DEN HAAG (E.). *The Fabric of Society. An introduction to the social sciences.* New York: Harcourt, Brace, 1958. 9½". Pp. xv + 777. \$10.00.

[To be reviewed.]

RUSSELL (R. B.). *A History of the United Nations Charter. The role of the United States, 1940-1945.* Washington, D.C.: The Brookings Institution, 1958. 9½". Pp. xviii + 1140. \$10.00.

[This is one of a series of studies on the United Nations undertaken by the Brookings Institution. It describes the evolution and negotiation of the Charter within the framework of United States foreign policy during the period from 1940 to 1945. It is largely based on State Department documents that have not been made public before.]

Selected Studies of Migration since World War II. Proceedings of the Thirty-fourth Annual Conference of the Milbank Memorial Fund. New York: Milbank Memorial Fund, 1958. 9". Pp. 244. \$1.00.

[This symposium deals with both international and internal migration. The contributors include D. Kirk, I. B. Taeuber, R. B. Vance, E. Rubin, C. H. Hamilton, D. J. Bogue, E. S. Lee, J. K. Folger, J. J. Spengler, S. Kuznets and D. S. Thomas.]

SNIDER (D. A.). *Introduction to International Economics.* Revised edition. Homewood, Illinois: Irwin, 1958. 9". Pp. xix + 598. \$7.00.

[The first edition of this workmanlike text-book was published in 1954. Compared with many other American text-books on international economics, a good deal of attention is devoted to theoretical issues, but the author's aim throughout is to tie this in with discussions of empirical issues.]

TAEUBER (IRENE B.). *The Population of Japan.* Princeton, New Jersey: Princeton University Press (London: Oxford University Press), 1958. 12½". Pp. xx + 461. 120s.

[The Princeton Office of Population Research is to be congratulated on sponsoring this addition to its excellent series of works on population in different regions. This is the first systematic treatment of Japanese population movements from early times until the present day. Dr. Taeuber has apparently been fortunate in the abundance of statistical sources available, and she has used these to produce a comprehensive and painstaking study. There are several maps, 141 tables and a full bibliography of original and secondary materials. The demographer will find a detailed statistical analysis of all aspects of population and, for the economist, there is useful information on occupational distribution, urbanisation, industrialisation and the effects of the recent war. One of the merits of the book is the way in which the interconnection between demographic and other developments is made clear.]

TEITELBAUM (P. D.). *Productive Uses of Nuclear Energy: Nuclear Energy and the U.S. Fuel Economy, 1955-1980.* Washington, D.C.: National Planning Association, 1958. 9". Pp. xi + 187. \$3.00.

[This contains projections of supply and demand for different types of energy in the United States. It is reckoned that roughly twice as much energy will be consumed in 1980 as in 1955, and that nuclear power will supply nearly 10% of it. This is believed to be a conservative estimate, since account cannot be taken of applications of nuclear power in new uses at present not foreseen. The author believes, however, that the cost of traditional sources of energy will not rise by much within the period under consideration, and the extension of the use of nuclear energy is thus likely to be dependent on a reduction in its cost.]

THOMASSEN (H.). *Business Planning for Economic Stability.* Washington, D.C.: Public Affairs Press, 1958. 9½". Pp. 60. \$2.50.

[The thesis of this paper is that it is feasible, given certain conditions, for economic fluctuations to be avoided without government intervention, so long as businesses pursue a stability-orientated type of policy. One of the chief conditions is that there should be a fairly high degree of concentration within the economy. The author frequently speaks of "rational" business policy as that conducive to overall economic stability. While it may be true that in most cases a firm's own interests will also best be served by a policy that contributes to stability in the economy as a whole, there must in a diverse economy be quite frequent cases where this is not so, and the author does not altogether clearly indicate how in such cases the divergence between private and social interest is to be overcome without government intervention.]

ZUCKER (E.). *"Standard Gold" and Silver. The way out of a crisis.* New York: Kraus Reprint Corporation (16 East 46th Street, New York 17), 1958. 9". Pp. 88. \$2.50.

[This is a reprint of a booklet published in 1932. In a new introduction, the author suggests that despite the great changes in economic conditions since that time, some at least of the ideas he set forth in 1932 remain relevant. The gist of the author's proposals were that the purchasing

power of the world gold stock should be increased by reducing by one-third the normal requirement of fineness and that at the same time silver should be re-established as an additional monetary metal.]

German

STISSER (R.). *Problems of India's Economic Development*. Kiel: Institut für Weltwirtschaft, 1958. 9½". Pp. 83. DM. 8.

[This is based on a more elaborate work in German by the same author to be published shortly. It provides a brief and largely factual survey of India's current problems and of the efforts being made to cope with them.]

Dutch

Some Quantitative Aspects of Stock Trading on the Leading U.S. Stock Exchanges. Twenty-five years of development, 1930-1954. By the Foundation for Economic Research of the University of Amsterdam. S.E.O. Publication No. 11. Leiden: Stenfert Kroese, 1958. 10". Pp. viii + 124 + appendices. Fl. 19.

[This is a descriptive account of the relative importance of different ways of dealing in stocks practised in the United States and of the different types of stocks traded in.]

THEIL (H.). *Economic Forecasts and Policy*. Contributions to Economic Analysis, No. XV. Amsterdam: North-Holland Publishing Company, 1958. 9". Pp. xxxi + 562. 100s.

[To be reviewed.]

Swedish

GARDLUND (T.). *The Life of Knut Wicksell*. Translated from the Swedish by NANCY ADLER. Stockholm: Almqvist & Wiksell, 1958. 8½". Pp. 355. Sw. kr. 30.

[To be reviewed.]

Mexican

RICARDO (D.). *Notas a los Principios de Economía Política de Malthus*. Translated by F. M. TORNER. Mexico City: Fondo de Cultura Económica (Av. de la Universidad 975, Mexico 12, D.F.), 1958. 9½". Pp. xiv + 331.

[This is the second volume of the Spanish translation of the Royal Economic Society's edition of Ricardo, edited by Mr. Sraffa.]

Official

BRITISH

HORNBY (W.). *Factories and Plant. History of the Second World War*, United Kingdom Civil Series, edited by SIR KEITH HANCOCK. London: H.M. Stationery Office and Longmans, 1958. 10". Pp. xiii + 421. 37s. 6d.

[To be reviewed.]

INTERDEPARTMENTAL COMMITTEE ON SOCIAL AND ECONOMIC RESEARCH. *Guides to Official Sources*. No. 1: Labour Statistics. Material collected by the Ministry of Labour and National Service. Revised edition. London: H.M. Stationery Office, 1958. 9½". Pp. 78. 5s.

[The third edition of this booklet has been enlarged and rewritten. As well as being brought up to date, the new edition incorporates information on such data as are available for the period before 1917.]

MINISTRY OF AGRICULTURE, FISHERIES AND FOOD. *Domestic Food Consumption and Expenditure, 1956. Annual Report of the National Food Survey Committee*. London: H.M. Stationery Office, 1958. 9½". Pp. 172. 9s. 6d.

[This seventh report in its series follows the same general lines as last year's issue, with certain minor reclassifications and amplifications. A new chapter deals with differences in dietary patterns that are associated with occupational status.]

ROYAL COMMISSION ON COMMON LAND 1955-58. *Report*. London: H.M. Stationery Office, 1958. 9½". Pp. viii + 284. 14s.

[The Commission was appointed to consider possible changes in the law. Its unanimous report provides an extensive list of recommendations relating to registration, public access, and management and improvement of common land. These recommendations are preceded by a description of the present state of affairs. Several papers by members of the Commission are given as appendices, including a paper on the "History of Common Land and Common Rights," by Dr. W. G. Hoskins, and a paper on "The Geographical Distribution of Common Land," by Professor L. Dudley Stamp.]

CANADIAN

RESTRICTIVE TRADE PRACTICES COMMISSION, DEPARTMENT OF JUSTICE, OTTAWA. *Material Collected for Submission to the Commission in the Course of an Inquiry into certain Discriminatory Pricing Practices in the Grocery Trade*. Ottawa: Department of Justice, 1958. 10". Pp. xiii + 220.

[Unlike most of the reports issued by the Commission, this is not a report into a specific alleged malpractice but a report of a more general character on a group of practices. It contains a chapter of general economic analysis of price discrimination. This is followed by a description of the special discounts and allowances operating in the Canadian grocery trade and their relation to the working of competition in that trade.]

INDIAN

DIRECTORATE OF ECONOMICS AND STATISTICS, MINISTRY OF FOOD AND AGRICULTURE. *Agricultural Legislation in India. Vol. II: Consolidation of Holdings*. Delhi: Government of India Press, 1957. 9½". Pp. xxvi + 162.

[The small size and bad lay-out of many Indian farms impedes agricultural improvement. The introduction to this work provides some statistical evidence of this problem and reviews recent measures taken to rectify it. The main part of the book reproduces the various Acts passed by state governments on the consolidation of holdings.]

AMERICAN

High Rent Housing and Rent Control in New York City. New York: State of New York Temporary State Housing Rent Commission, 1958. 9". Pp. 173.

[This is a study of the working of rent control in the case of apartments in Manhattan with a legal maximum monthly rent of \$200 or more. Extensive statistics, based on a sample survey, are given of the economic and rental history of apartments of this class, of new private construction in Manhattan, of occupants' characteristics and housing experience, of space utilisation, and of relations with the uncontrolled sector. Rents of these apartments have risen more than the average of all rents in New York City but less than the price-level as a whole. The conclusion drawn by the authors from their extensive empirical investigation is that rent control has in this instance worked as "a social instrument permitting orderly change" and has not, as far as can be seen, had the harmful effects commonly alleged.]

NERLOVE (M.). *Distributed Lags and Demand Analysis for Agricultural and Other Commodities*. Agriculture Handbook No. 141 of the Agricultural Marketing Service of the U.S. Department of Agriculture. Washington, D.C.: U.S. Government Printing Office, 1958. 10". Pp. 121. 60c.

[This is a methodological study. The types of cause that may lead to distributed lags in demand relationships are enumerated, including psychological factors, technological factors, institutional factors, uncertainty and, in the case of consumer durables, the operation of the acceleration principle. The author considers the techniques appropriate for handling the equations that result in each of these cases and in particular how best to avoid having to work with equations involving an unmanageable number of variables. The author's most original contributions are in connection with lags due to uncertainty. The monograph concludes with a discussion of the Friedman "permanent income" hypothesis and its application to the demand for individual commodities.]

IRAQ

FENELON (K. G.). Iraq National Income and Expenditure, 1950-56. Baghdad: Ministry of Economics, 1958. 9½". Pp. 28. 100 Fils.

[This is the first study of the national income of Iraq to have been published. Figures are given of consumption, investment and other classes of expenditure generating national income in each year from 1950 to 1956. Estimates are also given of national income at constant prices and of expenditure on different items of consumption. The real national income is found to have risen by nearly 14% per annum over the period.]

INTERNATIONAL

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT. Thirteenth Annual Report, 1957-1958. Washington, D.C.: International Bank for Reconstruction and Development, 1958. 11". Pp. 68.

[The year 1958 was marked by a considerable expansion in the Bank's activities, its new loans amounting to over 75% more than the average of the three previous years. This report gives details of the loans granted and their economic background. It also describes progress of development projects assisted by Bank loans made in earlier years.]

INTERNATIONAL MONETARY FUND. International Reserves and Liquidity. A study by the staff of the Fund. Washington, D.C.: International Monetary Fund, 1958. 11". Pp. 104.

[To be reviewed.]

EUROPEAN PRODUCTIVITY AGENCY, ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. Freedom of Entry into Industry and Trade, by P. MUNTHE. Paris: O.E.E.C., 1958. 10½". Pp. 76. 7s.

[The author analyses the various factors affecting freedom of entry: technology, market structure, government regulation and restrictive business practices. He then proceeds to consider the effects on productivity and the implications for government policy. The author's aim is to provide a survey of the current state of thinking on this topic rather than to put forward any new views of his own, but his general sympathies are with the view that active competition by means of new entry has an important effect in promoting efficiency.]

EUROPEAN PRODUCTIVITY AGENCY, ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. The Influence of Sales Taxes on Productivity. Project No. 315. Paris: O.E.E.C., 1958. 9½". Pp. 268. 18s.

[The author of this report is Mr. Charles Campet. Part I of the report contains a theoretical analysis of the possible effects of different types of sales tax on the allocation of resources, the choice between methods of production, the degree of vertical integration, etc. Part II describes and comments on sales taxes currently in operation in nine European countries.]

EUROPEAN PRODUCTIVITY AGENCY, ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. Resale Price Maintenance, by S. GAMMELGAARD. Paris: O.E.E.C., 1958. 9½". Pp. 114. 8s.

[This provides a lucid and convenient digest of recent literature on the subject. It is dealt with under the following headings: general description of resale price maintenance; legislation; why in theory; and economic effects in practice. It is useful to have the not very abundant empirical information discussed as well as the more familiar theoretical considerations. The author's belief is that resale price maintenance has an unfavourable effect on productivity in retailing and no clear favourable effects to offset this in manufacturing.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. The Elimination of Double Taxation. Report of the Fiscal Committee of the O.E.E.C. Paris: O.E.E.C., 1958. 9½". Pp. 63. 5s.

[The Committee puts forward recommendations on four questions that arise in double taxation Conventions between countries. These are concerned with the listing and definition of the taxes which should be covered by such Conventions; with the concept of a permanent establishment; with the concept of fiscal domicile; and with tax discrimination on grounds of nationality.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. Terms Used in International Trade and Payments and National Accounts. Glossary and definitions English-French and French-English. Paris: O.E.E.C., 1958. 10½". Pp. 146. 12s. 6d.

[This two-way glossary, designed to supplement existing reference books, was prepared mainly for the use of translators within the O.E.E.C. The translations provided are accompanied in many cases with explanatory notes giving full background information.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. Eighth Annual Report of the Managing Board of the EUROPEAN PAYMENTS UNION for the Financial Year 1957-58. Paris: O.E.E.C., 1958. 9½". Pp. 59 + tables and graphs. 8s.

[This report covers the year ending June 30, 1958. The operations of the Union and the activities of its Managing Board are described against the background of the general economic situation in Europe. Full particulars are given of the financial assistance granted to France, which was the main feature of the year's operations.]

UNITED NATIONS. Management of Industrial Enterprises in Under-developed Countries. New York: United Nations, 1958. 9". Pp. v + 35. 2s.

[This report is based on the discussions of a panel of experts with practical experience of management problems in underdeveloped countries. It deals both with the recruitment and training of managers and with the actual problems of management in the spheres of labour, production, marketing and financial control, and relations with government.]

UNITED NATIONS. Reclassification of Government Expenditures and Receipts in Selected Countries. New York: United Nations, 1958. 10½". Pp. ix + 124. 11s.

[This paper is concerned with certain problems in the classification and presentation of statistics relating to the economic activities of government, looked at mainly from the point of view of how best to facilitate economic analysis, but with some regard also to practical questions of administration and accountability. The topics discussed are: accounting systems used in studies of the public sector; links between conventional budgets and economic reclassification schemes; use and development of cross-classification; and treatment of transactions of public enterprises. Extensive illustrative tables are included taken from the accounts of individual countries.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. The State of Food and Agriculture 1958. Rome: F.A.O., 1958. 11". Pp. x + 222. 12s. 6d.

[In addition to a general survey of the year, this report contains two special chapters, one on food and agricultural developments in Africa south of the Sahara, and the other on the growth of forest industries. In the survey chapter, the report notes that in 1957/58 there was for the first time since the War a check to the upward trend in world agricultural output. The immediate cause of this lay largely in unfavourable weather, but longer-term economic factors were also partly responsible. An increasing contrast is shown to exist between the agricultural situation in advanced countries, where output could comparatively easily be raised above existing levels but demand is deficient, and backward countries, where the reverse is the case. In the opinion of the authors of the report the pricing policies followed in the two types of country have been such as to aggravate this disharmony.]



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EQUILIBRIUM, EXPECTATIONS AND INFORMATION

THE central thesis which this paper will seek to justify can be quite briefly asserted. We shall argue that the familiar "general equilibrium of production and exchange" cannot be properly regarded as a configuration towards which a hypothetical perfectly competitive economy would gravitate or at which it would remain at rest. We attempt to support this assertion by showing that the activity required of entrepreneurs would require a certain minimum information; that the availability of such information is a function of the nature of the economic arrangements or system postulated; and that the conditions necessary for adequate information are incompatible with perfect competition.

These conclusions, even if accepted, may be regarded by many as of little importance, on the grounds that the perfect competition model is known to correspond very imperfectly to reality. Nevertheless, its use as an analytical instrument is very widespread, both in positive analysis and by virtue of its normative significance as a Pareto Optimum.¹ While the disturbing effects of indivisibilities and external economies may be freely admitted by all writers, it is commonly assumed that, in the absence of these factors, there would be a tendency to move towards, or at any rate to remain in, general equilibrium. In denying this belief, we shall concentrate on the informational aspect of economic systems, the importance of which in the analysis of group equilibrium is not confined to perfect competition.

The function of the equilibrium concept in economics is to enable us to say what particular activities will be undertaken in response to certain postulated conditions. The association between these may take a strong or a weak form; we may assert either that the activities would ultimately be undertaken or merely that, if undertaken, they would be persisted in, provided, of course, that the specified conditions do not change. Our ability to forge these connections derives from the use we make of the idea of compatibility; we argue that no activities will be undertaken or maintained unless they are compatible, first, with the particular constraints imposed

¹ Vide G. Stigler, "Perfect Competition, Historically Contemplated," *Journal of Political Economy*, February 1957. "Today the concept of perfect competition is being used more widely by the profession in its theoretical work than at any time in the past. The vitality of the concept is strongly spoken for by this triumph."



by the assumed environment, and, secondly, with the objectives of the agents.

It is obvious that no direct connection can exist between objective conditions and purposive activity; the immediate relationship is between *beliefs* about relevant conditions and *planned* activities which it may or may not prove possible to implement. Only those activities will be planned, we assume, which are compatible both with the agent's objectives and with the opportunities he believes to exist. In certain situations only one projected course of action may meet these conditions. The simplest way to proceed from here to the establishment of relationships between objective conditions (as opposed to beliefs) and actions (as opposed to plans) is by assuming that the agent has full information. We, the model builders, postulate an agent with certain objectives and opportunities from which we can deduce the course of action which would be best for him; by then assuming that he is fully acquainted with the "determinants" of his choice, and that he will pursue his objectives, we convert our system into a model of behaviour in response to given conditions. The ordinary account of consumers' behaviour is clearly of this kind; we can recognise that our model is no more than an incarnation of the logic of choice. What the agent needs to know are the conditions of the allocation problem as laid down by the model builder. What we may call the "informational requirements" of the model are simply that the agent have knowledge of the determinants of the equilibrium (*e.g.*, preferences, prices, income, etc.); if this is so the equilibrium activities will be immediately undertaken and persisted in so long as the determining conditions remain unchanged. For all simple models constructed in this way the informational requirements are the same; they cease to hold good, however, when we pass from the equilibrium of an individual to that of an interrelated group.¹

It seems clear that a sufficient condition for a group, or for a system as a whole, to be said to be in equilibrium, is for all the component units individually to be in equilibrium. The condition, however, should not be regarded as necessary, for stability in the aggregates (*e.g.*, levels of output) by which we describe the system might be maintained without stability in the contributions of individual units. Nevertheless, we shall concentrate our attention on the more obvious possibility, and assume that group

¹ All models of individual equilibrium are not of this simple kind. Though full information is a sufficient condition for attaining the equilibrium, it is not necessary. All that is required is, first, that the man should believe a particular action to be optimal and, secondly, that the process of carrying it out should provide no information which is inconsistent with, or would cause him to doubt, his belief. By and large, however, economists have concentrated on the case where equilibrium is secured because beliefs are actually correct, though attention has also been given to hypothetical constructions in which the execution of a plan formed on the basis of inadequate information brings new evidence to light, so that full information, and therefore equilibrium also, are obtained gradually by a process of trial and error. What concerns us is to note that in no case can we identify equilibrium activities without reference both to objective conditions and to states of belief.

equilibrium is reached when individual equilibrium is universal. This implies, as we have seen, first, that everyone believes his projected activities to be optimal, and, secondly, that their implementation goes according to plan, so that there is no reason for changing them. In the case of the isolated individual we could regard this latter condition as met, provided his estimates of the given external conditions were not contradicted by the evidence brought to light in the execution of his plan. These external conditions were viewed impersonally as fixed opportunities and restrictions, and no attention was paid to the activity of others which lay behind them. Now that the activities of related individuals are being considered explicitly, however, it is important to draw a distinction between two different kinds of conditions which any agent has to attempt to estimate. By "primary conditions" we shall mean the technical possibilities of production and the existing state of consumer preference; "secondary conditions" will refer to the relevant projected activities of other people in the system, of customers, competitors and suppliers. It will be convenient to refer to information, or to uncertainty, as primary or secondary according to whichever of these it is related. This terminology is meant to convey no difference in the importance of these two kinds of conditions; a distinction is made between them because of the different ways in which information about them has to be obtained.

Now if a plan is to be carried out as intended, the estimates on which it is based must not be contradicted by events; this will be ensured in the most obvious way if the estimates are correct. The agent must therefore have adequate primary information about his own preferences and the more or less technical relationships to which his plans cannot run counter, as well as adequate secondary information about what other people, whose actions concern him, may or may not do. From the point of view of an observer external to the system (as opposed to an agent within it) this requirement can be put in slightly different terms by saying that the activities of all agents must be compatible both with primary conditions and with each other.

With these considerations in mind, let us now examine the celebrated attempt to identify a general equilibrium of production and exchange under perfect competition. Perfect competition is a hypothetical system characterised, as is well known, by the existence of many buyers and sellers of homogeneous commodities and by the absence of any artificial restraints on their activities—a condition sometimes referred to as perfect mobility. Most writers also recognise that these buyers and sellers must have certain necessary information, but this requirement is usually left obscure,¹ and its relationship with the other conditions of the model is not examined. Given perfectly competitive conditions, an equilibrium pattern of economic activities is identified and made to depend on external conditions represented by the preferences of individuals, the endowment and original distri-

¹ For a notable exception, see F. A. Hayek, "Economics and Knowledge," *Economica*, 1937.

bution of resources, and the state of technique. We shall now briefly state its properties in terms of a simple objective description of the activities pursued and their relationship to the "fundamental determinants" of the system; any reference these activities might have to the wishes or intentions of those engaged in them will be postponed.

The equilibrium configuration has two kinds of special properties, the first relating to the situation in which each individual unit finds itself, the second to relationships between the aggregate levels of the activities in which they are engaged. Every consumer has an expenditure equal to his income and a pattern of consumption such that his marginal rate of substitution between any two goods is equal to the ratio of their prices, which are the same for all. Each producer can be observed to be producing as efficiently as possible, given the state of technique, and the price paid to any of the factors of production is equal to the value of its marginal product in that use. These conditions suggest optimality; the others, consistency. The aggregate current production of each commodity is neither more nor less than is used either by other firms or in final consumption. (We avoid saying simply that supply and demand are equal, for this would involve a reference to plans and expectations which are not to be introduced at this stage.) All scarce resources are likewise fully employed, and the expenditure of any individual is equal to the income derived from the sale of the resources at his disposal, as determined by the distribution of skill and property originally assumed.¹

These, then, are the relationships which characterise the perfectly competitive economy in what is believed to be its equilibrium state. But, as has been observed, equilibrium is not secured merely by the existence of a particular set of economic activities in themselves, but by their co-existence with a particular set of beliefs. Therefore we have to discover what beliefs must be associated with the configuration we have described in order that it can be regarded as self-perpetuating. One of its properties consisted of the correspondence of marginal rates of substitution to relative prices, so that consumers were each objectively in an optimum position, provided their incomes and the prices of commodities were regarded as fixed and given. They would themselves believe their situation to be optimal, and therefore have no wish to change it, provided we endow them with certain information and beliefs. It would be sufficient to assume that they were fully acquainted with their own preferences and with the prices of all goods, and that they expected that neither these preferences and prices, nor their income, would change in the future. Producers will also wish to persist in the activities the configuration allotted to them, if they know all the production possibilities, if they know all product and factor prices and expect them to remain the

¹ This account of the conditions of equilibrium is of course loose and inadequate; but, for our particular purposes, there is no need for a full elaboration. We shall ignore questions as to the "existence" and "stability" of equilibria, at least in the sense which these terms are used in current theoretical discussions.

same, and finally, if there is some reason for preventing them expanding the current scale of their operations. (The significance of this third and final condition, for which the equilibrium of the consumer had no simple counterpart, will be developed in due course.) Similar conditions of belief will ensure that the suppliers of productive resources are also content with the rôle they have been allotted.¹

The second set of properties of the equilibrium configuration, those concerned with the consistency of the various component activities, now ensures that the plans of consumers and producers can be successfully carried out and the beliefs or expectations which formed their basis will be proved correct. We are now entitled to say that, in every particular market, demand equals supply, in the sense that the aggregate amount which buyers plan to buy equals the amount which sellers plan to sell. It seems therefore that the conditions of belief required for equilibrium have been established; they consist in the possession of primary information about preferences and production functions and in the belief as to the permanence of existing prices, though it is necessary to add, somewhat untidily, that each producer must feel unwilling or unable to raise the level of his output.

Before examining the value of this conclusion, let us consider whether the conditions of belief which will ensure the maintenance of equilibrium activities will also ensure their adoption. This question can be interpreted in either of two ways. One may ask whether the universal expectation of equilibrium prices could produce a direct movement to equilibrium. Alternatively, one may ask whether the universal belief that current prices, whatever they are, will be permanent, could, through some process of successive adjustments, produce a gradual, if indirect, movement to the equilibrium. Each of these possibilities must now be considered in turn.

Let us assume that the existing pattern of output, prices, etc., is not that which would exist in equilibrium but that everyone concerned happens to expect that the equilibrium prices will rule in the future. Will the system move to equilibrium? Here it is important to distinguish between two different assumptions which may be made about entrepreneurs. The normal assumption, or at any rate the assumption implicit in most treatments, is that entrepreneurial skill is undifferentiated, in the sense that no entrepreneur can have a comparative advantage over other entrepreneurs in any particular line. We shall adopt this assumption first and then take up the

¹ These conditions, though sufficient, are not necessary. Producers, for example, might be content to persist in their current activities if they expected their own output and input prices to remain the same, but were unable to form any estimate of the future level of any other prices in the system. There are in fact an indefinite number of expectational conditions which would, under reasonable assumptions as to human motivation, secure the maintenance of the general equilibrium. And though some of them no doubt appear less unrealistic than others, there seems no clear reason why any one set is most appropriate. Our concern will be with the way in which expectational conditions are commonly introduced into equilibrium analysis, rather than with the merits of some conditions in comparison to others.

alternative one, according to which no entrepreneurs are alike, there being between any two of them differences of comparative advantage in different lines of production.

Under the first of these assumptions it is clear that, given the expectations postulated, the actions of any entrepreneur are wholly indeterminate. The product and factor prices he expects are such (*ex hypothesi*) that afford normal profits in all lines, and there is no reason why he should choose one rather than the other. There is therefore no reason why the equilibrium configuration should come to be established. The situation is altered, however, once we assume the presence of differences in comparative advantage between entrepreneurs. If this is the case the equilibrium configuration will no longer leave the allocation of output between firms undetermined; for each entrepreneur there will be one particular niche in which his income will be a maximum. Immediately equilibrium prices come to be expected, therefore, all entrepreneurs will move directly to their appointed stations, equilibrium will be established and expectations fulfilled.

We ought now to deal with the second version of our question as to the beliefs required to ensure the adoption of equilibrium activities. Will the expectation that prices will maintain their current values suffice for our purposes? Now clearly if current prices have any values apart from those appropriate to the equilibrium configuration, any universally held expectations as to their continuance will be immediately disappointed and plans based on them will prove impossible to fulfil. Equilibrium in this model could therefore be shown ultimately to come about only as a result of a process in which expectations were continually revised; the execution of plans would have to produce a set of prices which were again projected into the future and used as a basis for fresh plans and so on. In this way a model can presumably be constructed which would exhibit movement to equilibrium in a group of related markets, but an examination of the work done in this direction would lead us too far afield. There exists in any case a fundamental objection, which we must now consider, to this whole approach.¹

In order to establish the general equilibrium it was recognised that we had to postulate certain beliefs; the universal expectation that prices would stay the same was an example of what was required. No attempt was made, however, to explain or account for these conditions, which were introduced

¹ There are in fact serious objections to the simple cobweb theorem, let alone a generalised version of it. These are discussed in the author's article, "Demand and Supply Reconsidered," *Oxford Economic Papers*, June 1956. Some of them may be overcome, however, if we are prepared to postulate differences in the comparative advantages of entrepreneurs. Needless to say, it is unrealistic to assume that current prices will always be projected with certainty into the future, notwithstanding persistent disappointment of this expectation; and if we suppose that each entrepreneur holds identical expectations as to all prices, we thereby renounce any claim to solve the essential problem of decentralised systems—that of how economic order is secured despite the fragmentary and diverse character of the information possessed by individual entrepreneurs.

as a *deus ex machina* in order to convert our postulated set of consistent, optimising activities into the equilibrium position of a hypothetical system. But are we entitled thus to conjure up expectations as we please? Our interest is to establish a cause-effect relation between objective conditions and the activities of the group. Although this must come about through the mediation of states of expectation, we are surely obliged to postulate such of these as have some inherent plausibility or can somehow be accounted for within the framework of the model. Expectations are based on information, the availability of which, as we shall see, is a function of the kind of economic system or arrangement we choose to assume. They cannot be properly regarded as a wholly extraneous element for which we need have no responsibility.

It is commonly believed that expectations are satisfactorily taken care of merely by assuming their functional dependence on other elements of the system. Entrepreneurs are represented, as we have seen, as expecting future prices to maintain their current level or to be equal, let us say, to some average of the values they have attained in the past. Such "rules" for the formation of expectations may be simple or complex; although they usually in fact imply unanimity and certainty, and are rarely assumed to be modified by experience, yet these restrictions could presumably be removed. Nevertheless, the propriety of this whole method of approach seems questionable. We are rightly accustomed to assume that economic agents generally act rationally on the basis of their beliefs; we ought similarly to assume that beliefs or expectations themselves are rational, that they are based, that is to say, on adequate evidence or information.

The special significance of this requirement in the analysis of group activity has perhaps not been fully appreciated. The availability of primary information (about technical production possibilities, etc.) does not depend, in any obvious way, on the kind of system or organisation under consideration. If therefore we postulate a certain "state of the arts" or "given production functions," we are justified, for most purposes, in simply assuming that the average entrepreneur is acquainted with them. We cannot do this, however, in relation to secondary information (*i.e.*, information about what related members of the group will do), for the availability of this kind of information is quite certainly a function of the economic arrangements, or system, which we postulate. If we assume that consumers are fully informed about their own preferences and about the properties of all the goods they may buy, then the models we construct will indeed suffer in terms of realism and will offer a less than perfect account of the workings of the actual economy. Nevertheless, this simplification of actual circumstances might be appropriate in an idealised system, which, for some purposes, offered a tolerable approximation to reality. If, however, we assume that a producer has some secondary information regarding the projected activities of others with whom he is economically related, then we are committed to assume the

existence of an economic system in which such information would be available. We could assume, for example, that members of the system planned their activities in concert, or that they renounced their independence to put themselves under the orders of a central planning authority, which, by itself deciding what everyone would do, could dispense with any need for secondary information. But if both these solutions are put aside as inconsistent with free competition, how, then, are we to suppose that producers become adequately informed about such activities of others as will affect the profitability of their own investments? Although this may be the most fundamental of all the problems of group organisation, it is one to which our accepted theory of the perfectly competitive economy offers no satisfactory solution.¹

The problem arises, as we have said, from the fact that the activities of the members of the system, though inter-related, are taken independently. No individual member, it is clear, can decide what to do on the basis of primary information only, of information, that is, about such factors as production functions and consumers' preferences. He could not, for example, judge whether to invest in any particular direction without a minimum of knowledge about the supply plans of his competitors. In other words, it is of the essence of the competitive system that the profit opportunities open to one seller depend on the actions proposed by others, so that, for example, if A, B, C, . . . are all equally well placed to supply a given market, then A cannot rationally decide upon a particular level of output without some knowledge of what B, C, . . . *et al.* may do, while each of these similarly need some prior secondary knowledge of the intentions of A and others. This mutual interdependence clearly presents, for entrepreneurs, a barrier to obtaining the necessary secondary information, and, if we are to hope to show how a system can work, we cannot escape the obligation to explain how the barrier is overcome.²

¹ As is well known, both Walras and Edgeworth suggested procedures for the attainment of equilibrium. But neither the system of prices "*criés au hasard*," nor "recontracting," are convincing solutions to our problem, and in any case they involve a measure of concerted action which is inconsistent, at least with the spirit of perfect competition. The proposal that we should consider a model of perfect competition with an infinite number of markets embracing goods for sale at any future date likewise circumvents, rather than helps to solve, the real problem.

² An analogy may help to reinforce the point. The opportunities open to an entrepreneur in a perfectly competitive market might be compared to an unusual kind of lottery. Entry, let us suppose, is invited on the following basis. If subscriptions amount, by a certain date, to say £1 million, they will simply be returned in full; should they amount to less than this, they will be returned together with a share of a certain prize money; but should they exceed £1 million, they will be returned subject to a deduction depending on the excess. Now evidently, unless it were possible to make some assumptions about factors which independently restrict or condition how many people would enter the lottery, and what stakes they might make, it would be impossible for anyone to form a rational estimate of the chances of success. In the analogue of the competitive market there would be no basis for a rational investment decision; here also the possibility of making profits or losses depends on the total commitment of resources, while it is by no means obvious that any way of obtaining information about this is available.

The discussion in this article makes no mention of the theory of games, which is concerned with

This difficulty has frequently been noticed, but only to be pushed aside as a tiresome logical catch. Yet this reaction is curious in view of the attention which has been given to oligopoly and the so-called indeterminacy inherent in it. If there are only a few firms, then A, in formulating an output policy, must consider B's reaction to it, which may itself depend on what B thinks A might do in return; this being so, it has been recognised that without additional data we cannot hope to predict what the strategies and counter-strategies might be. The existence of many sellers, however, is supposed to eliminate this indeterminacy—but why should this be so? Where there are many producers, an individual entrepreneur planning his output still needs some knowledge of what the total supplies of competitors will be. It is true in this case that their plans will not be affected by knowledge they might have of what he in particular will do, for we can assume that his output will be small compared to the total; but this in no way makes their plans any more predictable by him. Their large number and their independence of his own particular actions do not obviously make competitive supply any easier to forecast than in the oligopolistic situation; the actions of individual entrepreneurs therefore appear equally indeterminate. Presumably the conviction that perfect competition, unlike oligopoly, has a determinate equilibrium is based on the fact the entrepreneur in perfect competition cannot affect the price at which he sells, which therefore acts as a fixed parameter in his decision-taking; but this is to ignore the fact that it is the expected future price which is relevant to him and that it is not known. One is not entitled to claim that prices, widely known and freely determined, constitute a sufficient system of communication; the current levels of prices are quite inadequate signals from an entrepreneur's point of view.

We have suggested that de-centralised systems can work only provided their constituent members can obtain the minimum necessary secondary information, and that the first objective of our analysis of them should be to explain how this is made possible. Yet although adequate knowledge is the basic prerequisite of purposive action, it cannot, of course, ensure that action will be taken or that it will be in a socially desirable direction. If therefore we are concerned to demonstrate how individual activities, though subject to no deliberate co-ordination, may yet, under certain conditions, produce a socially desirable result, our analysis must meet essentially two requirements. It must first explain how opportunities are made available and known to people in the system, and then go on to show that the opportunities will be of such a kind that action taken in response to them, and under the influence

what courses of action may be rationally chosen even when information is lacking as to which of several courses others may take. Our present concern, however, is not with the logic of choice under uncertainty, but with how, in a competitive economy, this uncertainty may be reduced to the extent compatible with purposive economic activity. Beyond a certain point, uncertainty would become intolerable to average entrepreneurs, and institutional change would be made in order to reduce it. Business may proceed in an informational twilight, but not in utter darkness.

of the type of motivation we assume, will further whatever is considered to be the general interest. The former requirement is, in a sense, the most fundamental, for purposive activities could not take place without an adequate basis of information, whether or not they resulted in desirable results; nevertheless, it is this requirement that our models of the competitive economy most strikingly fail to meet.

There appears to exist in our theory of the competitive economy no adequate analysis of how the supply of secondary information is functionally related to the structure of the system assumed. Certainly this cannot be provided here; but it is possible to set out, very briefly, some of the elements.¹ Producers could obtain information about the prospective activities of those to whom they are inter-related in three principal ways. The first of these, explicit collusion, needs no explanation; it is clearly inconsistent with perfect competition. The second, which we propose to describe, with some hesitation, as "implicit collusion," will be explained presently. The third, which is of a quite different character, exists by virtue of the existence of factors which condition or limit the activities of entrepreneurs independently of their estimates of the plans of those in a competitive or complementary relationship. These factors, which we shall simply call "restraints," will be explained more fully later.

The significance of the second way through which information may become available, implicit collusion, may be illustrated in connection with the general equilibrium "configuration" discussed above. This represented a set of activities which would be persisted in provided everyone expected prices to remain unchanged. We have now to ascertain the conditions under which it would be rational to entertain these expectations. An entrepreneur could rationally expect prices to remain the same if everyone else in the system continued in his present activities, but this they would do only if they could likewise expect all others (including the entrepreneur in question) to do the same. We seem therefore to be involved in the old difficulty; no one can decide upon his optimal activity without knowledge of what others (who are in the same difficulty) will do. Nevertheless, it seems that it may be possible, in this case, for a continuance of the *status quo* to be rationally expected. Let us suppose that everyone in the system expects primary conditions to remain unaltered and knows that profits are everywhere normal, thus realising that he cannot improve his position so long as others maintain their current activities; and let us suppose further that everyone knows everyone else to share his beliefs. Under these conditions, which though manifestly unrealistic, are possible in principle, everyone would be induced not to alter his activities, for they would see that each individually was powerless to improve his lot, but that if they all kept their course, at least no one would be worse off. Such a state of affairs may

¹ An adequate treatment of this subject is precluded by reasons of space; the author has been engaged in writing a book which endeavours to provide it.

perhaps be usefully described as implicit collusion, on the grounds that there is a general understanding that no one will alter what they are doing. It cannot, it would seem, be regarded, in this context, as anything but a somewhat fanciful possibility, but it is difficult to see how else, in a perfectly competitive economy, entrepreneurs would be entitled to entertain rational and certain expectations that all prices would stay the same.¹

Although equilibrium might be maintained, it clearly could not be reached by means of implicit collusion of this kind. If a number of people were asked to write down each a number, in the hope that the sum would equal 100, they would clearly have, in the absence of explicit collusion, only an infinitesimal chance of success. But if they did miraculously happen to choose an appropriate set, and knew that they had done so, it is reasonable to expect that they would then write down the same numbers if invited to play the identical game on further occasions.

We have to conclude therefore that the beliefs required to maintain the perfect competition equilibrium could be rational (*i.e.*, based on information which is available in principle), though only under very special conditions and in a very special way. If, however, any configuration other than the equilibrium is assumed to exist, we have as yet discovered no way in which entrepreneurs could obtain any information about each other's projected activities and therefore no way in which rational expectations could be formed.

We are, of course, frequently inclined not to take this sort of difficulty very seriously, for the reason that it is overcome somehow in the real world. But the real world is not perfectly competitive, and there exist in it (apart from collusive arrangements) a variety of factors, usually classed as "frictions" or "imperfections," which make possible a third, and practically important, way in which secondary information becomes available. According to the forms and strengths which such "restraints" assume in particular markets, adequate information and therefore economic adjustment may or may not be possible. Only the briefest account of this can be given here.

Let us suppose that there arises a demand for a particular commodity. This may be assumed to create a general profit opportunity, but it need not at the same time create particular profit opportunities available to individual entrepreneurs. A profit opportunity which is known by and available to everybody is available to nobody in particular. A situation of general profit potential can be tapped by one entrepreneur only if similar action is

¹ The notion of implicit collusion has a respectable history. Hume, for example, in discussing respect for property, refers to it in these terms: "When this common sense of interest is mutually expressed, and is known to both, it produces a suitable resolution and behaviour. And this may properly enough be called a convention or agreement betwixt us, though without the interposition of a promise; since the actions of each of us have a reference to those of the other, and are performed upon the supposition that something is to be performed on the other part. Two men who pull the oars of a boat, do it by an agreement or convention, though they have never given promises to each other." (*A Treatise on Human Nature*, Book III, Part II, Section II.)

not intended by too many others; otherwise excess supply and general losses would result. In other words, a general opportunity of this kind will create a reliable profit expectation for a single entrepreneur only if there is some limitation upon the competitive supply to be expected from other producers. Corresponding to this negative condition appropriate to the production of substitutes there is a positive condition appropriate to complementary production. In general, a producer will need to know both that the production of complements (such as raw materials) will be adequate and that the production of substitutes will not be in excess. For the sake of brevity, however, we shall ignore the existence of complementarity and concern ourselves only with the necessary restrictions on competitive production. We have seen that we cannot in general assume that producers could have knowledge of their competitors' plans. But there is no logical objection to them having information about the *maximum possible* competitive supply in some relevant future period, if supply is bound by restraints. They may also be able to estimate likely minimum supply (either competitive or complementary) if this is determined by the momentum of previous commitments.

Let us glance briefly at the nature and effects of these restraints. They will impose themselves, at any particular time, with different force on different firms so as to limit their ability to increase either production or sales. An entrepreneur need fear no threat of increased competitive supply if others are unaware of the unsatisfied demand; he enjoys a form of monopoly, until of course the knowledge which he alone possesses becomes diffused. In this way, paradoxically, the imperfect distribution of information may sometimes facilitate economic adjustment. But even those firms who are aware of the increased demand will have a limited capacity to meet it, depending on the reserves and the limited external finance which is currently available.¹

Firms which were able to mount the required increase in production might still experience difficulty in penetrating the market in which demand has increased. Not only do the firms already in the industry have "economies of experience," they also, as Marshall indicated, have "particular markets," in which, by virtue of goodwill or other ties, they have a significant advantage. These restrictions or "imperfections" could reasonably be regarded as natural, but they shade into others which are artificial, in the sense that they were devised for the purpose. A tacit agreement to live and let live may become formal market sharing, and entry may be impeded by deliberately erected barriers. Firms may agree to bind their hands and seek

¹ Here we may recall the celebrated criticism of perfect competition made by Sraffa, when he pointed out that the conditions of the model left the size of the individual firm indeterminate. Sraffa was concerned to point that, as the supply response of the individual firm in perfect competition was indeterminate, the continued survival of many firms in the industry could not be relied upon. We require limits to the supply potential of each firm and to the number of firms which can respond, but for a different reason. Unless a firm can set some upper bounds to the possible volume, in the near future, of competitive supply, it will not be prepared to invest.

to bind those of others. All this helps to make the world a safer place for entrepreneurs; by narrowing the possibilities of competitive supply it makes the market of each more secure.

Almost all the protective or restrictive influences to which we have alluded are inconsistent with the letter or the spirit of perfect competition; moreover, they are commonly the objects of criticism; if natural, they are regretted; if artificial, they are often condemned outright. And of course this long-standing attitude has its good reasons. It is certainly necessary, in a decentralised economy, that the activities of different firms be kept in step, and these so-called imperfections are part of the machinery which ensures this. But there are other objectives which may sometimes be in conflict with this one. Entrenched monopoly positions may bring about the misallocation of resources and may weaken the process of competitive selection by which the efficiency of the private enterprise economy is furthered. Many would therefore be tempted to regard the restrictive influences which we have described as "necessary evils," comparable, let us say, to the position of the laws in civilised society—but it is well to remember the highly paradoxical nature of such a classification. In addition, one cannot assume that *any* degree of restrictive influence will be injurious; factors which give only short-run stability to a firm's market will rarely tempt it to exploitation and to neglect of efficiency. It would be helpful in practice if we could assume that natural imperfections were tolerable, whereas artificial man-made ones were not. But whether or not such a division might be a useful policy guide, there seems no theoretical justification for it; there seems no reason to believe that the optimum degree of "imperfection" is reached exactly at the point where ordinary market restrictions begin to be supplemented with protected measures designed specifically for the purpose.

If, then, we are right, and our model of the purely competitive economy is seriously deficient in the way we suggest, it is interesting to speculate on the reasons for its adoption and for the strong grip which it continues to hold on our minds. The comparative neglect, in accepted economic doctrine, of the informational aspects of hypothetical systems may, perhaps, be the result of a variety of causes. The broad rationale of the private enterprise system was, of course, appreciated long before the development of modern equilibrium analysis. It was realised that, if the demand for an article increased, the profit opportunities created would permit private self-interest unwittingly to further the public good—subject, of course, to qualifications which were paid a varying degree of attention. It was realised also that if the total supply of the article were in single hands, then the terms of sale could be influenced to the benefit of the seller, but at the general disadvantage; and it could also be shown that both ignorance, and the artificial restriction of enterprise, could hamper the invisible hand. These ideas, in the latter half of the nineteenth century, were expounded with a new degree of logical rigour and precision and fitted together so as to form a hypothetical system

purporting to represent the workings of the economic system reduced to essentials. There were to be no artificial restrictions and no impediments due to ignorance; by assuming atomistic units and the absence of collusion between them, the terms of any exchange were safely withdrawn from individual control. Such a system of "perfect competition" was not represented, at least by reputable economists, as accurately describing the actual state of free-enterprise economies, from which all elements of restriction, ignorance and monopoly had not been purged. But it stood as an ideal or model form of organisation—strictly speaking only a logical as opposed to an ethical ideal, although the distinction was not always sharply made. Those who objected to the model in terms of realism could easily be reminded of the abstraction inherent in all scientific thinking and of the need to concentrate on essential elements. On account both of its pedagogic convenience and its suitability as a basis for extensive formal and mathematical elaboration, perfect competition obtained a central place in theoretical discussion, and the many qualifications with which, let us say, Marshall would have hedged the concept, fell into the background. There was little realisation that the elements which had been banished as "imperfections" were necessary, in some measure, for the provision of adequate information, that an unequal distribution of knowledge and the existence of "frictional" restraints were required in order to make the system work.¹

One suspects that our discussion of the competitive model may also have suffered, on occasion, from a confusion of perspective, from confounding the view-point of the model builder himself and that of his creatures within the model. For the creator there is no problem of knowledge, for the objective facts about the system appear as postulated data from which could be deduced (or so it was believed) the equilibrium configuration. Was it always appreciated that information about these "determinants," that "perfect knowledge" in this sense, would have been of no use to the members of the system even if they could ever be assumed to possess it? To put the matter somewhat differently, was it realised that the problem of knowledge for a member of a group was not the same as that of an isolated agent working in a passive medium—that the availability of secondary information, as we have called it, is a function of the nature of the economic system postulated, so that "adequate knowledge," far less "perfect knowledge," may in fact be incompatible with certain kinds of economic arrangements? The question is not so much whether these contentions would have

¹ The usefulness of the perfect competition model has been compared to that of mechanical statics. The latter, though it assumes no friction, nevertheless enables us to determine the equilibrium position of a system of forces. An economic model which abstracts from all friction can, it is argued, perform a similar function. But the analogy is treacherous. It ignores the fact that, in economics, both the maintenance and the attainment of equilibrium require the existence of certain beliefs or expectations, which have no strict counterpart in mechanics; and by doing so it may conceal the need to show how such expectations could be rationally formed in the type of system we assume.

been explicitly denied, but whether anything approaching full weight was given to them in the construction of theoretical foundations.

The argument of this article may be briefly summarised, in conclusion, as follows. Our accepted analysis recognises that equilibrium requires the existence of certain expectations, but does not recognise that expectations, if they are to be rational, must be based on available information. Nor does it take account of the fact that the availability of certain essential information depends on the economic arrangements or system which we postulate. These contentions were maintained in connection with the so-called general equilibrium of perfect competition. This was found to represent a set of optimising activities consistent with given constraints and with each other, but we denied that it was a position towards which a hypothetical perfectly competitive system would gravitate. Our denial was based on the fact that perfect competition, strictly interpreted, would fail to afford adequate secondary information for purposive entrepreneurial activity. If concerted action were ruled out, only the existence of certain restraints (or "imperfections") in appropriate form and strength would make available the information required for economic adjustment.

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PROFIT SHARING BETWEEN PRODUCING COUNTRIES AND OIL COMPANIES IN THE MIDDLE EAST¹

It is widely taken for granted that the oil-producing countries in the Middle East are being in some sense "exploited" by the foreign oil companies; that their share in the revenues from the production and distribution of oil is "unfairly" low; that their participation in the different operations of the companies is unduly restricted; and that foreigners are benefiting from "Arab" oil at the expense of the rightful beneficiaries. The purpose of this paper is to explore some of the underlying considerations determining the sharing of oil revenues between oil companies and producing countries, and the related subject of nationalisation, and to investigate what meaning can be given to the notion of economic "exploitation" used in an invidious sense.

The vested interests in Middle East oil are so diverse and varied, the financial relationships between the various interests are so complicated, the formal terms and practical applications of the oil agreements are so difficult to comprehend, and the organisation of the distribution of oil is so intricate, that it is no wonder that even experts seem at times confused, let alone the general public. I shall not be concerned here with any particular agreements or negotiations; I merely want to re-examine the fundamental principles involved in the matter and to point out a few of the significant economic relationships.

Some of the propositions advanced in the various discussions on the subject seem implicitly to assume that the sharing of oil revenues should accord with some principle of "natural law"—this is often implied, for example, in the proposition that the "Arabs" as such have a "right" to all oil profits quite apart from legal arrangements. Here, I shall assume that all rights in the matter arise from man-made law, but that the governments of the countries under whose lands the oil lies have a legal right to deal with its exploitation as they see fit, with due respect for existing contractual arrangements. I shall also assume that governments have the right to nationalise the property of foreigners as well as that of their own citizens upon payment of fair compensation, and that such action is justified when the economic interests of the country are thereby furthered.

At present the sharing of oil revenue is primarily a matter of bargaining between the different interests concerned, the bargaining being influenced by political as well as by economic considerations. Since I am not concerned with political controversies among the leading world powers over the

"control" of Middle East oil, I shall treat the oil companies as privately owned concerns interested primarily in making profits. The desire for an assured access to adequate supplies of oil for military and strategic as well as for economic purpose has made control of the sources of oil a major concern of many governments. Thus inter-governmental political manoeuvring has been a dominant characteristic of the past controversies over oil concessions, and the present position of the oil companies in the Middle East is very largely the result of the past policies of and rivalries among certain Western powers. As a reaction against this, demands have been increasingly heard recently that the West should put its oil relationships with the Middle East on a "business basis"; this presumably means that military and political considerations should no longer dominate the scene. In order to analyse the nature and probable consequences of oil relationships on a "business basis," I shall, therefore, neglect the military and political interests of the Western powers.

I shall deal first with the basic conditions affecting the bargaining process, secondly with the question of "exploitation," and thirdly, with some problems of international policy.

I. THE PROFIT-SHARING BARGAIN

We start from the present position; we are not concerned with political and economic conditions relating to earlier times and earlier contractual arrangements, but only with the considerations affecting the granting of new concessions or the re-negotiation of present agreements. The governments of the producing countries have reserves of oil under their territorial jurisdiction. Some of the oilfields are actually being worked by foreign companies under agreements which, on specified terms, enable them to explore for oil, produce and sell it. In other areas, not yet in production, exclusive concessions have been granted; in still others new concessions may be granted; and finally, some existing agreements contain provisions requiring the company, under specified conditions, to give up part of the area covered by the agreement and not yet in production. The governments of the producing countries may cancel existing concessions (we assume with fair compensation) through nationalisation or otherwise and may re-negotiate existing agreements as well as grant new concessions. But whenever a foreign company is involved, the share of the revenue obtained by a government depends entirely on the agreement it can extract from the oil company. This is obvious in the case of new concessions; but even with respect to an established company the government is dependent on the willingness of the company to give up its profits, since the oil revenues arise in foreign currency outside the country, to which the government does not have access, and which it cannot even tax without the company's consent.

We have then, in essence, two opposing interests both benefiting from oil

¹ I would like to thank for their criticism and suggestions all those who read earlier drafts of this paper, especially those from the University of Baghdad, the Iraqi Government service, and the Iraq Petroleum Company, Dr. Yusif Sayigh of the American University of Beirut, Professor Fritz Machlup of the Johns Hopkins University and my husband, Professor E. F. Penrose.

production, the benefit each obtains being largely determined by the terms of the bargain it can make with the other. One of the interests invests capital to start the industry and runs it, the other supplies the basic raw material, which is a "wasting" asset—the extraction of the oil permanently reducing the resources of the country. Both are interested in promoting the most profitable long-run expansion of the industry, but each individual government is concerned with the profitable expansion of its own oil production, while the companies, with some choice of country in which to invest new capital, will be most interested in those countries from which the higher returns can be expected.¹

Let us now examine the factors determining the outcome of the bargain that can be made. For the present I shall include royalty payments per ton of oil produced as well as all the various technicalities affecting the actual share of the revenues obtained by a producing country (as distinct from the nominal percentage share) in the category of "profit-sharing," although in fact there are many reasons why a country's actual share of the "profits" may diverge from any announced percentage share.² I want first to consider the respective positions of an individual country and an oil company and the nature of the bargaining process between them when there is no competition in the industry and when the producing country cannot run it. It should be kept in mind that "running" an industry includes selling the product.

Stripped of all complicating variations and special circumstances, the essence of the matter can be stated in its simplest terms as follows: The proportion of its profit that a company will be willing to give up depends on its estimate of the cost of meeting the government's final demands compared with the cost of resisting them, up to the point where the loss in either case makes the business unprofitable.³ The demands of the government will depend *either* on the loss it believes it can inflict on the company by not giving or by cancelling the concession under negotiation, that is, on its estimate of the value of the concession to the company, *or* on the amount it thinks the company will be prepared to give up in order to avert political disturbances

¹ The large oil companies control oil properties in numerous countries throughout the world and their production policies in the different countries are subject to many influences, including tariff arrangements, the domestic policies of national governments, questions of the political security of their investments and reliability of their supplies of oil, and private agreements aimed at "regulating" supplies of oil. Most of the companies control both low- and high-cost sources of oil but do not necessarily put the greatest emphasis on expanding low-cost oil.

² See below p. 243. Many of the demands made by a government often affect the costs of the company rather than the profit-sharing arrangements. But since increased costs reduce the profits available to both parties, we can, for our purposes, treat such demands as part of the profit bargain. They are usually quantitatively unimportant in any case.

³ It is possible, indeed even likely, that an oil company would concede some of the demands of a government simply because it considered the demands to be fair, and not at all because it believed that to do so was necessary to safeguard its position. But it would be extremely difficult to separate concessions made solely in a "spirit of fairness" from those made because the company was convinced that a "spirit of fairness" would in the long run assist in safeguarding its position.

and to maintain political goodwill. The latter alternative applies in the case of negotiations with a company already established in a monopolistic position in the country and in circumstances where the government cannot run the industry.

In the absence of intergovernmental political manoeuvring (which we have ruled out of our terms of reference), the problem with respect to new concessions is quite straightforward and need not detain us. The cost to the company of meeting the government's demands is the share of profit it must give up in one form or another; it need merely compare what is left to it under the terms proposed with the return that can be expected on alternative uses of its resources, making due allowance for differences in risk. It can always bargain for the best terms possible, but if the government sticks at too high a point, the company has no alternative but to refuse the concession. The cost of resistance is the loss incurred if the concession is not obtained. What the government demands depends on how badly it wants the oil production, how much it is aware of the value of its oil and how much it thinks it can persuade the company to give.¹ If the country cannot itself discover or produce the oil, it will gain by permitting the oil company to operate for nothing (if necessary it may even be worth the government's while to subsidise the company in some form), since exploration and production would alone bring considerable revenue and other advantages to the country in the form of wage payments and other outlays and of social and technological contributions of various kinds. A skilful government negotiator, however, may easily extract substantial royalties from the company if he realises the value of the concession he has to give.

But with respect to the re-negotiations of existing agreements with companies already established in the country the matter is more complicated. Although the cost to the company of acquiescing in the government's demands is the additional profit lost in one form or another, the cost of resistance is not necessarily the loss of the concession and with this the future profits expected, for the company is in a strong position to precipitate an economic crisis which may drive the negotiating government out of office. The company need take no positive action; by simply refusing to budge it forces the government to take the risk of action which, once taken, cannot in the nature of the case, be easily retreated from. Furthermore, the government's action must be political—it may promote publicity which arouses the populace against the oil companies; it may condone or incite political harassment, such as acts of sabotage; it may and indeed is likely to threaten nationalisation on terms unacceptable to the oil company. But where the economy of a country has become heavily dependent on the continuance of

¹ I must emphasise that we are only concerned with present-day conditions. There is little doubt that some of the early agreements were made by rulers who knew little of what they were signing away and who were attracted as much by the immediate cash for themselves as by any calculation of the future returns to the country.

oil revenues, while at the same time foreign enterprise is required to run the industry, an existing oil company, if it has a virtual monopoly or acts in concert with other companies, is in an extremely strong position *vis-à-vis* any single government, for the latter courts political disaster if it gets into an all-out fight with the company. The progressive economic deterioration following the cessation of oil revenues is likely to cause greater and greater political difficulties for the government, its eventual repudiation and the substitution of a government pledged to re-establish the flow of oil revenues. The cost to the company of such a fight may be extremely heavy, but given the structure of oil interests in the Middle East, it is not likely to be disastrous for any of the big companies while it is likely to be disastrous for the government in power.¹ A company will naturally seek to avoid such an extreme show-down, but only if the expected cost of resistance exceeds the ultimate gain if the resistance is successful—and if the government's demands are very high in relation to existing royalties the company may stand to gain a great deal by resisting the government.

In deciding whether or not to refuse particular demands of the government, a company will not, in the first instance at least, be as much concerned with the probable cost of a show-down as with the question how much pressure it can bring on the government without forcing it to take serious action. This last question may be an extremely delicate one. Both the company and the government may be well aware of the government's fundamentally weak position if the battle lines are drawn, but the position of the parties is such that, when the company refuses to yield to its final demand, the only thing the government can do is to threaten nationalisation. Once it makes such a threat publicly, it may easily become a prisoner of its own acts and be unable to back down, for in all the Middle Eastern countries popular feeling is easily aroused against the oil companies, and this puts great pressure on the moderate members of the government and strengthens the position of the more extreme members.² If the people can be appeased by the oil company's acceptance of the government's terms, the matter may

¹ This point is well illustrated by the conflict between the Iranian Government and the Anglo-Iranian Oil Company in the years 1948-53. To be sure, both the British and American Governments intervened in the dispute, but without much avail, and the dispute ended with the fall of the Iranian Government and the practical victory of Western interests. The company suffered heavily; but during the years 1952 and 1953 when it had no Iranian oil its profits were greater than ever, derived of course from other sources than Iran. See the *Annual Reports and Accounts* of the company and also the discussion by Benjamin Shwadran, *The Middle East, Oil and the Great Powers* (London: Atlantic Press, 1956), Chapter VI. Stephen H. Longrigg's account in *Oil in the Middle East* (London: Oxford University Press, 1954), Chapter X, is more sympathetic to the company than that of Shwadran. The latter implies that nationalisation as such was the primary issue; Longrigg places the emphasis on the terms of the nationalisation. Both accounts should be read.

² The ordinary people of the Middle East, knowing nothing of the oil industry, are easily convinced that their government could run it as well as the foreign company. The relative success with which the Egyptians have been able to operate the nationalised Suez Canal has enormously strengthened the belief in the more developed countries such as Iraq that similar success would attend the nationalisation of oil.

end with a capitulation by the oil company, but there is likely to be a long-run enhancement of political antagonism on all sides. If the government is forced by popular pressure to go through with its threats of nationalisation on unacceptable terms, the oil interests in the end may prevail through the downfall of the government in consequence of economic distress and in spite of the emotions of the masses.

The knowledge of most Middle Eastern governments that they really cannot afford a controversy that causes the cessation of oil revenues, together with the fact that it is they who must undertake positive action if the oil company cannot be persuaded by argument, puts them in a relatively weak position. A responsible government realistically attempting to increase its share of oil revenues must keep the company aware of the need to avert political action, but at the same time must not commit itself too deeply to an action it knows would be disastrous in the end. Paradoxically enough, this bargaining strength of the government may arise as much from its internal political weakness as from its political strength, as much from its inability to prevent disorder if the people become aroused as from its ability deliberately to interfere with the oil operations, as much from administrative incompetence as from Machiavellian policies.

On the other hand, the company's awareness of the danger of forcing the government into a position from which it could not easily retreat may induce it to accept even stiff demands in order to avert unpleasant and costly conflict. The dangerous aspect of the matter lies in the political repercussions over a wide area if the oil company miscalculates the pressures on the government and fails to give in before it is too late or if the government insists on more than the company is willing ultimately to give. So long as popular pressures are not great and the government's revenues are significant in absolute amounts negotiations may proceed reasonably peacefully, but clearly anything that increases popular agitation against the oil companies or against the terms of existing agreements, such as crises in other countries or even the mere knowledge that a government has obtained better terms in some other country, will reduce both the willingness and the ability of the government to compromise, and must therefore increase the company's estimate of the risk of loss if it makes no further concessions to government demands.¹

In these circumstances there is every incentive for oil companies to adopt accounting and other techniques designed to permit a maximum effect on public relations with a minimum actual concession. For example, the prices at which the oil is sold as it leaves the control of the producing companies are posted by the companies. But for the most part the oil goes in pre-determined shares to the foreign oil companies who own the producing companies. These same companies, or their affiliates, also control the major distribution channels of the world, and it is extremely difficult to believe that

¹ The Iraqi agreement, for example, makes express provision for re-negotiation if better terms are obtained in neighbouring countries.

they do not have a powerful influence on the prices with reference to which the profits to be shared are calculated and consequently on the revenue obtained by the producing countries. It has often been alleged that the prices are unfair, and arbitrarily set to the advantage of the oil-distributing companies. It is not, therefore, so strange as it appears on the surface that one of the demands of the oil-producing countries has been that they should share in the profits made by the major oil companies from the distribution of Middle East oil in world markets. Furthermore, the calculation of the "costs" to be subtracted from the total revenue in determining profits is, as every accountant and economist knows, subject to any number of arbitrary judgments. Any announcement that a high percentage of profit is to go to the government has a popular appeal, but in fact the actual percentage granted may be much less significant than the method of calculating total profit.

This analysis of the essence of the factors underlying the bargaining process may seem extreme; and indeed for some countries, in particular the smaller autocratic sheikdoms, it would need considerable modification. Its chief purpose is to demonstrate that, in the absence of effective competition and when the producing countries are unable to run the industry (which includes selling the oil) there is no such thing as oil relationships on a "business basis." We are dealing essentially with a problem in bilateral monopoly slightly modified by marginal competition, and with a bargaining game in which there is a strong element of bluff; but one of the pawns in the game is the political stability of a sensitive area of the world. It seems highly probable that the Middle Eastern governments have obtained as much of the oil profits as they have with few overt crises precisely because the oil companies were willing to pay for political goodwill and because the profitability of oil in the Middle East has been so great that the companies could well afford to pay handsomely.

It remains to consider how the bargaining process would be affected by the introduction of competition, either competition among the oil companies for oil concessions or potential competition from the government made possible by growing ability to run the industry.

Again, the position with respect to new concessions need not detain us. If the government can itself exploit its own oil resources it naturally will not give a foreign company the right to do so unless it can obtain a share of the profit at least equal to the profit it could make by running the industry on its own account when all opportunity costs of the resources required are taken into consideration. This implies that the government is in a position to produce and sell oil as efficiently as the companies and only permits them to operate because it has equally or more profitable alternative uses for its own resources.¹

¹ As we shall see below, this last consideration means that it will not necessarily be advantageous to a developing country to exclude foreign companies merely because it can produce as efficiently as they can.

If there were effective competition among the oil companies for a concession, this would plainly push the share of profits offered to the government up to the point where the company obtaining the concession retained only "normal" profit which, for our purposes, may be defined as the amount of anticipated profit that would just induce the company to enter or continue to invest in the oil industry of the country. In these circumstances, the government should be able to extract nearly the full value of the concession from each company; whether the introduction of effective competition would actually mean an increase in the government's revenue, however, is an open question which is discussed below. In the past, as noted above, competition for oil concessions was largely restrained by intergovernmental agreements, in which military and political considerations were dominant, as well as by the private agreements and financial inter-relationships among the oil companies themselves. Many, if not most, of these restraints still exist, and thus even for new concessions there is only marginal competition such as that recently offered by the Japanese and Italian oil companies, which are outside the combines at present controlling Middle East oil.

Even a small amount of competition for new concessions, however, is significant, for whenever any producing country obtains terms in an important agreement better than those prevailing elsewhere, an immediate pressure tends to be created for the revision of old agreements, because the amount the government can reasonably expect to get and the point at which it would be willing to compromise are both raised.¹ In addition, better terms obtained from a new competitor in the industry may encourage the rulers of the producing countries to attempt to withdraw concessions on some areas over which exclusive rights were granted under earlier agreements and which often include vast parts of the country not yet in production and relatively unexplored from a geological point of view when the agreements were made.

The companies are very much aware of these dangers to their positions and naturally seek to avert them—a difficult task if loose cartel arrangements had to be relied on; much easier when the various producing companies are jointly owned by a few dominant international concerns, which is largely the case in the Middle East.²

The government would also be in an extremely strong position with respect to established oil companies if it could run the industry without their help and obtain revenues not significantly below those previously obtained from the companies. In these circumstances, it would no longer risk political

¹ The rapid spread of the so-called 50-50 arrangements after the Aramco agreement with Saudi Arabia in 1950 is an example of this; and almost immediately after the announcement that 75% had been obtained by the Emir of Gatar from the Italians and Japanese, increased pressure was evident in Iraq for a revision of that country's agreement with the Iraq Petroleum Company, which is jointly owned by British Petroleum, Royal Dutch-Shell, Compagnie Française des Pétroles and a combination representing five American companies.

² This is not intended to pass judgment on whether or not competition would be desirable from other points of view. See Section II below.

disaster if it nationalised the industry. Its threats of nationalisation if the company did not comply with its demands would no longer be dangerous bluff but would have to be taken seriously, and the company's chief source of bargaining strength—the economic disaster for the country if the oil revenues cease—would disappear. If necessary to keep its concession, the oil company would gain by continuing to give up part of its profits so long as the remaining return on its investment was at least equal to the return (appropriately adjusted for risk) it could expect if it invested compensation funds elsewhere,¹ and skilful government bargaining could conceivably force the company's share down to this level.

It would not be to the country's economic advantage, however, for the government to demand more than this, and thus force the company out, or to demand a share that would inhibit further expansion of the company's activities and create excessive emphasis on the short run, unless the government could operate the industry at a profit not only equal to but considerably greater than the revenue obtainable from the company over the life of the oilfields involved. A foreign company brings many indirect advantages to a developing country, including an increased rate of innovation, a freer flow of technical information leading to the more rapid application of advanced technology, and a greater supply of technical and managerial personnel. Most underdeveloped countries accepting foreign capital or foreign enterprise urge that the foreign companies should employ domestic personnel as far as possible, especially in technical and managerial positions. The primary purpose is to ensure that foreign enterprise provides opportunities for the nationals of the country to develop their own skills and executive potentialities. But as such countries develop, many of the men and women so trained will be required in other fields, and it may well be desirable to use to the fullest possible extent opportunities for training made available by the foreign companies (which presupposes the presence of foreign personnel capable of providing the training) and also to withdraw trained and capable domestic personnel from the companies for other uses.² Nationalisation may well mean that a larger proportion of domestic managerial and technical skills will have to remain in the oil industry and thus will not be available elsewhere.³ Paradoxically, many people in the underdeveloped countries

¹ I do not want to discuss the difficult problem of how "fair" compensation can be determined. Clearly the amount of compensation the company expects to get would be an important consideration in its calculations of the profitability of remaining in the country. For our purposes it is not necessary to examine this question; we merely assume that some agreed formula can be found.

² The Iraq Petroleum Company, for example, has an excellent training programme which not only provides skilled people for the company but for outside firms as well. In particular, those who are unable to reach the high standards necessary to complete the programme successfully often take positions in other firms where the training they have had gives them substantial advantages over others and benefits their employers as well.

³ It need not do so if the nationalisation were carried through reasonably amicably and if the new owners were able to retain or obtain the foreign personnel they needed. But it is almost certain that the ease with which qualified foreign personnel could be obtained would be substantially reduced.

complain of the shortage of capable technicians and capable management and at the same time urge the elimination of the foreign supplement to the available supply.¹

In addition, the expenditures of the oil company in its ordinary operations in the country as well as for investment in further expansion and for taxes are an addition to the foreign exchange available to the country and these, too, would be lost on nationalisation. Thus, even if we should make the assumption that a country could run its oil industry at a total profit equal to that made by the oil company before royalties and taxes, it still might not be advantageous to the government to buy out the company. The government, in calculating the advantages of nationalisation, should take into consideration the alternative product that could have been produced by those of its resources which must be diverted into the oil industry (*e.g.*, the loss of productivity in other industries due to the reduced supply of managerial talent); the foreign exchange previously contributed by the oil industry in its operating expenditures, taxes and further investment expenditures; the amount of profit the company would have been willing to hand over to the country; and the compensation payments agreed on.

In fact, of course, even apart from problems of marketing the product, which we discuss in the next section, it is extremely unlikely that any of the producing countries will be able for some time to manage their oil industry as efficiently as the oil companies can, although some of them might well be able to produce oil with the help of individually hired foreign technicians. It would seem, therefore, that nationalisation of the oil industry would rarely be profitable in any of these countries unless the alternative opportunities available to the oil companies or the rate at which they discounted the future made them hold out for such large shares of profit that the government would gain even after making allowance for less efficient operations and the considerations mentioned above.

Nevertheless, the ability to run the industry without risking economic disaster would put the government in a strong bargaining position *vis-à-vis* the oil companies, which would probably enable it in time to extract the lion's share of the profit from oil, and which would also help to reduce those political elements which now make it impossible to put East-West oil relationships on a "business basis." But inability to sell the oil is the enduring difficulty; this makes even the reasonable and reflective Arab nationalists feel at the mercy of the oil companies and they passionately believe that the companies are exploiting their country in an oppressive sense.

¹ On the other hand, there are sometimes acute problems arising in an underdeveloped country because a growing group of middle class "educated" young people have difficulty finding suitable employment, and the government tends to look to the oil companies to help solve this unemployment problem. To some extent the problem arises because of an unwillingness of the young people to seek employment in agriculture and other occupations of low prestige value and because of the failure of the government to encourage small-scale private industrial enterprise.

II. THE QUESTION OF EXPLOITATION

The term "exploitation" has no generally accepted meaning in economic analysis (except in the Marxian sense); it is full of ambiguities and is, like the word "imperialism," used more often as an undefined term of abuse than for serious analytical purposes. But the notion has a tremendously important influence on the actions of the people in the less-developed countries of the world, who often go so far as to treat all foreign investment as a form of exploitation.

Most of the serious charges that exploitation exists rest implicitly or explicitly on the assumption that foreign interests use their superior political or economic power to prevent the exploited country from making the most profitable use to itself of its own resources. This, broadly interpreted, provides a reasonable definition of exploitation for our purposes;¹ it does not imply that foreign investment is necessarily exploitative in itself, for if the profits of a foreign firm are high entirely because of its efficiency in production or in meeting demand, one cannot assume that the foreign firm is in any way preventing the domestic economy from using its resources more advantageously, even though the firm pays out a large proportion of its profit to foreign shareholders. On the other hand, even when the productivity of a foreign firm is higher than that which any domestic firm could hope to attain, a large part of its profit may nevertheless be traceable to factors other than its greater productivity, for example, to monopolistic restrictions enforced by the firm within the country, and it is then possible to inquire whether this leads to a less advantageous use of other resources and therefore whether exploitation exists. Thus, neither the actual amount of profit earned, nor the efficiency of the foreign firm, necessarily provide by themselves evidence of exploitation or the absence of it.

Apart from a number of popular fallacies which are not worth discussing, such as the notion that *any* oil profits going to foreigners imply exploitation, most of the substantial complaints that oil companies exploit the producing countries can, in the light of the preceding analysis, be reduced to the charge that the oil companies weaken the ability of the producing country to increase its share of the profits from oil by: (a) excluding competition for oil concessions, and (b) retarding the growth of the producing country's ability to run the industry, including its ability to market the product. In addition, there are charges of political interference and domination which are outside the scope of this paper.

(a) We have seen that, *other things being equal*, a producing country can be expected to obtain more advantageous terms in its oil agreements if there is substantial competition among the oil companies. Therefore, to the

¹ It should be noted that to accept this definition of exploitation is to look at the matter from the point of view of the producing country and not from the point of view of the economist analysing the optimum utilisation of the world's resources.

extent that competition is effectively restricted by the blanket nature of existing concession agreements (covering very large territories, if not the whole country), by understandings among the major oil companies with respect to the territories in which each will operate, or by the exercise of power to control distribution channels and to restrain the competitive entry of newcomers, the charge of exploitation *may* be justified. The past agreements among companies dividing the Middle East among them have often been pointed to by the Arab nationalists as humiliating examples of exploitation, and it can be safely said that competition, for all of these reasons, is still effectively restrained by the major international oil companies operating in the Middle East. But this statement does not yet indicate whether or not the restraint of competition actually prejudices the producing country and therefore whether or not they are "exploited."

Effective competition presumably implies the existence of a considerable number of competing companies; no means has as yet been found, even by the United States anti-trust authorities, for making two, three or even four companies compete effectively if they prefer not to do so. It can be cogently argued that because of the immense size and speculative nature of the original undertaking in the geographically and politically inhospitable countries of the Middle East, only big, financially responsible companies could enter the field with any assurance of success, and that this itself limited the possibilities of competition. On the same grounds, it can be reasonably held that restraint of competition was necessary to encourage such companies to undertake the risks involved. In addition, it is entirely possible that the producing countries might be worse off if oil concessions had been granted to many companies on a competitive basis, not only because the value of each concession would be substantially less and the royalties each company would have been willing to pay substantially smaller, but also because the utilisation of their oilfields would have been more costly in the short run and wasteful in the long run. In other words, it can be plausibly argued that the same amount of investment would not have been forthcoming under a competitive regime in the industry, and that neither the efficient utilisation of oilfields nor the organisation of pipeline transportation and world-wide distribution would have been possible. Unlimited competition in oil production and distribution is held by many, if not most, oil experts to be not only wasteful but completely impracticable, for "other things" do not remain equal when a competitive and monopolistic organisation of an industry is being compared.

Thus the argument that the monopolistic position of the oil companies necessarily involves exploitation of the producing countries, in the sense that the countries would be better off if there were extensive competition, is by no means conclusive. Nevertheless, the fact remains that the absence of competition, for whatever reason, does give the oil companies a strong hand in bargaining and forces the producing countries to put the emphasis either on politics or on the necessity of developing their own ability to run the industry.

(b) It is charged that, by restricting the development of trained local personnel and by barring access to world oil markets, the oil companies interfere with the growth of the producing countries' ability to run the industry. In the first case, the complaint is that the companies fail to hire the nationals of the country for senior technical and managerial positions and to train local workmen for skilled jobs, thus effectively preventing the development of local skills and experience in the industry—that an important industry thereby remains in a very real sense an “alien” in the local economy.

Plainly, even if there are very few nationals of the producing country in such positions, there is no warrant for assuming a deliberate attempt to harm local development, for if a given firm hired men only on the basis of their ability to do a job, the proportion of local employees in skilled and senior positions would tend to rise only slowly in underdeveloped countries. This is widely recognised, but critics insist that the foreign firms have not gone sufficiently out of their way, and at their own expense, to train local workers and executives. In fact, however, the modern oil company makes serious and costly efforts to develop local skills, partly because it needs such skills and partly for political reasons; there can be little question that this complaint is not well founded to-day, even if it were once valid, for there is strong evidence that the companies do go as far as is practicable in this respect.

The second aspect of the charge has more substance; regardless of the producing country's ability to produce the oil efficiently, the control of international distribution channels by the major oil companies can be used effectively to prevent the country from selling the oil.¹ The major companies not only can refuse to accept oil offered by a producing country but they may also be able to dissuade others from doing so, thus barring markets and forcing oil operations to shut down: the country is unable to run the industry.

Again, however, it is not clear that a concerted international attempt to break down the control of distribution channels by the major companies would, even if successful, benefit the Middle East countries; it is equally possible that any benefit would go primarily to the ultimate consumers of oil and oil products through a subsequent fall in prices. Perhaps consumers are also being “exploited” by the oil companies, and it is possible that the disappearance of this “exploitation” would bring in its wake a substantial fall in the oil companies' profits and a consequent fall in the revenues received by the producing countries whether or not they ran their own industries. Thus the elimination of this source of alleged exploitation might

¹ As noted earlier, it is also alleged that the control of distribution permits the oil companies to set arbitrarily low prices at which Middle East oil is sold as it leaves the producing areas and enters the world distribution system. This, however, is essentially a question of how the profit to be shared should be calculated and becomes part of the profit-sharing agreement. Whether or not the producing countries can alter such conditions in their favour depends on their bargaining power at the conference table.

leave the producing countries with much less than they receive under conditions in which the oil companies can exploit both their suppliers and their customers.

What, then, can we conclude with respect to the problem of exploitation? We have defined it as the use of superior economic or political power by foreign interests to prevent a country from making the most profitable use of its own resources. Clearly if the destruction of monopoly in the position of the oil companies would enable the producing countries to obtain greater revenue from their oil resources, we would be entitled to say that they are at present exploited by the monopolistic power of the companies. But if the facts should be otherwise, and the introduction of a substantial element of competition would raise current costs, or inhibit investment, or lead to wasteful utilisation of the oilfields in the long run, or reduce prices and revenues to such an extent that total revenues to the producing countries became less than at present, we could not say that these countries were exploited *because of the monopolistic organisation of the industry*, for the removal of the monopoly would not permit a more profitable use of its resources by the country.

Nevertheless, the superior economic power of the oil companies arising from their ability to inflict a disastrous economic loss on the producing country does give them a bargaining position which holds down the share of the profits the producing country can obtain in the oil agreement. Thus the producing countries can still complain of exploitation in the sense that their resources are being used to earn large profits which do not accrue to them. There is no reason from the producing country's point of view why the oil companies should earn greater profits than is necessary to induce them to continue investing in the industry and to compensate them for their initial risks. In other words, if excess profits are to be earned, a producing country can complain of exploitation to the extent to which it is unable to obtain them for itself because of the superior bargaining power of the oil company; if it could obtain them, the use of its resources would thereby become more profitable to the country. Whether this is reasonable, that is, whether, from some other point of view, the producing countries *should* properly have such profit is another problem entirely. Part of what appears as profit may really be economic rent attributable to the natural resources, and if the rest is basically due to monopolistic organisation which is considered necessary for the efficient operation of the industry, a good case can be made for the claim of the producing countries. That excess profits, or at least large rents, do exist is plainly evident from the mere fact that the oil companies can afford to hand over large shares to the governments and still find it worth their while to expand their operations and continue their investments. Whether the oil companies are giving up all they could give

up without making operations unprofitable cannot be decided here; it seems improbable.

III. RECONCILING CONFLICTING INTERESTS

The countries consuming Middle East oil want an assured and adequate supply of oil at acceptable prices; the countries supplying it want to obtain the maximum possible revenue from its sale; the companies producing it want a profitable return on their investment. No one wants continuing political conflict, but each wants the maximum protection for its own interests. At present none of these aims can be achieved if the producing countries attempt to take over and operate their oil industries without the co-operation of the existing oil companies; perhaps as the countries develop they may be able in time to handle with reasonable efficiency the extensive scientific, technological and managerial problems of running this enormous industry, and to reach the point at which they themselves can supply their own oil for sale on acceptable terms to the companies controlling the world's distribution systems. That time has not yet come for most countries; perhaps it has nearly come for a few. But nationalisation designed to eliminate foreign control would have consequences adverse to all interests at present; "nationalisation" as a formal act leaving the existing companies in effective control is, of course, always possible (and indeed even likely before very long in some countries) but would solve none of the difficulties.

There are three other possibilities: (1) matters could be left alone in the hope that moderation and good sense will prevail on all sides; (2) the Arabs could attempt to create some form of effectual united front for consultation and perhaps action in dealing with oil companies; and (3) some form of international approach to the problem could be attempted.

(1) Although under present conditions there is danger of unexpected political explosion, actual conflict is not inevitable. It is true that the supply of oil is only uneasily assured as the contending groups attempt to maintain or improve their positions and that no amount of "correct" behaviour on the part of the oil companies, of assurance that their intentions are good and their acts benevolent, nor demonstrations that the results of their operations are beneficial all around, will change the fundamental feelings of the producing countries that these powerful foreign interests are getting more than they should from the oil Allah has provided. On the other hand, although the oil companies must consider their "duty to shareholders" and do not and cannot look upon their activities as simply a "task of trusteeship"¹ for the producing countries, their tendency is to go very far to meet the demands and respect the feelings of the producing countries. They have placed

¹ Stephen H. Longrigg, *op. cit.*, p. 48. Mr. Longrigg, in his able and sympathetic exposition of the history of the oil companies in the Middle East, swings uneasily between these two points of view.

nationals of the countries on their boards of directors, they have agreed to outside arbitration of disputes, and have virtually accepted the principle that existing contracts may be re-negotiated as circumstances change. It seems highly probable that increasingly greater shares of profit will be demanded by the governments. But if both sides have a genuine desire to avoid political conflict, it is possible that these demands will not be pressed too far and that the companies, for their part, will believe that political explosions are too dangerous to be risked, that it is preferable to continue to accept the government's demands so long as it is at all profitable to remain in the Middle East. Thus the various problems may be ironed out as they arise by negotiation between companies and governments as they have been in the past, with grumbles on both sides but few serious disturbances.

(2) Advocates of the political "unification" in some sense of the Arab world argue that concerted action by all oil-producing countries in the Middle East would remove their bargaining weakness and even give them the upper hand.¹ Apart from the very real difficulties in obtaining such unity between countries whose national interests with respect to oil production differ substantially in many respects, this argument has substantial validity in the short run; a single producing country is obviously more vulnerable than the group combined. But even for the group as a whole there is considerable risk in relying on the effectiveness of joint action, for though Middle East oil is low cost, fear on the part of the consuming countries of political instability or of extreme dependence on the whims of Middle Eastern governments would intensify the development, not only of new sources of oil, but also of substitutes for oil. This process has already gone a considerable way, and the producing countries would do well to remember that world supply conditions can change rapidly in times of crisis. Thus, even though concerted action would strengthen the bargaining position of the producing countries, it would not resolve the political tensions nor ensure that either their own economic interests or those of others would be advanced.

(3) In the minds of the people of the Middle East, the Western governments cannot escape responsibility for the position or actions of the oil companies whose relations with the producing countries must inevitably become in some degree entangled with their politics. Many of the difficulties arise because the oil companies are themselves international organisations, their activities transcending national boundaries, their interests and powers of control involving many peoples and governments, yet subject to the effective scrutiny, let alone supervision, of none. It is unrealistic to think that oil relationships with the Middle East can be put on a "business basis" and the question may be fairly asked whether the Western governments should stand

¹ At present the Arab League is promoting conferences and discussions among the Arab countries with a view to obtaining a "unification" of their oil policies. One of the most discussed projects involves the construction of an Arab fleet of tankers which is presumably partly intended to weaken the transportation monopoly of the major companies.

aside and leave their oil relationships with the producing countries solely to such business and commercial interests. Perhaps the more hopeful long-run solution to this type of problem lies in the creation of some form of consultative international body representing all groups, including producing countries, oil companies, the consuming public, the interested as well as "neutral" governments (including perhaps some of the non-producing countries in the Middle East), and well staffed with economic and technical experts empowered to investigate all aspects of the operations of the companies—their costs, their financial interrelationships, the nature and effects of their agreements and operating policies, the way in which their prices are fixed at all levels of operation. Such a body could have many advantages: it could provide a consultative group where disagreements could be discussed and also an independent source of information; it could reduce the feeling of dependence in the producing countries on the goodwill of the oil companies; and it could certainly help to make information more readily available; for at present the serious student of the problem finds important facts missing.

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THE FORMULATION OF DEVELOPMENT PLANS IN THE BRITISH COLONIES

I

ALL British colonies now formulate development plans, allocating capital becoming available to them to different projects. This seems a necessary exercise so long as considerable funds are channelled through public bodies, both on the granting and receiving sides. The type of plan prepared to-day by colonies was initiated by the Colonial Office in 1945, and designed to cover expenditure for the ten-year period 1945–55. Subsequently, plans were prepared for a five-year period, 1955–60.¹ Capital aid from Britain began in 1929, but between 1929 and 1945 plans were not prepared as at present, integrating finance from Britain, local taxation and loans.

The post-war plans have not been a signal success, as indicated by cases of inability of achievement, under-utilisation of completed projects and the necessity of very frequent revisions.

This article¹ discusses the size and composition of the plans of many of the British colonies, in an effort to discover the weaknesses in plan formulation. Under the question of size, there appears no relation between the amount of the total development budget and either British grants which form part of it or any such principle as capacity for development, richness or poverty, of the area. Further, the composition of plans appears seriously at fault in the sense that there is a lack of co-ordination between individual sector plans. Such co-ordination is essential both for testing each sector plan against resources, which is attempted only in a minor way, and for testing against expected future demand, which is attempted still less.

On the problem of size, more connection might be expected between allocation of funds by Britain and the economic characteristics of an area, and between other sources of funds (largely under the control of the Colonial Office) and such characteristics. This might be expected in view of the declared aim of maximum economic expansion of the Colonial Empire.²

When we come to composition, it appears that economic analysis could really come into greater play. For the techniques of input-output analysis, and possibly linear programming, are well designed for the calculation of total repercussions, both on the supply and demand sides, following expansion in each individual sector.³ It is not denied that serious problems need

¹ For helpful comments, I am indebted to Professor Alan Peacock, Mrs. Ursula Hicks, Mr. Basil Yamey and Mr. Parry Selwyn of H.M. Colonial Office; none of these, of course, must be identified with any views in this paper.

² For a government statement to this effect, see Hansard, 1954–55, Vol. 535, p. 554.

³ The "doctrine of balanced growth" (the literature relating to which may be seen in M. Fleming, "External Economies and the Doctrine of Balanced Growth," *ECONOMIC JOURNAL*, Vol. LXV, June 1955), where payments to factors of a "new" enterprise provides demand for
NO. 274.—VOL. LXIX.

to be overcome in the use of these techniques in underdeveloped countries. But what is important is that if they are not faced, so that the total repercussions are not known, the planners are likely to find incomplete achievement of projects occurring through problems on the supply side and/or in incomplete utilisation through inadequate analyses on the demand side.

Section II examines in some detail present practice in the formulation of the plans of British colonial territories, and attempts to substantiate the criticisms made in the preceding paragraphs.¹ Section III suggests ways in which plan formulation might be improved, making greater use of economic ideas and economic analysis. Brief conclusions are contained in Section IV.

II

In the ensuing discussion of development plans of British colonies we shall keep two aspects of the plan distinct: (a) its total size; and (b) the allocation of funds between investment projects within the country. This is useful methodologically, though only partly legitimate in fact. As far as private capital is concerned, the total amount available to a country and its allocation is determined in a single process. Further, the size of funds from granting governments is determined partly by the projects on which the receiving governments intend to spend them. However, for other types of funds, the formulation of the plan can be split into two parts. For instance, loan funds raised on the London market by a colonial government are normally for "general development purposes." Similarly with the funds set aside for development purposes from a country's ordinary revenue; these funds are not earmarked, and the allocation is still to be determined when the total size is already known.

Keeping the distinction for all types of funds for discussion purposes, we may ask how the total size of the plan of a colonial government is determined.

In the case of British colonies it is convenient to consolidate funds from various sources into four categories:² aid from overseas, loan funds (raised at home or overseas), local resources (saving out of revenue) and private

others, could be construed as an implicit, partial, input-output approach: increased demand for inputs from the labour sector produces an expansion in the consumption demand sector, distributed according to income elasticities of the labour group in question. For an explicit application of input-output analysis in a "semi-developed" economy see H. Chenery, P. Clark and V. Cao Pinna, *The Structure and Growth of the Italian Economy* (Rome: U.S. Mutual Security Agency, 1953), and H. Chenery, "Application of Interindustry Analysis to Problems of Economic Development" in *The Structural Interdependence of the Economy*, edited by T. Barna (New York, 1954).

¹ In addition to examination of actual development plans, the author is basing his remarks on experience gained in East Africa on the Tanganyika National Income project, sponsored by the Colonial Economic Research Committee.

² This corresponds with the published statistics in *The Colonial Territories, 1955-56*, App. III, Cmd. 9769.

capital (from home and overseas savings).¹ The first category is met by grants under the Colonial Development and Welfare Acts. Loan funds come largely from loans raised on the London market or are made through the International Bank for Reconstruction and Development. The third category is also public capital; the fourth, of course, is not.

We would stress that development plans are cast in terms only of public capital, a fact which will be important to us later. At the moment, the point we wish to investigate is the distribution between the three sources of public capital in different colonial development plans.

Table I shows the relative importance of the three sources of finance in different development plans (Columns (4), (6) and (8)). It is clear that

TABLE I

*Development Plans in Relation to Sources of Finance
and Gross Domestic Products*

Territory.	Plan- ning period.	Total size of plan (£000).	Size per capita (£ per annum).	G.D.P. per capita (£ per annum).	Proportion of plan met by C. D. & W. funds (%).	C. D. & W. grants per capita (£ per annum).	Proportion of plan met by loan funds (%).	Loan funds per capita (£ per annum).	Proportion of plan met by local re- sources (%).	Local resources per capita (£ per annum).
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Somaliland . . .	1945-56	1,021	0.14	7.5	100	0.14	—	—	—	—
Sierra Leone . . .	1956-59	7,622	1.27	12.5	33	0.41	48	0.61	19	0.25
Tanganyika . . .	1955-60	25,849	0.56	16	17	0.10	55	0.35	18	0.11
Gambia . . .	1955-60	865	0.56	17.5	92	0.53	—	—	8	0.03
Nigeria . . .	1955-60	292,176	1.96	20	6	0.12	31	0.60	63	1.24
Uganda . . .	1955-60	30,000	1.10	22	2	0.02	38	0.42	60	0.66
Zanzibar . . .	1955-59	1,300	1.19	35	30	0.39	—	—	70	0.80
Dominica . . .	1955-60	1,070	3.42	45	85	2.93	15	0.49	—	—
St. Lucia . . .	1955-60	1,280	2.97	45	100	2.97	—	—	—	—
St. Vincent . . .	1955-60	954	2.52	45	62	1.57	38	0.94	—	—
Antigua . . .	1955-60	944	3.65	50	84	3.05	—	—	16	0.60
Montserrat . . .	1955-60	254	3.65	50	92	3.35	8	0.30	—	—
St. Christopher-Nevis Virgin Islands . . .	1955-60	1,445	5.35	50	34	1.85	31	1.65	35	1.85
Fiji . . .	1949-60	13,840	3.75	75	15	0.52	53	2.03	32	1.20
Barbados . . .	1955-60	8,247	7.22	85	9	0.59	58	2.38	33	4.25
North Borneo . . .	1955-60	5,495	3.00	100	25	0.80	48	1.40	27	0.80
British Guiana . . .	1955-60	19,000	7.80	100	23	1.80	64	5.00	13	1.00
Trinidad . . .	1950-56	10,500	2.43	135	9	0.27	54	1.21	37	0.95

Notes

1. Actual amounts expected from each source, corresponding to columns (4), (6) and (8) can be seen in Appendix III of *The Colonial Territories, 1955-56*, Cmd. 9769. The source for gross domestic products for 1955 in most cases is the *Digest of Colonial Statistics*, November-December 1956. Population figures are available in each issue of the *Digest*.

2. Certain colonies have been omitted for one or more of the following reasons: strategic factors operating very strongly; plans under revision or gross domestic products not estimated, both at the time of writing.

3. The rate of expenditure and receipts over the planning period has been assumed constant to achieve comparability with annual gross domestic products.

the differences in emphasis are very significant. In particular, the proportion of funds required that is met by C. D. & W. funds differs widely, colony by colony. What can account for this?

The first explanation is that certain political or strategic motives are

¹ A special temporary source is that created by the running-down of currency fund assets as recommended by, *inter alia*, W. T. Newlyn and D. C. Rowan, *Money and Banking in British Colonial Africa* (Oxford, 1954). Some colonies are, in fact, now running down to something like an 80% cover.

operating strongly in the allocation of aid. This is certainly true, but we have already tried to minimise their effects by excluding from the table colonies particularly affected, *e.g.*, Cyprus, Kenya, Gibraltar, Malta, Aden. This still leaves wide divergencies.

A second hypothesis is that the amount of aid (and also the amount a colony is expected to raise locally) is related to the poverty or richness of an area, or what comes to the same thing, the degree of development already reached. The obvious index here is gross domestic product per head. Columns (3), (5), (7) and (9) in Table I relate the expected receipts from each source of finance and gross domestic product per head.

It is clear that little positive correlation exists between aid given (or the amount from local sources) and gross domestic product per head.¹ Thus grants from Britain do not seem apportioned to current standards of living in colonies either in an absolute sense (C. D. & W. funds per head) or a relative sense (proportion of the plan met by C. D. & W. funds).²

We shall turn now to present practice in respect of the second aspect of development plans, the allocation of total funds among investment possibilities within a country. The main factors which appear to determine distribution in the British colonies are as follows. The first task for the governments involved appears to be to find an appropriate balance between "social" investment (health, education, etc.) and "productive" or "economic" investment. This balance is based on socio-political ideas, varying from colony to colony. Within the "productive" category distribution is based on some kind of economic criteria, qualitative rather than quantitative. The approach often leads to a "one-sector" concentration in "productive" investment. All decisions are influenced, further, by the bargaining strengths of existing interests. We shall try to show these determining factors at work before criticising the procedures.

The concern for balance, particularly between so-called "productive" and "social" investment, often appears to be the dominant consideration. This can be seen in several plans. For instance, in the Fiji plan,³ the dropping of a large hydro-electric project "meant that more than half a million pounds had to be deducted from the provision for economic development projects and the first serious signs of lack of balance appeared." Again "a study of the balance (in 1952) as between the various types of

¹ See A. R. Prest, "The Role of National Income Estimates in the Statistical Policy of Under-developed Countries: A Comment," *Review of Economic Studies*, 1953-54, Vol. XXI (3), No. 56. In arguing for emphasis on national-income estimates, Dr. Prest considered that one of their important uses might be to give a guide to granting governments in the disbursement of funds to colonies. The British Government certainly does not use colonial national-income figures in this way.

² When the colonies in the Table are ranked in descending order of (a) proportion of plan financed by C. D. & W. funds, and (b) *per capita* C. D. & W. grants, the rank correlation coefficients with *per capita* gross domestic product, for what they are worth, are: (a) +0.25; (b) -0.45.

³ *Report of the Economic Review Committee*. Legislative Council Paper No. 12, 1953.

projects showed that . . . the social services had crept up and were out of proportion." In the Tanganyika plan¹ "it is suggested that a fair balance . . . has been achieved." Kenya² provides a nice example of socio-political forces at work, for in attempting to find the "correct ratio of directly productive projects to those which yield a long-term or indirect effect," two distinct views emerged on the Development Committee, each obviously actuated by different political ideas. The majority view considered that whilst minimum social services were desirable, they should not be provided unless the country can bear the heavy capital costs involved (in addition to those involved in making "productive" investments). The Minority Report took the view that the available funds should be devoted to " (1) education of the children, (2) mass education and adult literacy, (3) provision and expansion of health services, and only after these three groups of services have been adequately supplied, to schemes for agriculture and industrial expansion." The Jamaica plan was based largely on the report of the I.B.R.D. mission to Jamaica.³ The mission reported thus on this topic:

"We have included the basic services, particularly education, public health, and housing, in the scope of the Development Programme. Here the primary task was to find an appropriate balance between expenditures in these fields and those more directly related to production. We have recognised that some development of the social services is essential to the improvement of productive efficiency, but at the same time we have been aware of the danger of raising these services to standards which the economy would be unable to support."

With different social and political forces operating in different territories, variations in the balance decided upon between "social" and "productive" investment are not surprising. Table II illustrates these variations in countries for which it has been possible to derive appropriate statistics.

In the decisions on balance within "productive" investment, we should expect to find economic ideas more prevalent. But perhaps balance is an inappropriate word here. For it is characteristic of this part of the plan that extreme concentration is frequently placed on one sector, a procedure which strongly suggests a non-integrated plan.

Transport and communications is a sector often singled out for special development. Thus Sarawak⁴ is devoting 54% of the total funds in the plan to this sector, and only 14% to agriculture and fisheries and fuel and power. The Federation of Rhodesia and Nyasaland⁵ is devoting 40% to this sector and 4% to agriculture.

¹ *The Development Plan, 1955-60*. Capital Works Programme, 1955.

² *The Development Programme, 1954-57*. Sessional Paper, No. 51, 1955.

³ *Report of the I.B.R.D. Mission to Jamaica*, 1952.

⁴ *The Development Plan, 1955-60*. Report by the Development Secretary and Chairman of the Board, 1954.

⁵ *The Development Plan, 1955-59*, presented to the Federal Assembly, June 1955 (cyclostyled).

On the other hand, agriculture is itself singled out by some territories. The Fiji¹ plan considers that "the economy of Fiji must for many years be based on agriculture and that the quickest and surest way of increasing the national income is to be found through the development of agriculture."

TABLE II

Development Expenditure on "Social" and "Productive" Investment

Territory.	"Social" investment (%)	"Productive" investment (%)	Not classified (%)
Sarawak	25	68	7
Trinidad and Tobago	18	76	6
Fiji	25	70	4
Rhodesia and Nyasaland	34	64	2
Kenya	40	40	20
Tanganyika	24	68	8
Uganda	35	47	18

Notes

1. "Social" investment covers medical services, education and housing.
2. "Productive" investment normally includes the development of natural resources and communications.
3. In the "Not classified" category are placed reserve or contingency funds and investment in projects not unequivocally falling under the other two heads. These projects are usually public buildings connected with general administration or internal security. In the case of Rhodesia and Nyasaland, some public building is unavoidably included in "social" investment.

Accordingly, a larger percentage is devoted to the development of national resources than to communications. In the case of Jamaica,¹ the I.B.R.D. Mission wrote: "this Report places emphasis upon Agriculture. In part this is a recognition of the dominant place of agriculture in the economy. . . . In even greater part, however, it reflects the Mission's conviction that the potentialities in this field are far greater than has generally been assumed in the past." In British Guiana,² the I.B.R.D. recommended a greater investment in agriculture than transport, even though the latter had been given special attention.

A third factor mentioned earlier, which influences the allocation between different investment projects, whether "social" or "productive," is the bargaining strength of existing interests, usually in the shape of departments of government or semi-public corporations. Thus if an agriculture department is already strong, a greater proportion than otherwise is likely to be devoted to agriculture, whether or not that sector is relatively under- or over-developed. This assertion is naturally difficult to document, but we do get a slight glimpse behind the scenes in the Uganda³ plan: "The detailed phasing of the plan was not concluded without difficulty, in the

¹ *Op. cit.*

² *Report of the I.B.R.D. Mission to British Guiana, 1953.*

³ *A Five-year Capital Development Plan, 1955-60, 1954.*

face of the understandable wish of all Secretaries and Heads of Departments to have their capital works executed early rather than late."

How can the plan have been devised as an inter-dependent whole, when, as in the Sarawak¹ 1955-60 plan, "in several cases, detailed plans have yet to be prepared," though for other sectors the plans are complete? Again, the Tanganyika¹ 1955-60 plan "does not purport to cover all Government's activities in the direction of 'development.'" In particular, it does not expose in any detail that part of Government's policy which is directed to the encouragement and assistance of local development plans of the kind that are formulated and carried out by Town or County Councils or by Local authorities." It is true that the report recognises the need "to dovetail these plans into the Central Government's plans both in respect of natural resources and communications, in such a way as will derive a maximum advantage from the complementary application of local and central resources of finance and manpower."² But the sector estimates have been drawn up before this has been done, so there may be a much greater total call on some sector than has been allowed for.

Some of the I.B.R.D. Missions were certainly aware of the need to take account of inter-dependence between sectors in formulating the plan. But this was done qualitatively rather than quantitatively. For the latter approach, statistical knowledge of input-output relations is required which was not available to them. But further, their approach appears to have been partial, in the sense that only some sector inter-dependencies were taken into account, and further, only primary repercussions were allowed for. If building and electricity requires skilled workers, training skilled workers requires, in its turn, building and electricity.

These criticisms may be substantiated by the following extracts. In British Guiana,³ "the proposed transport programme is closely tied to agricultural development. . . . The forestry programme aims to increase the production of timber products . . . for building materials . . . in housing and transport. . . . The social sector provides for an expansion . . . to meet the growing needs of the economy for skilled labour." But these inter-dependencies are not taken into account quantitatively. Thus "in order that the transport and communications system may serve adequately and economically a growing and developing economy, the mission recommends an investment in this sector second only to agriculture" and "Because transport and communications have been relatively neglected in the past, the recommended investment programme in this sector is second only to agriculture." Whilst there is some recognition of the inter-dependence of the transport sector with others on its output side, there

¹ *Op. cit.*

² But no dovetailing with the development plans of the East African Railways and Harbours Administration or East African Posts and Telecommunications Administration is envisaged.

³ *Op. cit.*

is not on its inputs side. Nor is there any reference to secondary and tertiary repercussions.

Similar considerations apply to the Jamaica¹ report. In the case of British Honduras,² whilst the quality of manpower is recognised as a main factor limiting progress, to be repaired through education, it is strange to find only 6% of the plan devoted to education for 1952-60, surely a figure not determined on inter-dependence grounds.

The Nigeria I.B.R.D. Mission³ report comes nearest the desirable procedure. "The programme is an integrated one: the component parts complement and support each other. Development in one sector affects, and is in turn affected by, developments in others." This admirable statement of principle does not appear to have been carried quantitatively into the recommendations. Expansion in every sector is dependent on skilled man-power, the Mission considers. But we do not find their education expansion to be based on series of labour-output coefficients in conjunction with the proposed expansions in other sectors. Instead, the education expansion appears to have been decided on rather general grounds: "The pace of educational advance and the formulation of a financial programme for the period ahead are affected by a variety of factors, including popular interest in education, financial resources, the extent of organisational and managerial ability, and most important, the number of trained teachers available." There is no mention of the quantitative need by administrative services, transport, posts and telegraphs, electricity and water works, mechanised agriculture, etc., for skilled and semi-skilled people. Still, the approach of the Nigeria Mission is a considerable advance on the plans of some other territories.

The conclusions which we think it fair to draw from this discussion of present practice in relation to the formulation of development plans are as follows:

(i) The allocation of funds by the British Government does not follow any consistent principle, neither economic return nor relative overall poverty nor the anticipated need for social services, etc. A multiplicity of factors appears to be working, which probably includes past performance, extent of proved but undeveloped natural resources, strength of pressure groups, etc.

(ii) The allocation by receiving governments between investment possibilities is determined more by socio-political ideas and bargaining strengths than economic criteria, often leading to an "all-or-nothing" approach to sector allocation. This does not seem consistent with the usual aim, declared or implicit, of maximum expansion of real income.

¹ *Op. cit.*

² *Report on British Honduras*, prepared for the I.B.R.D. by D. L. Gordon, 1954.

³ *Report of the I.B.R.D. Mission to Nigeria*, 1954.

(iii) There is no integration of the plan in order to allow for the repercussions and counter-repercussions in the many sectors following an expansion in one sector. Nor is there any testing against resources, a procedure for which this integration is necessary.

III

It is suggested that economic analysis could be used to give a more consistent and rational approach in the formulation of plans. Improvements could come along the following lines: allocation by the granting government to receiving governments more according to principle, allocation by receiving governments between competing investment possibilities giving more weight to economic criteria, and the fuller recognition of the inter-dependence of the economy in formulating an integrated plan.

The first point, allocation by the granting government, is of rather a different order from the other two. Some assistance towards solving the latter two problems might be obtained from input-output analysis, and this is taken up after a discussion of the first question.

We desire to seek some principle which should govern a granting government's disbursements to underdeveloped countries. The decision is bedevilled by strategic and political considerations, but ignoring those cases where these will be overwhelmingly important, we might consider two procedures which would make the process more meaningful. A straightforward allocation to achieve maximum return would involve a ranking by the Colonial Office of all possible investment projects in all colonies in terms of some criterion of economic return. Those projects would then be financed down to a certain level of return (and each to the extent that it gave that return) until the (fixed) Exchequer grant were exhausted; similarly with the allocation of permission to proceed with the raising of loans. With the addition of that proportion of its ordinary revenue that a colony thought it could put aside for development, the total size of the development plan would be determined, as would the broad lines of distribution within the country.

A major drawback of this method is that there would occur little "social" investment, which is characterised by long-term, indirect benefit rather than an early pecuniary return.

Our second suggested method takes care of this difficulty. It is proposed that C. D. & W. funds, being the sole source where return is not necessarily important, be used to finance the "social" part of the plan. The allocation could be made according to some rough idea of common standards of social security in the colonies. Some allowance could be made for countries in different stages of development, but some such common principle would be more meaningful than the extremely variable proportions of funds at present devoted to social services in various colonies, proportions decided by personalities and pressure groups within the territories. This

method would then bring together the "economic" investment and the "economic" sources. For productive investment could be met only by loans on which interest had to be paid, or home finance, for which sacrifices in consumption had to be made.

We shall proceed on the assumption that minimum social services have been met by interest-free grants. The government of a colonial country is then left with the task of making the "economic" part of the development plan as great as possible, allocating it between projects, and seeing that it is integrated to take account of inter-dependence and that it is feasible in terms of resources. We shall examine each of these three stages in the formulation of the plan.

Prior to that, however, we should state that the task as a whole can be satisfactorily performed only if the plan covers not only public investment but private investment as well.¹ Such an extension is necessary for a number of reasons. For one, it is economically desirable to avoid duplication in the sense that both public and private investors see an opportunity as high-yielding with the result of over-capacity in that sector. Another reason is that full integration and testing against resources is obviously only possible if all investment is taken into account; otherwise, for instance, the total increased demand on some supplying sector as a result of public plus private investments in others may not be properly foreseen by either the public or private investor separately. This extension of the plan does not imply control of private investment in any sense.

The first task for the government of an underdeveloped country is to decide on the total size of the plan. Earlier on, we listed the sources of finance for such a country. We are assuming that gifts from other countries are used up for the provision of social services. In most colonial underdeveloped countries, domestic private saving² and domestic public loans are not likely to be very important. Loans raised abroad are, as far as those raised on the London market are concerned, fairly closely regulated by H.M. Colonial Office and H.M. Treasury.³ It appears that the amounts obtainable from several sources are virtually fixed: the government has an important degree of freedom only in maximising private capital flowing from abroad and determining the amount of ordinary public revenue to be devoted to investment.

¹ This does not occur in any of the plans under consideration. For example, though the I.B.R.D. Mission to Malaya (Report, 1955) declared, "Essentially, our recommendations for government action and investment are for the fostering and assistance of private development," there is no real attempt to link development with what is specifically expected in the private sphere.

² Marketing Board surpluses are being considered a direct tax on producers rather than "voluntary" saving by producers.

³ H. H. King's view in "Sterling Balances and the Colonial Monetary System," *Economic Journal*, Vol. LXV, December 1955, that a colony can borrow any reasonable amount, given "a sound government borrowing programme and development plan," does not take account of the general surveillance over both colonial borrowing and development plans that is exercised by the Colonial Office and the Treasury.

The weapons for maximising inflows of private foreign capital include the granting of tax concessions, subsidies and monopoly rights, the provision of certain services and the carrying on of propaganda abroad.

The exercise of these weapons provides a task for economic analysis: to estimate reactions of various types of investors to various weapons. It also presents the first of several forecasting problems which a country has to attempt to overcome in preparing its development plan.

The size of the proportion of national income that underdeveloped countries might save has been extensively discussed.¹ We do not intend to enter into it here, but only to put the matter in the context of the whole problem.

So much for the total size of the plan. The other two stages in the formulation of the plan are allocation between projects and overall integration. It is intended to discuss these only in a general way in the present paper.² The two processes may be separated. Capital may be apportioned between sectors according to capital-output ratios, the social productivity of capital or more complex schemes.³ This does not necessarily carry with it an assurance of integration; some modification may have to be made to the criterion to obtain consistency with limits on resources on factors other than capital, and to take account of demand patterns of final use.

We can remain agnostic about the use of the various criteria put forward, and still plead for integration through input-output analysis, so that projects are completed and utilised, however chosen.

Assuming this position, we can briefly mention the practical economic tasks which need to be undertaken. Input-output tables for the economy are required, and some forecasting on the demand side is likely to be desirable.⁴

We have stressed the need for the development plan to take account of private as well as public investment. The input-output approach naturally does so, for all sectors of the economy are included in the input-output tables. It will be necessary to form some estimate of the total and the direction of private capital becoming available (usually an inflow from abroad). Then the plan can be integrated in an economy-wide sense.

¹ For instance, in the work of Simon Kuznets, Colin Clark and W. A. Lewis.

² For a technical discussion see Chenery and Kretschmer, "Resource Allocation for Economic Development," *Econometrica*, October 1956, and Ghosh, "Input-Output Analysis, Allocation, and Development Planning," *Economica*, February 1958.

³ This aspect of investment criteria has been the subject of long-standing discussion by J. J. Polak, A. E. Kahn, H. B. Chenery, W. Galenson, H. Leibenstein and O. Eckstein in the *Quarterly Journal of Economics*.

⁴ These statistical aspects of plan formulation are dealt with in detail in a forthcoming article. In the case of demand it might be mentioned that there are distinct possibilities of forecasting home income-elasticities. Export demand is a complication, but short-term forecasting is not out of the question; plans for the export sectors must, perhaps, be kept flexible and short-term. Those who try to escape these problems, e.g., Ghosh, *op. cit.*, by assuming a "centrally-determined social welfare function" would seem to be in great difficulties, especially in respect of export demand.

IV

This article has been critical of present practice in the distribution of capital funds to the colonies by the British Government, and the allocation of the capital available between competing investment possibilities by Colonial Governments. The general aim we have assumed, namely rapid increase in real income, seems to conform to reality. In this light, it has been suggested that present practices do not achieve the best result possible.

It has been tentatively suggested that C. D. & W. funds could be used to finance "social" investment in colonies, whilst, more definitely, we have suggested that plan formulation within a colony could benefit from a fuller application of economic analysis; the sector plans could be more integrated (covering private investment as well) using input-output analysis, so that development planned in any one sector is feasible in terms of supplies (existent or planned) from other sectors, including labour, and realistic in terms of expected demand patterns.

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A SAVINGS AND INVESTMENT APPROACH TO DEVALUATION¹

1. THE object of this paper is not to say anything new, but to present in a compact and intelligible form, without the technicalities which are readily available elsewhere, what is to the best of the author's understanding the present state of the analysis of the effects of devaluation. The theory of the balance of trade² usually starts with an account of the relation between changes in a country's imports and exports and changes in the levels of income and employment at home and abroad. This type of analysis is known in economic jargon as "the foreign-trade multiplier." This analysis employs a set of concepts such as income and investment which are familiar to anybody who has studied national income and employment theory.

2. The analysis then goes on to a second method of adjustment in the economic relations between countries, that which considers the effects on the balance of trade of changes in relative price and wage levels in two countries. The simplest form that such relative price changes can take is that where each country's prices in terms of its own currency remain constant, while one country's currency is devalued, *i.e.*, there is a reduction in its price in terms of units of the currencies of the countries making up the rest of the world.³ This analysis is also readily comprehensible, since it employs concepts of the elasticities of supply and demand familiar from the theory of market equilibrium. Usually, however, the complexities of a taxonomic discussion of the possible results of devaluation under various conditions of supply and demand elasticities hold the forefront of the analysis. Some perfunctory reference is often made to the multiplier effects of any change in the balance of trade brought about by the operation of the various elasticities, but the changes in savings and investment involved are rarely accorded any major importance.

3. It is almost invariably apparent, when pupils who appear to have mastered both the types of analysis above are questioned about the effects of devaluation by a country suffering from internal inflation, that the importance of the national-income accounting aspects of devaluation has

¹ The author is indebted for comments and suggestions to Professor J. Jewkes and Mr. K. M. Savosnick, who are not, however, responsible for any of the views expressed.

² In what follows it is assumed for simplicity of exposition that there are no capital movements and no international payments of property incomes, so that "balance of trade" is used below as a synonym for "balance of payments on current account," and a surplus or deficit on current account is assumed to lead to an equal rise or fall in the size of a country's foreign-exchange reserves. Where there are capital flows, appropriate adjustments must be made to the text, but the basic analysis of the effects of devaluation on the balance of trade is left unaltered.

³ The classical exposition of this type of analysis is that given by Mrs. J. Robinson, "The Foreign Exchanges," *Essays in the Theory of Employment*, pp. 134-55.

not really been grasped. Intellectually it is unsatisfactory that pupils are prepared first to accept an account of the balance of trade as being determined by savings and investment, and then to switch to an analytical scheme under which imports and exports are determined as the result of the interplay of supply and demand in particular markets, with no reference at all to savings and investment. This must imply either that they are not taking the subject seriously or that they can sense the difficulty but are baffled in seeking from the standard references on the subject any clues as to how the difficulty should be stated, let alone resolved.¹ It is also disquieting to think that teachers of economics are losing an opportunity to help pupils to base their future opinions about the advisability of devaluation as a means of public policy on a real grasp of the mechanism involved.

4. The starting-point for a sensible account of how devaluation is supposed to work must be national-income theory. Starting from first principles, in the world as a whole income and expenditure must be equal. In any one country, however, some incomes may be derived from the sale of goods² to foreigners, and some expenditure may be devoted to the purchase of foreign goods. For each country taken separately, imports and exports need not be equal. However, in each country incomes must be derived from the sale of goods either to home consumers or to foreigners or for use in home investment; while incomes must either be expended on the purchase of consumer goods from home producers or from foreigners or it must be saved. Thus, cancelling out sales and purchases of goods from home producers by home consumers, which are identically equal,

$$\text{Exports} + (\text{home}) \text{ Investment} = \text{Imports} + \text{Savings}$$

or, re-arranging the terms,

$$\text{Exports} - \text{Imports} = \text{Savings} - \text{Investment}$$

i.e., a surplus of exports over imports (a positive or favourable balance of trade) must imply an equal excess of savings over home investment.

5. This means that any measure of public policy which is intended to improve the balance of trade, by increasing the excess of exports over imports, or by decreasing the excess of imports over exports, must necessarily and at the same time involve an increase in savings relative to home investment.³ Any method which does affect the relation between savings and

¹ A good source for clues about the nature of the difficulties is "The Inappropriateness of Simple 'Elasticity' Concepts in the Theory of International Trade," by T. Balogh and P. P. Streeten, in the *Bulletin of the Oxford University Institute of Statistics*, Vol. 13, Nos. 3 and 4, April 1951. This, however, does not provide any coherent answer to the problems it raises.

² In what follows, "goods" includes services.

³ The best treatment known to the author along these lines, and one which has provided him with the major part of the inspiration for his own thoughts on the subject, is an article by Alexander, "The Effects of a Devaluation on a Trade Balance," *I.M.F. Staff Papers*, April 1952. This is rather too complex for use as an elementary treatment, however, and it does not distinguish clearly

investment must affect the balance of trade, and any method which fails to affect the relation between savings and investment must *ipso facto* fail to improve the balance of trade. This is equally true whether the method concerned is fiscal policy, monetary policy, devaluation, tariffs and subsidies, or direct controls on foreign trade. Furthermore, these conditions must be satisfied in the rest of the world as well as in the country in which the devaluation or other policy measures are applied.

6. If devaluation is to be considered in this light, how and under what conditions can it be expected to succeed in bringing about an increase in savings relative to home investment? Obviously a decrease in investment is unlikely to result from devaluation; if the devaluation brings about an improvement in the balance of payments, and the multiplier effects of this involve an increase in the size of the national income, this may be expected, if anything, to tend to increase the apparent profitability and therefore the quantity of investment. Thus the most favourable plausible hypothesis about the effect of devaluation on home investment is that it remains unchanged in real terms.

7. How, then, we are left to ask, does devaluation tend to bring about an increase in savings? The most important determinant of savings, both as suggested by Keynesian analysis and by statistical observation, is the level of the national income. Thus the most favourable circumstances for an improvement in the balance of trade following devaluation are those in which an increase in the level of the national income, in real terms, can most easily be brought about, *i.e.*, a position of general Keynesian unemployment. This surely helps to account for the rise of theories concerning the advisability of the use of devaluation as a means of international adjustment during the 1930s, and the succeeding years, in which the thirties were still the standard by which people formed their concepts of a normal state of affairs.

8. Under conditions in which an increase in output and employment, and therefore in income and savings, can readily be brought about by the application of more effective demand, there are two ways of providing the increase in effective demand. One is to increase "autonomous expenditures," *i.e.*, home investment or government spending, in the home country, or to decrease its average propensity to save by fiscal means, such as income redistribution in favour of low-savers. The other method is to divert away from foreign and on to home resources the multiplier effects of the existing total of autonomous expenditures at home and abroad. This increases home employment, income and savings, but at the same time it decreases them abroad, which accounts for the appellation "beggar-my-neighbour

enough between the comparative statics effects of an established alteration in the exchange rate and the once-for-all effects of the change itself, *e.g.*, on the demand for cash balances when prices change during the process of adaptation to the new equilibrium situation.

remedies" given to competitive devaluation in Mrs. J. Robinson's classical analysis of devaluation.

9. While devaluation when used alone in conditions of general under-full employment may fairly be described as a beggar-my-neighbour policy, it is not necessarily fair to make this criticism if it is used in conjunction with measures to expand the level of autonomous expenditure, or to reduce the propensity to save; for these other measures, taken alone by any one country, tend to worsen its balance of trade. Thus a country may be faced with the choice between internal expansion of autonomous expenditures *plus* a sufficient degree of devaluation to divert enough effective demand back to its own resources to keep its balance of trade satisfactory; or doing neither and remaining depressed. As this latter course would be no better for other countries than the first alternative, but worse for the country itself, the others as a whole¹ have no reasonable grounds for complaint if one country chooses to expand its own economy, but to devalue so as to keep the benefits to itself.

10. Under conditions of full employment, however, whether of labour or other resources, a country's savings cannot hope to benefit as the result of devaluation in the same straightforward way. If there is no tendency for savings to rise, but foreigners attempt to buy more exports, and home consumers attempt to buy more home-produced substitutes for imports, then the effects can only be inflationary, and are liable to cause increases in home prices and costs which wipe out the initial price-advantage given by devaluation. In fact, as the process of inflation, once started, tends to be cumulative, the result of an unsuccessful attempt to cure a country's payments problem by devaluation may actually be to lead to a final position with a higher price level relative to the rest of the world than it had before devaluation.

11. Thus devaluation in circumstances of full employment can only be expected to succeed in improving the balance of trade if it somehow entails a rise in the propensity to save of the devaluing country. There are some ways in which this might take place; if the inflationary process which follows devaluation involves a shift in real incomes from classes of the community with low savings propensities to others with high ones, *e.g.*, from pensioners to entrepreneurs, then savings could rise. A second possibility arises if, when demand increases, sellers allow waiting lists for their products to form or lengthen instead of, or in addition to, raising prices. The amounts of savings and home investment actually carried out, as distinct from planned, may then be affected by the degree of priority given to export

¹ However, the effects of devaluation plus an increase in the level of autonomous expenditure in one country may be that the devaluation raises exports to and cuts imports from one group of foreign countries, while the expansion of imports due to increased incomes mainly benefits the exporters of a second group. Thus the process as a whole may give rise to a need for adjustments abroad, and may possibly lead to retaliation from those foreign countries whose balances of payments suffer in the process.

orders in the administration of the queues. Home investment may be cut via a diversion of investment goods to export markets, or savings increased via a diversion of consumer goods to foreign purchasers.

12. It must be remembered, however, that analogous causes could lead to opposite effects on savings and investment in quite plausible cases, *e.g.*, where inflation involves shifts in real income from high-saving landlords to low-saving tenants via fixed rents, or where firms faced with long waiting-lists prefer to give priority to home customers because of greater risks in export markets. In any case the amount of benefit to the balance of trade likely to result from shifts in the savings propensity enforced by inflation does not appear likely to be large enough to justify the social consequences of inflation. Thus it is a fairly adequate generalisation to say that in circumstances of full employment devaluation used in isolation from other means of public policy is likely to be ineffective; this conclusion does not depend on the detailed assumptions made about price-elasticities of supply and demand in import and export markets.

13. It must not be concluded, however, that under conditions of full employment devaluation cannot effectively be used as part of a co-ordinated policy, involving fiscal and other means of influencing the level of effective demand as well as devaluation itself. The place of devaluation in such a co-ordinated policy is best seen by taking the case of a country which has just sufficient effective demand to keep its resources fully employed, but not more, and considering the effects of a cut in the level of effective demand on the level of incomes and on the balance of trade. Here the orthodox theory of the foreign-trade multiplier tells us that, neglecting repercussions in the other country's incomes and demand for exports, a cut in investment in the home country will lead to a rather larger cut in absolute terms, in incomes, but to a rather smaller cut in imports. Where the marginal propensity to import is M , and the marginal propensity to save is S , a cut of 1 unit of investment will involve a fall in incomes by $\frac{1}{M+S}$ units, but of only $\frac{M}{M+S}$ units of imports. This means that cuts in the level of autonomous expenditure are always an effective means of improving the balance of trade, but that except in circumstances of inflationary pressure, where excess demand tends to be heavily concentrated on imports so that M is abnormally high, it is a very expensive means of improving the balance of trade in terms of the $\frac{1}{M}$ units of incomes forgone for each unit by which expenditure on imports is reduced.

14. Thus in the case of a country which is just fully employed we find that whereas devaluation alone is ineffective as a means of improving the balance of payments, deflation of the level of effective demand is effective, but very expensive in its economic and social costs. A suitable combination of the two policies, however, will have the effect of reducing the total level

of effective demand at home with beneficial effects on the balance of trade, while shifting what demand there is, whether at home or abroad, towards home resources. This has a further favourable effect on the balance of trade, while its effects on the home level of employment exactly offset those of the cut in home demand, so that full employment remains unimpaired.

15. Such a combination of policies may in practice prove to be rather difficult to attain, both because of defects in the information available at any time about the present circumstances of trade and employment, and the varying time-lags involved in making the various policy-measures effective. This, too, is not the place to discuss whether the effects on speculation of a known preparedness to devalue may not be so great as to render devaluation unusable as a means of policy. It must be remembered, too, that devaluation has been used only as the simplest case of the many possible combinations of rises and falls in the domestic price levels of different countries, and of changes in the par values of their currencies, which combine to give rise to relative price changes.

16. The reader may by this time be wondering what has become of the elaborate study of the implications of various elasticities of demand in the different countries, which usually forms the bulk of any analysis of devaluation. Consideration of the elasticities is still necessary, to see whether the "stability conditions" are satisfied, and to estimate how effective devaluation is likely to be in diverting demand on to the devaluing country's own goods, and away from those of others. If the elasticity of demand for imports in the devaluing country is low, this implies that a rise in the price of imports may actually lead to a higher proportion of incomes being spent on them when their prices rise as the result of devaluation. Similarly, a low foreign elasticity of demand for exports may result in the proportion of foreign incomes spent on exports actually falling when their price falls. If these conditions prevail then devaluation will not result in any net shift of effective demand from foreign on to home resources, and the expansion of its national income needed to enable the devaluing country to save more will not take place, even though conditions are otherwise favourable. If devaluation does work the elasticities are relevant in showing how great the effects which will take place in peoples' spending both at home and abroad as the result of any given devaluation. The effects of devaluation on the balance of trade, however, depend on the "income effects" of changes in the levels of income and employment in the home and foreign countries, and no analysis of the subject can afford to treat the income effects as a mere afterthought, since it is the income changes which provide the means and motives for changes in savings.

17. There is, however, a second set of conditions for the success of devaluation which must also be considered; this is the question of the response of the rest of the world. For one country to be able to improve its balance

of trade by means of devaluation, it is necessary for that of some foreign country to be worsened. Therefore to see if any one country is in fact likely to be able to improve its balance of trade, the probable response of the other country or countries must be examined. Normally the other country¹ will have a positive marginal propensity to save, s , and also one to import, m . A unit change in the foreign country's exports will thus lead to a change in foreign incomes of $\frac{1}{m+s}$ and in the foreign demand for imports, *i.e.*, for the home country's exports of $\frac{m}{m+s}$ (neglecting the effects of further, induced, changes in the home country's incomes and thus in its demand for the foreign country's exports). The foreign country will thus have a "reflection ratio" of $\frac{m}{m+s}$, which is less than 1, and will tend to "absorb" via changes in its savings $\frac{s}{m+s}$ of the effects of any change in the demand for its exports.

18. If foreign countries are themselves suffering from balance-of-payments difficulties, however, this may be an unrealistic way of regarding their conduct. If the rest of the world is suffering from a tendency to chronic shortage of the devaluing country's currency it may rather tend to adjust its imports so as to keep them equal to its exports, and to prevent its reserves from varying. If this is the way the rest of the world is behaving, then it will not prove possible for one country to produce an improvement in its balance of trade by devaluation or any other means, since if these are temporarily effective, some form of retaliation will at once be adopted by the deficit countries to counteract the effects of the balance of trade. It does not matter for this purpose whether the deficit countries choose to retaliate with measures of the same sort as those being used by the surplus country to produce its surplus; competitive devaluation may be adopted for one country's devaluation may be countered by tariffs, direct controls or domestic deflation in the deficit countries. Whichever of these means is chosen, the devaluation will not be successful. It is only possible to acquire another country's exchange reserves by running a positive balance of trade with it if the other country both starts off with some reserves in hand and is prepared to allow them to be reduced.

19. But with the two provisos in mind, *i.e.*, that the elasticities must be sufficiently large for devaluation to cause a net diversion of effective demand in the world as a whole towards the goods of the devaluing country and that the other countries are able and willing to allow their balance of trade to worsen, it is suggested that the analysis of the effects of devaluation

¹ We consider the case where there is only one foreign country here for the sake of keeping the algebra simple. An analogous, though rather more complicated result, is obtained if it is assumed that there are several other countries.

is best conducted by treating it as the analysis of the effects on national incomes and foreign trade of a change in the proportion of income in each country spent on imports, along the lines of the foreign-trade multiplier; the price changes involved in devaluation being regarded simply as the means used to induce the changes in the direction of home and foreign expenditure.

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ASSET REVALUATION AND TERMS-OF-TRADE EFFECTS IN THE FRAMEWORK OF THE NATIONAL ACCOUNTS

THE systems of national accounts in current use do not provide scope for the recording of asset revaluation and capital gains and losses, nor for the recording of terms-of-trade effects in the widest sense. The present paper is concerned with the changes in accounting design that are required to accommodate these basic aggregates in the framework of the national accounts. In the context of terms-of-trade effects it furthermore deals with the deflation of non-commodity flows.

THE EXISTING ACCOUNTING FRAMEWORK

Although the design of the system of national accounts in current use varies greatly from country to country, it proves possible to describe the common characteristics of these various systems starting from a very simple accounting model. The basic types of account that form part of this model are three in number: a production account, an income account and a capital account. On a very aggregative level where only two sectors are distinguished in the world economy, viz., (a) the national economy, and (b) the rest of the world, this accounting set-up may be symbolised as follows:¹

(a) National Economy—

$$\text{production account: } Y + M = C + K + E$$

$$\text{income account: } C + S = Y + T$$

$$\text{capital account: } K + U = S$$

(b) Rest of the World—

$$\text{consolidated account: } E + T = M + U$$

where Y = factor income; T = net transfers from abroad;
 C = consumption; S = net saving;
 K = capital formation; U = net lending to abroad;
 E = exports; M = imports.

For the rest-of-the-world sector the three basic types of account are consolidated into one account, partly because the internal transactions in the rest of the world are not directly relevant to a description of the economic process of the national economy to which the accounts refer and partly because

¹ In order to avoid unnecessary complication of the argument no distinction is made here between national and domestic concepts (see my article on "A System of National and Domestic Accounts," *Economica*, August 1955). Furthermore, capital consumption is not explicitly introduced into the system (K , Y and S are defined net of capital consumption). Finally, transfers, saving and lending are entered on a net basis, with all transfers being classified as current.

information on these transactions is not directly available to the national statistician, even if it is available at all.

Once sectors are distinguished within the national economy domestic inter-sector deliveries of intermediate products (as distinct from imports and exports) will appear on the sector production accounts in addition to the items already recorded on the purely national production account. Likewise, domestic inter-sector transfers (as distinct from international transfers) will appear on the sector income accounts, and domestic inter-sector credit transactions (as distinct from foreign lending) will appear on the sector capital accounts. In general, the debit and credit entries in respect of each of these types of transactions will not be equal to each other sector by sector, but for the national economy as a whole they will be equal and thus cancel out in the process of consolidating sector accounts.

The system as presented here is fully articulated, *i.e.*, each item is recorded twice, once on the debit side of one account and once on the credit side of an other account. The system as such is therefore complete in itself. This, however, does not mean that it is also all-embracing. The most fundamental way in which it lacks this property is discussed in the next section. A less fundamental aspect of this failure of our accounting model to be all-embracing can, however, be dealt with more conveniently at this stage of the argument.

If income account and capital account were consolidated into what might be called a financing account, the new system would still be fully articulated and complete in itself, but it would no longer allow for the recording of saving. Moreover, the accounting distinction between consumption and capital formation, which consists in the fact that, while both are credited to the production account, consumption is debited to the income account and capital formation to the capital account, would disappear. Similarly, a consolidation of production account and income account into one current account would eliminate the possibility of recording factor income and consumption in the system and would also eliminate the accounting distinction between exports and net transfers from abroad.

The same argument applies, of course, in the opposite direction if the number of basic types of account is increased through deconsolidation of one or more of the accounts in our model. The effect of such an operation would be to introduce into the system new basic aggregates which previously had no place in it and to make accounting distinctions between basic aggregates which, although different in nature, previously followed the same accounting rules (compare the examples given above with respect to consumption and capital formation, and with respect to exports and net transfers from abroad).

Before deconsolidation the only way of distinguishing between aggregates following the same accounting rules is to record them separately on the accounts concerned. After deconsolidation the accounting structure itself forces them to be recorded separately.

Apart from increasing the number of accounts carried in the system, the effect of a deconsolidation of accounts by sector is entirely different from a deconsolidation by type of account. Sectoring does not cause any new basic aggregates to be accommodated in the system, but only forces certain flows, which were already recorded in the system on a more aggregative level to be shown in more detail. Again, of course, this sector-detail could have been shown before deconsolidation by means of separate recording of the component elements of the aggregates concerned (*e.g.*, private and public consumption), but after deconsolidation they have to be recorded separately.¹

With respect to such basic aggregates as deliveries of intermediate products (a category to which also exports and imports belong), transfers and lending, it may well be argued that they show up in the system presented here only by virtue of the fact that this system is a model distinguishing two sectors (the national economy and the rest of the world), but that in the purely national accounts for a closed economy (such as the world as a whole) no such flows would be recorded. This is, of course, true, but it does not constitute an exception to the rule that sectoring does not result in new basic aggregates being accommodated in the system. The common accounting feature of all these transactions is that the debit and credit entries by which they get recorded in the system are for each of them made on (sector) accounts of the same type (the rest-of-the-world account in our model being a consolidation of the three basic types of account for the outside world). Consequently, their net value for a closed economy is by definition zero, and it is only because of this that they are not shown explicitly in the accounts of such an economy if no distinction in sectors is made. In any system, no matter how condensed it is, there is, however, in principle a place reserved for the recording of these transactions, and once the particular type of account on which they should be recorded is distinguished separately in the system their accounting position is also uniquely defined.

All the systems of accounts in current use are in one way or another related to the model described above. For some, but not many, of them it may be necessary in the first instance to deconsolidate one of the basic types of account of our model, but in general all that is required is to first sector the accounts of this model and introduce breakdowns of aggregates and then to consolidate the accounts thus obtained either by sector or by type of account or by a combination of both in order to establish the link.

ASSET REVALUATION AND CAPITAL GAINS

In the system of national accounts described above there is at least one account, namely the capital account, that stands in need of further develop-

¹ *Inter alia*, the same device of separate recording can also be applied to show other types of breakdowns of certain aggregates, *e.g.*, the breakdown of capital formation between fixed assets and inventories and of the former between various types of construction and equipment, and the breakdown of transfers into various types of taxes and other transfers.

ment if it is to perform the function it should perform in principle. Ideally this account should provide a link between the national balance sheets at the beginning and at the end of the accounting period. However, in the absence of national balance sheets in the existing accounting framework, the capital account has been allowed to become the closing account of the system, that is to say the account on which the counter-entries are made for all those items that do not have a counter-entry elsewhere in the system. Consequently any item that has been omitted from the rest of the system will automatically also be omitted from this account.

Matters would have been different if the national accounts had been developed along the same lines as business accounts. The latter start off with, and end up in, a balance-sheet statement, and it can fairly be claimed that all the accounts in a system of business accounts are principally directed towards the purpose of linking the opening and the closing balance sheet of each accounting period.

Once national balance sheets are introduced into the framework of national accounts—a development which is bound to come sooner or later—the capital account should come into its own. By then, it will no longer suffice to record on this type of account (on the national level) only capital formation and saving (either gross or net of capital consumption), for these are not the only factors responsible for the changes in the value of the stock of real assets and the corresponding liabilities (*i.e.*, ownership claims) between two balance-sheet dates.

Even if capital formation over a given accounting period were zero, the value, but not the volume, of real assets at the end of the period might still differ from that at the beginning of the period owing to changes in the prices of such assets. Moreover, even in the absence of price changes, capital formation equal to zero does not necessarily mean that the volume of the total stock of real assets, and thereby its value, remains unchanged from the beginning to the end of the accounting period. This is due to the fact that capital consumption of fixed assets is computed on the basis of normal rates of wear and tear, obsolescence and accidental damage.¹ Any deviations from these normal rates during the accounting period will not affect the measure of capital formation, which is defined as the difference between additions to stock and capital consumption, but they will affect the actual volume of the total stock of real assets. These two types of value changes not accounted for in capital formation (*viz.*, value changes due to price changes, and value changes due to deviations from normal rates of wear and tear, obsolescence and accidental damage) I shall henceforth refer to as “asset-revaluation increments.” They have their counterpart in what are commonly called “capital gains and losses” on the financial claims held against these real assets.

¹ See my article “The Estimation of Capital Consumption in National Accounting,” *The Review of Economic Studies*, June 1956.

It is obvious that these asset-revaluation increments and capital gains will have to find a place in the system of national accounts if this system is to provide a proper link between the national balance sheets at the beginning and the end of the accounting period. But how? A first possibility that suggests itself is to redefine capital formation so as to include asset revaluation increments. But here, of course, the matter cannot rest, for both the capital account and the production account are thrown out of balance by this change in the definition of capital formation. The obvious way of restoring the balance of the system is to redefine at the same time a number of other items so as to include the corresponding capital gains and losses and to follow, for the sake of consistency, the same procedure with respect to those capital gains and losses that have their origin abroad. This operation results in a system of accounts which can be symbolised as follows, primed symbols being used to denote redefined items:

1. Production account: $I' + M' = C + K' + E'$
2. Income account: $C + S' = I' + T$
3. Capital account: $K' + U' = S'$
4. Rest-of-the-world account: $E' + T = M' + U'$

where I' = national factor income (including net factor income from abroad) *plus* capital gains on residents' investment at home and abroad;

M' = imports (including factor income to abroad) *plus* capital gains accruing to foreigners, but originating in the domestic economy (capital gains to abroad);

K' = capital formation *plus* asset revaluation increments in the value of domestic real assets;

E' = exports (including factor income from abroad) *plus* capital gains accruing to residents but originating abroad (capital gains from abroad);

S' = national saving *plus* capital gains on residents' investment at home and abroad (*i.e.*, capital gains of domestic origin *plus* net capital gains from abroad);

U' = net lending to abroad *plus* net capital gains from abroad.

This set-up does not distinguish between income from productive activity, on the one hand, and capital gains and losses, on the other. From an analytical point of view this is a serious drawback. National-accounts statisticians have always taken care to define and estimate the other items in the production account in such a way that the resulting estimate of factor income relates to productive activity only. It is with this object in mind that the change in the value of inventories between the beginning and the end of the accounting period is corrected for stock appreciation and that capital consumption is computed on the basis of normal rates of wear and tear, obsolescence

scence and accidental damage. It would clearly be a mistake to throw all this overboard.

Fortunately, there is no need to dispense with a separate estimate of factor income from productive activity, for there appears to be an elegant way out of the dilemma of establishing a link between national accounts and national balance sheets via the capital account, on the one hand, and providing a correct measure of the net result of productive activity via the production account, on the other. The solution concerned suggests itself once it is realised what is entailed in the process of redefining capital formation to include asset-revaluation increments and the corresponding changes in the definitions of all other items of the system, except consumption and net transfers from abroad, to include capital gains. What is entailed is that a parallel, equally self-balancing, framework of accounts relating to the phenomenon of asset revaluation and capital gains is superimposed on the existing accounting framework. Thus the account that has been superimposed on the existing production account may be symbolised as follows:

$$G + F = A$$

where G = (net) capital gains accruing to residents;

F = net capital gains to abroad;

A = (net) revaluation increment in the value of domestic real assets.

The relation between the corresponding items in the production account before and after the superimposition of this new account on it are as follows:

$$Y' = Y + G; M' = M + F_1; K' = K + A; E' = E + F_2$$

where $F_1 - F_2 = F$.

By keeping this new account, which henceforth I shall call the "capital revaluation account," separate from the production account in its original form the dilemma referred to above can be avoided. In order to preserve the articulation of the system, care should be taken, moreover, to make separate counter-entries on the other accounts of the system in respect of the items recorded on the production account and the capital revaluation account. The expanded system of national accounts then takes the following form, after renaming the capital account "balance-sheet mutation account" to signify that it now performs its proper function:

(a) National Economy—

production account:	$Y + M = C + K + E$
capital revaluation account:	$G + F = A$
income account:	$C + S' = Y + G + T$
balance-sheet mutation account:	$K + A + U' = S'$

(b) Rest of the World—

consolidated account: $E + T = M + U' + F$

It will be noted that for all but two items the aggregates in this new system are defined in the same way as in the original set-up of the existing accounting framework as described in the preceding section. The two exceptions are: national savings, which is now defined to include capital gains (positive or negative) accruing to residents ($S' = S + G$) and net lending to abroad, which is now defined to include net capital gains from abroad ($U' = U - F$; F = net capital gains to abroad).

The operation described above can also be interpreted as a deconsolidation of the capital account in the original model into a capital revaluation account and a balance-sheet mutation account introducing asset-revaluation increments (A) as a new type of basic aggregate and enabling accounting distinctions to be made between: (1) national saving including capital gains (S'), on the one hand, and national capital gains (G) on the other, two items which were previously shown as a combined item ($S = S' - G$); and (2) net lending to abroad, including net capital gains from abroad (U') and net capital gains from abroad ($-F$), two items which were previously also following the same accounting rules and accordingly showed up in the system as one combined item ($U = U' + F$). Formally this is correct, but in view of the fact that in the original set-up all aggregates were specifically defined to exclude elements of asset revaluation and capital gains, one may still prefer to interpret the operation described in this section as the superimposition of one self-balancing set of accounts on another.

Without in the least wishing to play down the difficulties that statisticians will have to overcome before they are in a position to draw up reliable estimates for asset-revaluation increments and capital gains, I would like to suggest here that for those countries for which estimates of capital consumption are available and estimates of capital formation (and capital consumption) at base-year prices are currently being prepared a good deal of the relevant information on asset revaluation and capital gains is already available. The statisticians of these countries must at least have some notion of the size and composition of the country's stock of real assets (*i.e.*, fixed assets and inventories) and the price development of such assets category by category. They may even have some notion of the year-to-year deviations from normal of the actual rates of wear and tear, obsolescence and accidental damage. All this should go a long way, if not all the way, towards estimating asset-revaluation increments for the economy as a whole and probably also by sector. As regards the distribution of capital gains between the national economy and its sectors, on the one hand, and the rest of the world, on the other, tax statistics and banking statistics (including quotations of shares on the Stock Exchange) may prove valuable sources of information, but in all likelihood they will have to be supplemented by special inquiries. On the whole, however, there would seem to be no reason to despair of the possibility of preparing some sort of estimates of asset revaluation increments and capital gains.

TERMS-OF-TRADE EFFECTS AND DEFLATION OF NON-COMMODITY FLOWS¹

Almost without exception² national-accounts statisticians have taken the view that only commodity flows can be deflated, *i.e.*, revalued at the prices of a common base period, since all other items in the system of national accounts, such as transfers, saving and lending, have no specific price attached to them. Consequently, deflation has customarily been confined to only one basic type of account, *viz.*, the production account, which is the type of account on which all commodity flows of the system get recorded.³

With all items on the production account in its aggregated form for the economy as a whole revalued at the prices of a common base period, this account may be symbolised as follows:

$$y + m = c + k + e$$

where small characters denote the deflated values of the items concerned. Each item in this account is determined by dividing the current-value estimate of the item concerned by its own price index, *e.g.*, $c = C/p^c$, where p^c denotes the price index of consumption; the units of measurement of these indices are such that their base-period values are equal to unity, thus $p^c_0 = 1.00$.

There is only one item in the production account which cannot be deflated directly because it is not a commodity flow in the true sense of the word, and that is "product" (Y). This difficulty, however, is overcome in an elegant manner by what has become known as the Geary method. This method is based on the idea that an account that balances in terms of current-price estimates ought to balance as well if presented in terms of the corresponding constant-price estimates. Consequently, it lays down the rule that y should be determined as a residual, or what amounts to the same, as the aggregate of all other items in the production account ($y = c + k + e - m$). Having thus determined y , one can define p^y —the price index of the net national product at market prices⁴—by dividing y into Y , thus $p^y = Y/y$.

Finally, it should be noted that the index number formulae for price and

¹ The proposals made in this section round off the various arguments which I have put forward in earlier writings on the subject of measuring terms-of-trade effects.

² The one exception I know of is a paper read to the 1957 meeting of the International Association for Research in Income and Wealth by R. W. Burge and R. C. Geary on "Balancing of a System of National Accounts in Real Terms." The proposals put forward by these two authors, however, differ considerably from those made here.

³ The problems encountered in deflating commodity flows and the principles that should guide statisticians in solving them have been reviewed in considerable detail in a penetrating study by Richard Stone, *Quantity and Price Indexes in National Accounts* (O.E.E.C., Paris, 1956).

⁴ It depends, of course, on the definition of the aggregate concerned whether the price index refers to a gross or a net concept of product, and equally whether it refers to a national or a domestic concept of product. In sector-production accounts the corresponding price index for each sector is a price index for the "value added" of that sector, *e.g.*, p^y_i is the price index for the value added in sector i .

volume should be chosen in such a way as to make the value index of each aggregate correspond to the product of its price index and its volume index. Normally this is achieved by applying the Laspeyres formula to volume indices and the Paasche formula to price indices, but the reverse combination or the application of either the Fisher formula or the new index number formula which I recently suggested,¹ to both price and volume would give equally acceptable results from this point of view.

So far so good, but what about the other items in the national accounts, the non-commodity flows? To them applies with equal force the statement made by Stone in the opening paragraph of his study: "The pound note of 1955 may look similar to the pound note of 1938 but in terms of what it will buy it is quite different" (*op. cit.*, p. 9). Here too, therefore, one would like to correct for changes in the purchasing power of money in making comparisons over time between the national accounts of different periods. The problem one is faced with here is to select an appropriate price index for this purpose, since the non-commodity flows do not have specific price indexes of their own. As with so many problems in national accounting, this is one which can be solved only by introducing a convention, for in last resort the choice is an arbitrary one.

In order to make the convention as acceptable as possible, it is clearly desirable to choose an index of wide coverage so as to guarantee that its movements properly reflect the general tendency of price developments. This condition narrows the choice considerably; so much so, in fact, that there is very little choice left. For one thing, indices of wide coverage are few in number, and for another they normally follow, with only slight variations between them, the same development over time. Examples of such indices that come to mind immediately are the price indices of total expenditure ($C + K$), total output ($C + K + E$) and total product ($C + K + E - M$). Of these, the latter has in a sense the widest coverage. Also it has the advantage that it is free of foreign elements, reflecting, as it does, the price development of a given combination of home factors of production of constant productivity. And last but not least, it is the only price index in the production account specifically constructed for the deflation of a non-commodity flow (factor income). I would, therefore, recommend that the product price index (p^y) be chosen as a common deflator. Where there is a choice between price indices of gross and net product, the latter would obviously deserve preference, since it is entirely free of duplication. Moreover, if there is a choice between national and domestic product, the domestic-product price index is an obvious choice, since national product, as distinct from domestic product, includes non-commodity flows in the form of factor income to and from abroad; the national-product price index cannot be determined within the framework of the production account with the help of the Geary method unless special conventions are introduced for the deflation of these

¹ See my article on "A New Index Number Formula," *Econometrica*, January 1957.

non-commodity flows which have no specific price index of their own attached to them. Finally, if there is a choice between market-price and factor-cost valuation I would prefer the market-price concept, since that is the price concept that underlies all economic transactions. Ideally, therefore, it is the price index of the net domestic product at market prices that should be used as a common deflator for the whole system of national accounts.

The system of national accounts in real terms that results from the application of such a common deflator to all items in the system of purely national accounts expressed in current-value terms as shown at the outset of this paper may be symbolised as follows:

(a) National Economy—

production account: $y' + m' = c' + k' + e'$

income account: $c' + s' = y' + t'$

capital account: $k' + u' = s'$

(b) Rest of the World—

consolidated account: $e' + t' = m' + u'$

where, as before, small characters denote deflated values, while primed symbols have been used to denote that the deflation from current values has been effected by means of a common deflator for all items.

The production account in this model is not the same as that shown at the beginning of this section. The difference between the two production accounts in real terms is, of course, due to the difference in the deflation procedure applied to the current-year values of the aggregates that get recorded on this type of account. Where a common deflator is used for all commodity flows their current-year values are corrected only for changes in the general price level (or purchasing power of money) between the base year and the current year. Where instead each commodity flow is deflated by its own price index the current-year value of each of these commodity flows is also corrected for the change in its relative price. Accordingly, one might describe the differences between the corresponding entries on the two production accounts in real terms as price-structure effects. For a given commodity flow such a price-structure effect measures the difference in the number of units of product or income of base-year purchasing power that have to be given up in the current year as compared with the base year to purchase the current-year volume of that commodity flow. In other words, these price-structure effects measure the real income gains that result from the change in the relative prices of the commodities purchased. They might, therefore, also be labelled "income exchange benefits" or "income gains" for short.

Denoting price-structure effects by double-primed symbols, e.g., $y'' = y - y'$, $m'' = m - m'$, etc., the above model can be rewritten as follows:

(a) National Economy—

production account: $(y - y'') + (m - m'') = (c - c'') + (k - k'') + (e - e'')$

income account: $(c - c'') + s' = (y - y'') + t'$

capital account: $(k - k'') + u' = s'$

(b) Rest of the World—

consolidated account: $(e - e'') + t' = (m - m'') + u'$

This set-up clearly shows how the production account in real terms in its familiar form as shown at the beginning of this section can be made to form part of a complete system of national accounts in real terms. Obviously what is required is a deconsolidation of the production account in the above model into two separate accounts, viz., (1) a production account in real terms of the familiar type, and (2) what may be called a "price-structure effects account" or "income exchange benefits account." The following fully articulated system of national accounts in real terms results from this operation:

(a) National Economy—

production account: $y + m = c + k + e$

price-structure effects account: $c'' + k'' = m'' - e''$

income account: $c + s' = y + c'' + t'$

capital account: $k + u' = s' + k''$

(b) Rest of the World—

consolidated account: $e + t' + m'' - e'' = m + u'$

It will be noted that in this model y'' does not appear any more for the simple reason that $y' = y$ (both deflated by the same price index) and therefore $y'' = 0$. Furthermore, e'' is shown with sign reversed. The purpose of this particular form of presentation, which makes it possible to combine m'' and e'' into one item ($m'' - e''$), is to demonstrate the relation that exists between price-structure effects, on the one hand, and the terms-of-trade effect, on the other. To make this relation clear we first write out in full the expression for e'' , denoting the price index of exports by p^e and the common deflator by the symbol p without a superscript:

$$e'' = e - e' = \frac{E}{p^e} - \frac{E}{p} = e \left(1 - \frac{p^e}{p} \right)$$

This expression with sign reversed ($-e''$) would correspond exactly to the conventional measure for the effect on a country's economy of changes in its terms of trade with the rest of the world, expressed in base-year prices, if p^m —the import price index—had been chosen as a common deflator and if, moreover, the terms of trade were defined according to continental usage (p^e/p^m) rather than according to United Kingdom usage (p^m/p^e).¹ Likewise,

¹ I introduced this expression for the first time in Appendix II to my paper on "The Use of National Accounts in Economic Analysis," *Income and Wealth*, Series IV (London, 1955), which

$m'' = m\left(1 - \frac{p^m}{p}\right)$ would provide a measure of the effect of changes in the terms of trade on the economy's foreign balance and its available resources in real terms if p^e were chosen as a common deflator and the terms of trade were defined according to United Kingdom usage. As I had already on occasion to point out elsewhere,¹ these two different measures of terms-of-trade effects can be derived from a more general formula, viz., $m'' - e'' = m\left(1 - \frac{p^m}{p}\right) - e\left(1 - \frac{p^e}{p}\right)$, of which the first term vanishes if $p = p^m$, while the second term vanishes if $p = p^e$. This formula shows clearly that the price index which is used as a common deflator forms an essential element in the definition of the terms-of-trade effects.² It also shows that it is correct to describe the terms-of-trade effect on a country's foreign balance as the difference between the effects of changed terms of trade on the country's exports and imports separately. It proves particularly necessary to retain this formulation if neither the import price index nor the export price index is used as a common deflator, as happens to be the case in the model of the national-accounts system in real terms presented above. In this system one can accordingly replace $m'' - e''$ on the rest-of-the-world account and the price-structure effects account by one single item (n''). This item I shall call the "net terms-of-trade effect" in order to distinguish it clearly from the (gross) terms-of-trade effect on imports (m'') and the (gross) terms-of-trade effect on exports (e'').

When a similar statistical analysis is carried out for individual sectors of an economy the net terms-of-trade effects for the sectors will add up to the net terms-of-trade effect for the country as a whole, provided, of course, that for all sectors one and the same common deflator is used.

With respect to c'' and k'' it would be rather misleading to label them terms-of-trade effects instead of price-structure effects, the reason for this being twofold. First, c'' and k'' relate to intra-sector rather than inter-sector transactions and, of course, it only makes sense to talk of terms of trade and their effects with respect to transactions between sectors. Secondly, and this is the more important consideration, the size of c'' and k'' is not only dependent on the terms of trade of the national economy with the rest of the world. That this is so becomes immediately clear once we consider a closed economy. For such an economy n'' is by definition equal to zero, but c'' and k'' will normally have non-zero values, even though their sum will be equal

to zero. These non-zero values of c'' and k'' reflect changes in the prices of consumption goods relative to those of investment goods, or in other words, differences in the development of the net-value-added price indices for these two types of commodity flow. The same differences in the internal components of price developments will be encountered in an open economy, and therefore one should be on one's guard not to interpret c'' and k'' as simply reflecting the statistical impact of changed terms of trade on the real flows of consumption and capital formation.

Finally, I should like to draw attention here to the distinction between the concepts of real product (y) and real income (i). The latter concept is defined as $y + n''$, and may be described as a measure of how well-off the country is in the current year, taking into account the effect of changes in the terms of trade between the base year and the current year. Owing to the fact that c'' and k'' are recorded on different accounts, it can nowhere be shown explicitly in our model, unless one were to consolidate the income account and the capital account in real terms into a financing account in real terms. The latter account would look as follows:

$$c + k + u' = y + c'' + k'' + t'$$

In it the first three terms on the right-hand side add up to i . A further consolidation of this financing account with the price-structure effect account resulting in

$$c + k + u' = y + n'' + t'$$

would eliminate c'' and k'' from the system but would still leave scope for distinguishing real national income from real national product. While real product (y) would seem a concept relevant to the study of production and productivity developments, the real income (i) would seem a more appropriate concept to use in the study of welfare.

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STATISTICAL APPENDIX

In this appendix the United Kingdom accounts in current prices and in 1948 prices are presented for the period 1948-57 in the form of a system of national and domestic accounts. The distinction between national and domestic concepts makes for a slightly more complicated accounting framework than that used in the main text. The relation between the two types of concept, however, should be clear from the stub of the tables.

All data in these tables are based on information derived from *National Income and Expenditure 1958* (Central Statistical Office, H.M.S.O., London, August 1958). Reference to the series which have been obtained directly from this source are made in the last column of each table. In these source references the first figure refers to the table number and the second to the number of the line in that table (the original source does not give this numbering, but it can, of course, easily be determined in there by the user). Price-structure effects were determined by deflating each commodity flow by the net domestic-product market-price index (obtained by dividing the net domestic-product data in Table II into those in Table I) and by subtracting the figures thus obtained from the corresponding data deflated by their own price index.

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¹ See my article on "A New Approach to the Measurement of Terms-of-trade Effects" in *The Review of Economics and Statistics*, August 1956.

² How the measure of terms-of-trade effect is affected by the choice of p I showed in my article on "The Impact of Changes in the Terms of Trade on Western Europe's Balance of Payments" in *Econometrica*, January 1958, by using alternatively the gross national product, the gross national expenditure, and the import and export price indices as common deflator.

TABLE I
National and Domestic Accounts for the United Kingdom 1948-57 at Current Prices

£ million

	1948.	1949.	1950.	1951.	1952.	1953.	1954.	1955.	1956.	1957.	Source reference.
1. <i>U.K. Domestic Production Account</i>											
Factor services of:											
1.1. U.K. residents (2.4)	10,556	11,230	11,679	12,960	14,108	15,037	15,965	16,959	18,392	19,435	7.3
1.2. Non-U.K. residents (4.7)	214	224	246	321	361	355	374	469	473	486	
Net Domestic Product	10,770	11,454	11,925	13,281	14,469	15,392	16,339	17,428	18,865	19,921	
1.3. Domestic Imports (4.5)	2,104	2,306	2,694	3,915	3,386	3,295	3,429	3,972	4,092	4,236	[7.1 + 7.2 - 26.62]
Total = Net Domestic Input	12,874	13,760	14,619	17,196	17,855	18,687	19,768	21,400	22,957	24,157	
Consumption goods and services delivered to:											
1.4. U.K. residents (2.1)	10,143	10,720	11,314	12,376	13,360	14,138	14,954	15,855	16,816	17,547	[26.61 + 1.2]
1.5. Non-U.K. residents (4.3)	33	44	65	85	104	117	123	140	150	160	
Domestic Consumption	10,176	10,764	11,379	12,461	13,464	14,255	15,077	15,995	16,966	17,707	
1.6. Net Domestic Capital Formation (3.1)	740	735	520	1,350	891	1,152	1,209	1,613	1,755	2,078	1.3 + 1.4 - 1.23
Net Domestic Expenditure	10,916	11,499	11,899	13,811	14,355	15,407	16,286	17,608	18,721	19,785	
1.7. Domestic Exports (4.1)	1,958	2,261	2,720	3,385	3,500	3,280	3,482	3,792	4,236	4,372	[7.11 + 7.12 + 26.60]
Total = Net Domestic Output	12,874	13,760	14,619	17,196	17,855	18,687	19,768	21,400	22,957	24,157	
2. <i>U.K. National Income Account</i>											
Consumption:											
2.1. In U.K. (1.4)	10,143	10,720	11,314	12,376	13,360	14,138	14,954	15,855	16,816	17,547	[26.61 + 1.2]
2.2. Outside U.K. (4.6)	94	105	116	152	147	149	169	184	194	210	26.62
National Consumption	10,237	10,825	11,430	12,528	13,507	14,287	15,123	16,039	17,010	17,757	1.1 + 1.2
2.3. Net National Saving (3.3)	879	920	960	990	1,138	1,340	1,439	1,544	2,021	2,315	
Total = Disposal of National Income	11,116	11,745	12,390	13,518	14,645	15,627	16,562	17,583	19,031	20,072	

Factor income earned:											
2.4. In U.K. (1.1)	10,556	11,230	11,679	12,960	14,108	15,037	15,965	16,959	18,392	19,435	7.13
2.5. Outside U.K. (4.2)	401	386	583	538	454	518	602	646	676	712	
National Factor Income	10,957	11,616	12,262	13,498	14,562	15,555	16,567	17,605	19,068	20,147	
2.6. Net Transfers from Abroad (4.4)	159	129	128	20	83	72	-5	-22	-37	-75	
Total = Disposable National Income	11,116	11,745	12,390	13,518	14,645	15,627	16,562	17,583	19,031	20,072	
3. <i>U.K. National Capital Account</i>											
3.1. Net Domestic Capital Formation (1.6)	740	735	520	1,350	891	1,152	1,209	1,613	1,755	2,078	1.3 + 1.4 - 1.23
3.2. Net Lending Abroad (4.8)	139	185	440	-360	247	188	230	-69	266	237	7.7
Total = Investment of Net National Saving	879	920	960	990	1,138	1,340	1,439	1,544	2,021	2,315	
3.3. Net National Saving (2.3)	879	920	960	990	1,138	1,340	1,439	1,544	2,021	2,315	
Total = Net National Saving	879	920	960	990	1,138	1,340	1,439	1,544	2,021	2,315	
4. <i>Rest of World Consolidated Account with U.K.</i>											
4.1. Domestic Imports from U.K. (1.7)	1,958	2,261	2,720	3,385	3,500	3,280	3,482	3,792	4,236	4,372	[7.11 + 7.12 + 26.60]
4.2. Factor services of U.K. residents (2.5)	401	386	583	538	454	518	602	646	676	712	7.13
4.3. Consumption in U.K. (1.5)	33	44	65	85	104	117	123	140	150	160	-26.60
National Exports of U.K. to Rest of World	2,392	2,691	3,368	4,008	4,058	3,915	4,207	4,578	5,062	5,244	7.14
4.4. Net Transfers to U.K. residents (2.6)	159	129	128	20	83	72	-5	-22	-37	-75	
Total = Net Payables to U.K.	2,551	2,820	3,496	4,028	4,141	3,987	4,202	4,556	5,025	5,169	
4.5. Domestic Exports to U.K. (1.3)	2,104	2,306	2,694	3,915	3,386	3,295	3,429	3,972	4,092	4,236	[7.1 + 7.2 - 26.62]
4.6. Consumption goods and services delivered to U.K. residents (2.2)	94	105	116	152	147	149	169	184	194	210	26.62
4.7. Factor income earned in U.K. (1.2)	214	224	246	321	361	355	374	469	473	486	7.3
National Imports of U.K. from Rest of World	2,412	2,635	3,056	4,388	3,894	3,799	3,972	4,625	4,759	4,932	7.4
4.8. Net Borrowing from U.K. residents (3.2)	139	185	440	-360	247	188	230	-69	266	237	7.7
Total = Net Receivables from U.K.	2,551	2,820	3,496	4,028	4,141	3,987	4,202	4,556	5,025	5,169	

TABLE II
National and Domestic Accounts for the United Kingdom 1948-57 at 1948 Prices
 (Non-commodity flows deflated by net domestic-product market-price index)
 £ million

	1948.	1949.	1950.	1951.	1952.	1953.	1954.	1955.	1956.	1957.	Source reference.
1. <i>U.K. Domestic Production Account</i>											
Factor services of:											
1.1. U.K. residents (3.4)	10,556	10,938	11,204	11,555	11,554	12,001	12,518	12,880	13,118	13,342	
1.2. Non-U.K. residents (5.8)	214	218	236	287	296	283	293	356	337	334	
Net Domestic Product	10,770	11,156	11,440	11,882	11,850	12,284	12,811	13,236	13,455	13,676	
1.3. Domestic Imports (5.6)	2,104	2,259	2,279	2,602	2,358	2,520	2,630	2,914	2,996	3,094	-13.16 - 28.60
Total = Net Domestic Input	12,874	13,415	13,719	14,484	14,208	14,804	15,441	16,150	16,451	16,770	
Consumption of goods and services delivered to:											
1.4. U.K. residents (3.1)	10,143	10,460	10,647	10,724	10,902	11,290	11,662	11,924	12,005	12,115	28.59 + 13.2
1.5. Non-U.K. residents (5.3)	33	43	60	73	84	91	95	105	109	113	-28.58
Domestic Consumption	10,176	10,503	10,707	10,797	10,986	11,381	11,757	12,029	12,114	12,228	
1.6. Net Domestic Capital Formation (4.1)	740	726	519	1,078	679	906	952	1,213	1,231	1,433	13.3 + 13.4 + 13.18
Net Domestic Expenditure	10,916	11,229	11,226	11,875	11,665	12,287	12,709	13,242	13,345	13,661	
1.7. Domestic Exports (5.1)	1,958	2,186	2,493	2,609	2,543	2,517	2,732	2,908	3,106	3,109	13.5 + 28.58
Total = Net Domestic Output	12,874	13,415	13,719	14,484	14,208	14,804	15,441	16,150	16,451	16,770	
2. <i>U.K. National Income Exchange Benefits Account</i>											
Income gains on consumption purchases:											
2.1. In U.K. (3.6)	—	19	-207	-348	-40	6	-64	-117	11	68	
2.2. Outside U.K. (3.7)	—	-5	-15	-21	-16	-13	-11	-10	-5	5	
Income Gain on National Consumption	—	14	-222	-369	-56	-7	-75	-127	6	73	
2.3. Income Gain on Net Domestic Capital Formation (4.4)	—	10	20	-130	-51	-13	4	-12	-21	7	
Total = Income Gain on Net National Expenditure	—	24	-202	-499	-107	-20	-71	-139	-15	80	
2.4. Net Terms-of-Trade Effect (5.4) *	—	24	-202	-499	-107	-20	-71	-139	-15	80	
Total = Net Terms-of-Trade Effect	—	24	-202	-499	-107	-20	-71	-139	-15	80	

3. <i>U.K. National Income Account</i>											
Consumption:											
3.1. In U.K. (1.4)	10,143	10,460	10,647	10,724	10,902	11,290	11,662	11,924	12,005	12,115	28.59 + 13.2
3.2. Outside U.K. (5.7)	94	97	96	115	104	106	121	130	133	149	28.60
National Consumption	10,237	10,557	10,743	10,839	11,006	11,396	11,783	12,054	12,138	12,264	
3.3. Net National Saving (4.3)	879	896	921	886	932	1,069	1,128	1,173	1,442	1,589	
Total	11,116	11,453	11,664	11,725	11,938	12,465	12,911	13,227	13,580	13,853	
Factor income earned:											
3.4. In U.K. (1.1)	10,556	10,938	11,204	11,595	11,554	12,001	12,518	12,880	13,118	13,342	
3.5. Outside U.K. (5.2)	401	376	559	481	372	413	472	491	482	489	
National Product	10,957	11,314	11,763	12,076	11,926	12,414	12,990	13,371	13,600	13,831	
Income gain on consumption purchases:											
3.6. In U.K. (2.1)	—	19	-207	-348	-40	6	-64	-117	11	68	
3.7. Outside U.K. (2.2)	—	-5	-15	-21	-16	-13	-11	-10	-5	5	
3.8. Net Transfers from Abroad (5.5)	159	125	123	18	68	58	-4	-17	-26	-51	
Total	11,116	11,453	11,664	11,725	11,938	12,465	12,911	13,227	13,580	13,853	
4. <i>U.K. National Capital Account</i>											
4.1. Net Domestic Capital Formation (1.6)	740	726	519	1,078	679	906	952	1,213	1,231	1,433	13.3 + 13.4 + 13.18
4.2. Net Landing Abroad (5.9)	139	180	422	-322	202	150	180	-52	190	163	
Total	879	906	941	756	881	1,056	1,132	1,161	1,421	1,596	
4.3. Net National Saving (3.3.)	879	896	921	886	932	1,069	1,128	1,173	1,442	1,589	
4.4. Income Gain on Net Domestic Capital Formation (2.3)	—	10	20	-130	-51	-13	4	-12	-21	7	
Total	879	906	941	756	881	1,056	1,132	1,161	1,421	1,596	
5. <i>Rest of World Consolidated Account with U.K.</i>											
5.1. Domestic Imports from U.K. (1.7)	1,958	2,186	2,493	2,609	2,543	2,517	2,732	2,908	3,106	3,109	13.5 + 28.58
5.2. Factor services of U.K. residents (3.5)	401	376	559	481	372	413	472	491	482	489	
5.3. Consumption in U.K. (1.5)	33	43	60	73	84	91	95	105	109	113	-28.58
National Exports of U.K. to Rest of World	2,392	2,605	3,112	3,163	2,999	3,021	3,299	3,504	3,697	3,711	
5.4. Net Terms-of-Trade Effect (2.4) *	—	24	-202	-499	-107	-20	-71	-139	-15	80	
5.5. Net Transfers to U.K. residents (3.8)	159	125	123	18	68	58	-4	-17	-26	-51	
Total	2,551	2,754	3,033	2,682	2,960	3,059	3,224	3,348	3,656	3,740	

TABLE II (continued)

	1948.	1949.	1950.	1951.	1952.	1953.	1954.	1955.	1956.	1957.	Source reference.
5. Rest of World Consolidated Account with U.K.											
5.6. Domestic Exports to U.K. (1.3) . . .	2,104	2,259	2,279	2,602	2,358	2,520	2,630	2,914	2,996	3,094	—13.16 — 28.60
5.7. Consumption goods and services delivered to U.K. residents (3.2) . . .	94	97	96	115	104	106	121	130	133	149	
5.8. Factor income earned in U.K. (1.2) . . .	214	218	236	287	296	283	293	356	337	334	28.60
National Imports of U.K. from Rest of World . . .	2,412	2,574	2,611	3,004	2,758	2,909	3,044	3,400	3,466	3,577	
5.9. Net Borrowing from U.K. residents (4.2) . . .	139	180	422	—322	202	150	180	—52	190	163	
Total . . .	2,551	2,754	3,033	2,682	2,960	3,059	3,224	3,348	3,656	3,740	
* Origin of Net Terms-of-Trade Effect											
Income gain on national exports:											
(a) On domestic exports . . .	—	16	117	420	323	101	—2	—28	—85	—108	
(b) On consumption of non-U.K. residents . . .	—	—	2	3	1	2	1	1	—2	—3	
Income gain on national imports:											
(a) On domestic imports . . .	—	13	—306	—901	—415	—110	—59	—102	77	186	
(b) On consumption outside U.K. . .	—	—5	—15	—21	—16	—13	—11	—10	—5	5	
Total . . .	—	24	—202	—499	—107	—20	—71	—139	—15	80	

THE THEORY OF SELLING COSTS

THIS paper is concerned with the selling cost, price and output decisions of firms in various circumstances. The literature on this subject is quite large.¹ It appears to suffer from two deficiencies:² it has not produced a simple diagrammatic technique to represent the decisions in question and it does not contain an adequate statement of the second-order conditions of a maximum. These two defects are related, since a major part of the writings in this field are "geometrical," and the non-availability of simple diagrams has led to a neglect of second-order conditions. Without these, however, the theory cannot be applied to an examination of such questions as the effects of excise taxes and/or of sales-cost taxes on the decisions of the firm, nor can the theory be said to be complete.

In what follows, a simple technique is outlined which enables one to represent the decisions with respect to selling costs, price and output on a single diagram. The technique is then used to discuss a number of Problems in Monopolistic Competition Theory and Taxation. A mathematical appendix restates some of the results somewhat more rigorously.

I

(1) First some definitions and assumptions:

(a) Everyone knows that the distinction between selling and other costs is not at all precise. From the point of view of the theory of the firm, the need for the distinction arises from the recognition of the fact that not all outlays of a profit-maximising firm can be explained by the hypotheses that these outlays are minimised for any level of output. No difficulties arise if the physical characteristics of the output can be taken as given. If that is not the case, then no precise meaning can be given to the phrase "any level of output." In this paper it will be

¹ For an extensive bibliography see E. H. Chamberlin, *The Theory of Monopolistic Competition* (Cambridge, Mass. 1947, 5th edition. All references are to this edition.) T. O. Yntema, in a summary of a paper read before the American Economic Association, December 1933, in *American Economic Review*, Supplement XXIV, 1937, pp. 21-3, has some of the main results of this paper but without proof. H. Smith, in "Advertising Costs and Equilibrium," *Review of Economic Studies*, Vol. II, 1933, pp. 62-5, produced a neat diagram, but did not explore the analytical implications. Robert M. Shone, "Selling Costs," *Review of Economic Studies*, Vol. II (1933), pp. 225-31, states all first-order conditions diagrammatically, but does not really carry out a detailed analysis. K. Boulding, in *Principles of Economic Analysis*, presents a diagram which as far as I can understand it is wrong.

² Since this was written, my attention has been drawn to Norman S. Buchanan, "Advertising Expenditures: A Suggested Treatment," *Journal of Political Economy*, Vol. 50, 1942, pp. 537-57. This paper is undoubtedly amongst the best published on this topic, and must be exempted from the criticism in the text. Yet his diagram is not perhaps the best available, since it does not lend itself easily to a discussion of the connection between selling costs and the elasticity of demand. Mr. Buchanan did not use his diagram to carry out an exhaustive analysis.

(b) *Production Costs* are defined as all those costs which a profit maximising firm, producing a given product, would minimise for any level of output.

Selling Costs are defined as the total current outlay of the firm minus Production Costs.

Net Revenue is defined as Gross Revenue minus Selling Costs.

(2) We shall first examine the firm in isolation. By this I mean the behaviour of a firm when it has made certain assumptions concerning its environment and the latter's reactions to its own actions and when nothing has happened to falsify those assumptions. This firm we imagine as regarding the demand for its product as depending on two variables: price charged and the amount spent on selling costs. It is extremely unlikely (3) that the firm will assume the demand to be linear in both prices and selling costs. Observation suggests that there is a certain minimum selling outlay below which it has no perceptible effect on demand whatsoever. Thereafter it seems to be subject to ranges of increasing and diminishing returns. By "increasing returns to selling costs" we denote a situation where the increase in sales, at a given price, as a result of a small increase in selling costs is increasing. Diminishing returns are defined analogously.

$$x = P^\alpha [(K - S)^\beta + mS]; \alpha < 0, \beta \geq 2, K > 0, m > \beta K^{\beta-1} \quad (1)$$

¹ In what follows, only selling costs currently incurred are considered (by far the largest proportion of all such costs) and once over, selling outlays are ignored.

FIG. 1

That this is so can be seen as follows: $\frac{SR}{QR} = \frac{MO}{ON} = 600$, by construction. Hence $QR \times \frac{SR}{QR} = SR =$ Gross revenue associated with Q . Since $OR =$ selling costs, OS measures net revenue. OS is clearly maximised when the line parallel to MN just touches $x = 600$ from above.

In general, then, for any x , net revenue is maximised when $\left(\frac{dP}{dS}\right) = \frac{1}{x}$ ($x = \text{constant}$) or in words, when the increase in gross revenue is equal to the increase in selling costs which gives rise to it.

The following points should be noted:

- (i) At the point of maximum net revenue, if selling costs are positive, the marginal rate of transforming selling costs into higher prices, at constant output, must be diminishing.
- (ii) There may be no line parallel to MN which touches the relevant constant-sales curve from above. In that case no selling cost will be incurred by the profit-maximising firm.
- (iii) As we move from lower to higher output curves the slope of MN diminishes.
- (iv) In Fig. 1 TU measures average selling costs and OU average net revenue.

(3) We now wish to inquire how, under our present assumptions, the optimum point Q changes with sales.

Consider the point Q' on $x = 700$, where the price charged is the same as that charged at Q for $x = 600$. If Q' were to be the optimum point for $x = 700$, the slope of the curve $x = 700$ at Q' must be less than the slope of $x = 600$ at Q . (See the general equilibrium condition above.) Indeed, the slope at Q' (if it were the optimum) would have to be $\frac{6}{7}$ of the slope at Q .¹ But we have assumed the elasticity of demand to remain constant throughout, and so the absolute value of the slope of the demand curve at Q' must be $\frac{7}{6}$ times as great as it is at Q .² If the marginal return to selling costs at Q' were the same as at Q , Q' would indeed be the new optimum point. For a small increase in selling costs at Q' would shift the demand curve by the same absolute amount as it would at Q , but at Q' , in order to continue selling 700 units, the price could be raised by only $\frac{6}{7}$ as much as at Q . But the marginal return to selling costs must be lower at Q' than at Q , for at Q marginal returns to selling costs must be diminishing (see (i) above),³ and selling costs at Q' are higher than at Q . Hence it follows that the slope of $x = 700$ at Q' is less than $\frac{6}{7}$ of the slope of $x = 600$ at Q . By exactly similar reasoning, which can be left to the reader, it can be shown that the slope of $x = 700$ at Q'' (i.e., at the point where selling costs for $x = 700$ are the same as those at Q for $x = 600$) is greater than $\frac{6}{7}$ of the slope of $x = 600$ at Q . Hence the optimum point for $x = 700$ lies between Q'' and Q' .

The line PS is the locus of all points such as Q for different volume of

¹ Since $\frac{1}{700}$ is $\frac{6}{7} \times \frac{1}{600}$.

² The elasticity of demand at $Q = \frac{\partial x}{\partial P} \frac{P}{600}$, at $Q' = \frac{\partial x}{\partial P} \frac{P}{700}$, and since they are the same, $\frac{\partial x}{\partial P}$ at Q is $\frac{7}{6}$ times $\frac{\partial x}{\partial P}$ at Q' .

³ Strictly speaking, (i) refers to the marginal rate of transformation, and not the marginal return to selling cost. However, throughout this paper it will be assumed that the effect of a small price change on the marginal return to selling cost is small (absolutely), relatively to the effect of a small change in selling cost on the marginal return from selling costs. This assumption, which seems sensible, then implies the assertion in the text.

sales. On our assumptions, as we have shown, it is true that the optimum selling costs increase with sales, while the optimum price diminishes. This, in fact, will always be so as long as the elasticity of demand at points such as Q' is not less than it is at Q . It is only when selling costs lead to a decline in the elasticity of demand that PS may have a positive slope. We shall return to this presently. It should be noted that above V (Fig. 1) PS coincides with the vertical axis.

(4) To get any farther, we must introduce production costs. It is easily shown that for maximum profit, marginal production costs must equal marginal gross revenue. Since we are assuming a constant elasticity of demand, there will be one unique price, for any given output, at which this condition will be fulfilled. Hence once this price is known, we can find the amount that will have to be spent on selling costs in order that the given amount can also be sold. In Fig. 1 OC is the price at which the marginal gross revenue for $x = 600$ is equal to the marginal production costs of producing that amount. Hence for that output production equilibrium would be reached if OR' were spent on selling costs and the price charged is PR . The following points should be noted:

- (i) There may be no combination of selling cost and prices at which production equilibrium could be reached for the given output. That is, OC may be so large that the line CC nowhere intersects the curve $x = 600$.
- (ii) While it may be possible to satisfy the first-order conditions of production equilibrium for a given output, the second-order conditions of production equilibrium may be violated. The second-order conditions of production equilibrium are, of course, that the marginal-cost curve should intersect the marginal revenue curve from above. We shall plot such points only where these conditions are satisfied.

It will be obvious that, in on our present assumptions, the production equilibrium price will fall with rising output when marginal costs are falling and rise with rising output when marginal costs are rising. What about selling costs?

Suppose that marginal costs are falling and consider P' on $x = 700$, i.e., a point on that curve selling costs are the same as at P on $x = 600$. The demand curve has not shifted between P and P' . Since the second-order conditions of production equilibrium are satisfied at P , it follows that at P' marginal production costs must exceed marginal gross revenue. Hence P' cannot be a point of production equilibrium for $x = 700$, but this point must lie to the right of P' . The same is *a fortiori* true if marginal costs are rising. Hence the selling cost appropriate to production equilibrium must always be increasing with increasing output.

(5) In Fig. 2 MM is the locus of all points such as P , where both the first- and second-order conditions of production equilibrium are satisfied.

We have assumed a range of diminishing and increasing marginal production costs. We have also plotted the PS curve of Fig. 1. The curves intersect at W , and at that point both the first- and second-order conditions of production and selling cost equilibrium are satisfied. This, however, does *not* yet mean that W is a point of maximum profit.

Before examining this matter, we must note a rather obvious, but sometimes neglected, fact. In making its decisions, the firm is often regarded as making a number of pairwise comparisons:¹ *i.e.*, (a) it assumes selling costs

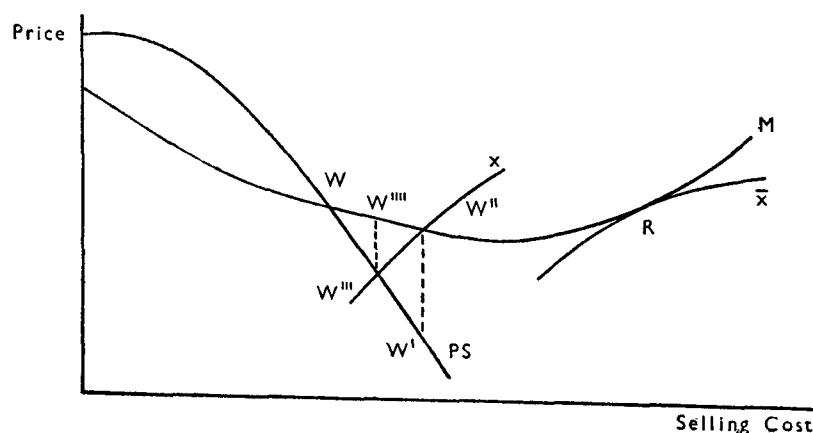


FIG. 2

as fixed and maximises profits (our production equilibrium); (b) it assumes sales as fixed and maximises profits (our selling equilibrium); and (c) it assumes price as fixed and maximises profits. But if (a) and (b) have been simultaneously accomplished, the first-order conditions for (c) are also satisfied. The first-order conditions for (c) are the equality between the increase in gross revenue minus production cost resulting from an increase in selling cost and the increase in selling costs. This condition is always satisfied when (a) and (b) have been dealt with. (See Appendix for proof.) The intuitive reason for this is that the firm has at its disposal only *two* independent decisions variables. That is, whenever two have been decided upon (say price and output, or price and selling costs), the third is also given. Hence we need only two first-order conditions to solve the maximum problem.

We are now ready to examine the intersection point W . We know that all first-order conditions for a maximum profit are satisfied there. We shall now show that all second-order conditions are also satisfied.

Suppose that the second-order conditions are *not* satisfied and that profits at W' are higher than at W . Consider W'' on MM , vertically above W' . The profits at W'' must be greater than at W' , since selling costs are the same at both points, but marginal cost exceeds marginal gross revenue at W' and is equal to it at W'' . In other words, a reduction in

¹ This dates back to Chamberlin, *op. cit.*

output must increase profits. Through W'' we have drawn the appropriate constant sales curve x . Let W''' be the intersection of that curve with PS . Then by the construction of PS it follows that profits can be increased (because net revenue for the given output can be increased), by moving from W'' to W''' . But then by the same argument as before, W''' vertically above W'' yields a still higher profit. Proceeding in this manner, we would finish up at W with the statement that profits at W are greater than profits at W , which is absurd. Hence there can be no point to the right of W yielding a high profit. By a similar procedure, it can be seen that there is no more profitable point to the left of W .

Hence the general rule for all second-order conditions to be satisfied is that the slope of PS minus the slope of MM should be negative.¹ (All slopes are referred to the vertical axis.) If we had drawn PS as intersecting MM from below (and only once), we could, by the argument already given, show that if a point such as W' is less profitable than W , that the profits at W must be less than they are, which is absurd, and so W would not be a point of maximum profits.

(6) Before concluding this first statement of the equilibrium conditions of the firm in isolation, a number of points must be noted:

(i) In Fig. 2 the constant sales curve $\bar{x}$ represents the highest output at which the production equilibrium conditions can be satisfied. Hence the MM curve will terminate at R .

(ii) If PS had intersected the vertical axis below MM and then intersected the latter three times, once on the vertical axis, once from below and once from above, there would have been *two* points at which both first- and second-order conditions of a maximum are satisfied. One of these would have been on the vertical axis (zero selling cost), the other would have been at the second intersection at positive selling costs. This is a case where the resources of calculus are insufficient, and the only course open to us is to compare the profits at the two true maxima and choose the highest.

It is, however, a case of some interest and perhaps even of practical significance. For it represents the probably not unlikely position of a firm which has maximised its profits at zero selling costs and which is aware that a relatively small selling outlay would diminish its profits, not because the marginal returns on a small selling outlay are zero but because its marginal production costs do not fall sufficiently rapidly. On the other hand, a substantial outlay on selling costs, by allowing it to take full advantage of its increasing returns of production, might be profitable. Total, not marginal, conditions are involved, and this is of some interest in the Theory of Monopolistic Competition discussed in the next section. The case considered also illustrates nicely the fact

¹ See Appendix.

that it is the effectiveness of selling costs in relation to the behaviour of production costs, rather than their effectiveness considered absolutely, which is important.

(iii) If at the output with zero selling costs marginal production costs are increasing, and if at that output all first- and second-order conditions are satisfied (PS intersects MM on the vertical axis), then the firm will not undertake any selling costs. This, of course, is only true so long as our assumptions concerning demand conditions are valid (see *post*).

(iv) It is obvious that in so far as equilibrium is achieved at positive selling costs, the scale of operation of the firm will be larger than it would have been had there been no selling costs. This, however, is only true of the firm in isolation.¹

II

(7) We shall now take account of the interaction of different firms. As a first step it will be useful to re-examine the pure Chamberlinian process of competition when selling costs are present. Professor Chamberlin's analysis is carried out by always assuming that one of the three choice variables—output, price or selling costs—is fixed, while the others are varied until all "excess profits" have been competed away.² This makes it extremely difficult to see what is happening. We shall not need this assumption. We shall, however, follow Professor Chamberlin in postulating that the firm never foresees the reactions of other firms to its own actions. We shall return to this point presently.

(8) Let $\bar{x}$ be the output produced by the firm. In Fig. 3 the curve $\bar{x}$ represents the combination of price and selling costs at which this firm believes that it can sell this particular output. Let OA represent the average production cost of $\bar{x}$ and draw AA with a slope of $\frac{1}{\bar{x}}$. Then the ordinate of AA measures the sum of average production and selling costs. Let the firm be situated at R , where AA intersects $\bar{x}$. PS and M_1M_1 are the curves derived in the previous section. They have a stable intersection point at W_1 where output is higher than $\bar{x}$ (represented by the curve x), prices lower and selling costs greater. The firm will choose this point. Alas, however, other firms, similarly situated, have done the same, *i.e.*, endeavoured to increase their sales by lowering prices and increasing selling costs. Let us now make the extreme assumption that when this happens our firm finds that in spite of its lower prices, etc., it can still only sell $\bar{x}$.

This means that $\bar{x}$ has now shifted downwards to the right to $\bar{x}_1$ and pass through W_1 . Clearly M_1M_1 will shift upwards (M_2M_2), since assuming, as we are, constant elasticities of demand and diminishing marginal pro-

¹ It is also only true as long as selling costs do not diminish the elasticity of demand.

² See Chamberlin, *op. cit.*, Chapter 7.

duction costs, the price at which these costs are equal to marginal revenue must be higher. On the other hand, *PS* will tend to shift downwards to the left. For if the marginal effect of selling costs on demand is independent of the actions of other firms, the slope of $\bar{x}$ at W_1 must be steeper than that of x at the same point, in the proportion that x exceeds $\bar{x}$. This again is a consequence of the constant elasticity assumptions. But the optimum way of selling $\bar{x}$ is to find the point where the slope of $\bar{x}$ is $\frac{1}{\bar{x}}$ and so *PS* would not shift. But the marginal returns to selling costs are likely to be diminished

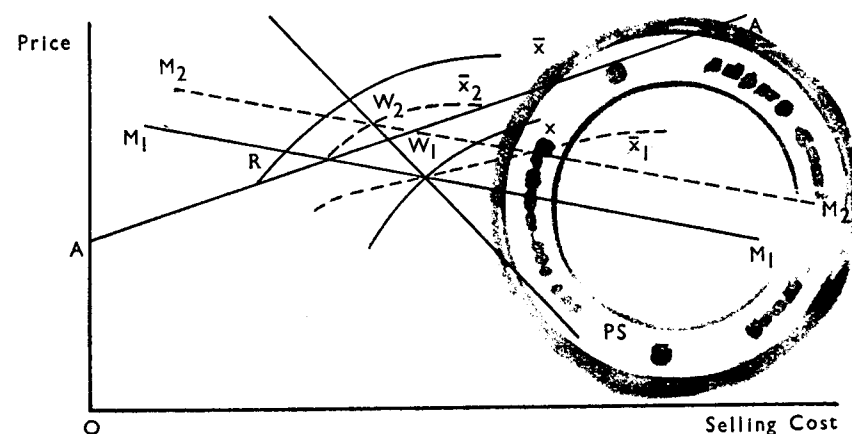


FIG. 3

by the action of other firms, and hence the slope of $\bar{x}$ at W_1 will exceed that of x at the same point by a smaller proportion than indicated and PS will tend to shift downwards. To prevent an undue number of curves we have none the less assumed PS stationary.

The new equilibrium of the firm is represented by W_2 , where M_2M_2 intersects PS . Since output is reduced below $\bar{x}$, the intersection point must lie above AA (for otherwise losses would be made). Prices are higher and selling costs lower. Other firms have acted in a similar fashion, and our firm finds this time that it has underestimated demand, since it can once again sell $\bar{x}$. The process is now reversed and M_2M_2 shifts down to the left. But it does *not* come to coincide with M_1M_1 but must lie above it. That is so because $\bar{x}_2$, which now passes through W_2 , lies to the right and below $\bar{x}$. The new intersection point will lie between W_1 and W_2 at an output which exceeds $\bar{x}$. The process now continues with ever-diminishing oscillations in price, selling costs and output. Depending on the number of firms, a final equilibrium will be reached above or on AA . Otherwise the number of firms will have to be reduced by bankruptcy.

(9) It is, I believe, self-evident that the process just described is entirely unrealistic. Firms do not raise and lower prices at all frequently, quite apart from the obvious point that they will tend to anticipate the reactions

of other firms. However, the analysis is of some interest when it is compared with the well-known Chamberlinian schemes. There the process of competition is always uni-directional, that is prices are continuously diminished or selling costs increased until equilibrium is established. This is due to two reasons. First, Professor Chamberlin deals only with one pair of variables at a time, and hence his analysis is incomplete. Secondly, he makes an implicit assumption concerning the elasticity of demand. Consider, for instance, the famous *DD* and *dd* curves.¹ The firm is assumed to choose a point on *dd* which maximises its profits. When, because of the action of other firms, demand is less than was anticipated, the firm further reduces prices. But that must mean that when the demand is less than anticipated, the marginal revenue curve of the new (lower) *dd* curve must exceed the (higher) marginal cost of the lower output. This can be so only if the elasticity of the new *dd* curve exceeds that of the old one, at the same price. This may or may not be a good assumption to make, but it should be made explicit. When such a change in elasticity is not postulated it is not easy to trace the equilibrating process on the Chamberlin diagram. We shall presently see how the relaxation of the constant elasticity postulate affects our own conclusion.

Since Chamberlinian competition of the kind under discussion is probably of limited significance, we will not endeavour to trace out the process when *W* in Fig. 3 lies on an upward-rising part of *MM*. In this case oscillating may or may not occur.

However, there is one point which deserves special mention. In (6) (ii) above we discussed the "multiple equilibrium" case. Firms finding themselves in this position may, in any practical situation, be justified in assuming that if they undertake the relatively large selling outlays implied by the second equilibrium point other firms will not follow suit. For once it has undertaken the venture other firms might easily find that there is no action on their part which will allow them to earn a profit. The "all or nothing" aspect of this case may be quite a potent cause of concentration. In any event, the step-by-step Chamberlinian process is quite inapplicable here, and in so far as (6) (ii) is a frequent situation, it strengthens the belief that his process must be rare indeed.

(10) From Professor Chamberlin, let us briefly turn to Mr. Harrod. Mr. Harrod has urged the view² that firms concerned with their long-run survival will tend to choose a price and output such that their profits do not attract newcomers. Geometrically, this is represented by the intersection of the average cost curve (including "normal" profit) with the long-run expected demand curves. We are not here concerned with the validity of the hypothesis as such, but rather with the problems it raises for the theory of selling costs.

¹ Chamberlin, *op. cit.*, Chapter 5.

² R. F. Harrod, "Theory of Imperfect Competition Revised," in *Economic Essays*, 1952.

If the hypothesis is accepted, there are two, not necessarily mutually exclusive, assumptions we can make to account for the presence of selling outlays:

(a) We may assume that, without selling outlays, there would be no output at which "normal" profits could be covered. That is the average cost curve including "normal" profits lies wholly outside and above the zero selling costs demand curve, or

(b) That selling costs increase the profit which can be earned without attracting new entrants or competitors.

There is not much one can say about assumption (a) except that it strikes one as extremely unlikely that it would account for the rather heavy selling costs which are incurred at present throughout the economy. Assumption (b) seems by far the more hopeful.

Once again our "multiple equilibrium" example is useful. By undertaking "large" selling outlays, the firm doing so will tend to raise the minimum selling outlay which can be undertaken by rivals hoping to survive. For by its action, the second equilibrium position (if it exists) of rival firms will entail even larger selling costs than before. True, we are not now supposing firms to maximise their profits in the old sense but to behave in a Harroddian manner. None the less, the point remains valid: Selling outlays introduce a certain lumpiness into competition and tend to raise the minimum size of new entrants. They are thus a kind of cushion against new competition and will very likely therefore raise the "normal" profit which can be earned in security.

However, once it is granted that the "normal" profit of a Harroddian firm is to some extent a parameter to be changed by the firm's own action, the door is re-opened for the more old-fashioned maximisation assumption. For as long as we assume that a firm is always concerned with the "long run" (is always a "sticker" in Professor Hicks' terminology¹) and that it can only earn a given "normal profit" in the long run, there is nothing left to maximise. But when selling costs are introduced the situation is changed to one of maximisation under constraint, the constraint being the relationship of long-run expected demand, on the one hand, and prices, selling costs and profits, on the other. This means that Mr. Harrod's important point can be rather more readily incorporated into traditional analysis.

III

(11) We shall now deal with the case where the elasticity of demand is not constant throughout.² The modifications are rather slight and concern mainly the construction of the *PS* and *MM* curves.

¹ J. R. Hicks, "The Process of Imperfect Competition," *Oxford Economic Papers* (New Series), Vol. VI, No. 1, February 1954.

² Everything said so far concerning the constant elasticity case applies *pari passu* to the case where selling outlays increase the elasticity of demand.

The easiest assumption to make is that the firm (in isolation) regards selling outlays as diminishing the elasticity of demand for its product within a certain range, but continues to regard the elasticity of demand as independent of the price it charges. Let us see how these new assumptions affect the *PS* and *MM* curves:

(a) The *PS* curve. Recall the argument in (3) above and once again consider a point such as Q' in Fig. 1. The slope of $x = 700$ at Q' measures the rate at which a given increase in selling costs can be transformed into a rise in price. But now, because of the increased selling costs and hence lower elasticity of demand, a given rise in price causes a smaller absolute reduction in demand than it did formerly. Hence $x = 700$ at Q' will be steeper than it would otherwise have been. Recalling the example in (3), a small increase in selling costs at Q' may allow prices to be raised by more than $\frac{9}{7}$ as much as they could have been raised at Q for a similar increase. Hence the optimum point for selling $x = 700$ may lie to the right of Q' . In other words, the *PS* curve may now have a positive slope.

But we have, quite reasonably, postulated that the effectiveness of selling costs in reducing the elasticity of demand will be diminishing (after a certain point) and hence the (positive) slope of *PS* will be diminishing and eventually *PS* will again have a negative slope. The curve is plotted in Fig. 4.

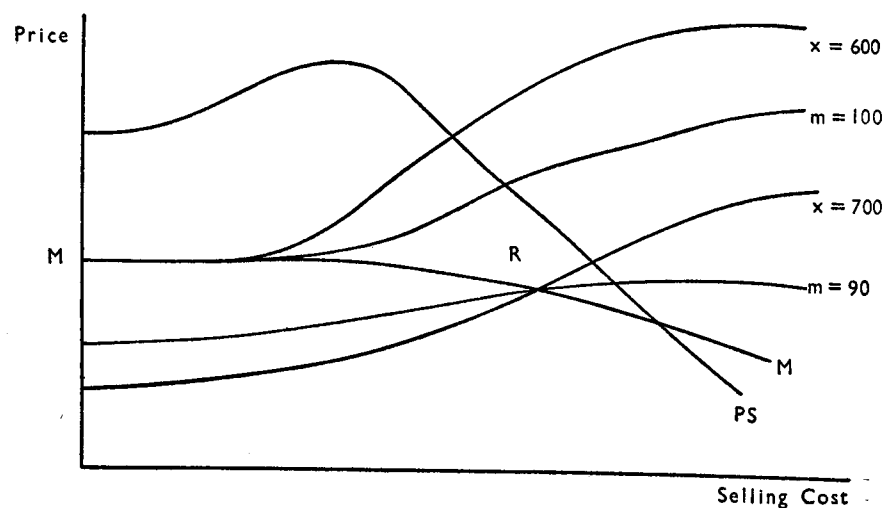


FIG. 4

(b) The *MM* curve. This is a little more difficult to construct than in the constant-elasticity case. Suppose that at an output of $x = 600$ marginal production costs ($= 100$) are equal to marginal gross revenue at zero selling costs. Now suppose selling costs to be raised. The

demand curve is shifted to the right and becomes less elastic. Recall the well-known formula relating marginal gross revenue to price of output. It is clear that the price at which, with positive selling costs, marginal gross revenue will equal marginal product costs of 100 will rise as the elasticity of demand declines. In Fig. 4 the curve $m = 100$ shows the combination of price and selling cost at which marginal gross revenue $= 100$. It has a positive slope, but this slope diminishes as the effectiveness of selling costs in lowering the elasticity of demand declines. We have drawn $m = 100$ as lying wholly below the constant sales curve $x = 600$. It is possible that it rises above this curve and intersects it at some positive selling outlay.¹ This case will not be discussed, since it occurs only when the effect of selling outlay on the elasticity of demand is very substantial indeed. But the reader can easily modify the analysis to take care of this contingency.

Now consider an output of $x = 700$. In drawing $m = 100$ we have assumed that the second-order conditions of a production maximum (marginal gross-revenue curve cutting marginal production-cost curve from above) are fulfilled at every point on this curve. Suppose that marginal production costs for 700 units $= 90$. Then the curve $m = 90$ showing the combination of price and selling costs associated with a marginal gross revenue of 90 must intersect the price axis above the intersection point of $x = 700$ with that axis. Suppose the contrary. Then at zero selling cost, marginal gross revenue is equal to marginal production cost at an output $x = 600$ and an output $x > 600$, which is impossible if the second-order conditions of a production maximum hold on $m = 100$. In other words, at an output of 700 with zero selling costs, the price at which marginal gross revenue is equal to marginal production cost of 700 units must exceed the price at which 700 units would be demanded.

The curve $m = 90$ intersects $x = 700$ at R . Proceeding in the same manner for higher and higher outputs, we obtain successive intersections of the appropriate m and x curves, and by joining these we obtain the *MM* curve we require. It should now be noted that *MM* may have a positive slope even though marginal production costs are falling. That this is so is obvious when we remember that selling costs now not only increase the amount which can be sold at any given price but also raise the "degree of monopoly."

(12) In (5) above we have discussed the second-order conditions of a true maximum. We shall here content ourselves with listing the main possible equilibrium positions:

(i) *PS* may intersect *MM* at a point where both curves have a positive slope. If this is to be a true maximum, *MM* must cut *PS* from below.

¹ See Appendix.

(ii) The intersection of the two curves may occur where PS has a negative and MM a positive slope. This is always a true maximum.

(iii) The intersection may occur where PS has a positive and MM a negative slope. This is never a true maximum.

(iv) There may be two intersections of the curves, one at zero and one at some positive selling costs which are both true maxima. This case we have already discussed in some detail.

(v) Lastly, the intersecting may occur where both curves have negative slopes. In this case, as we already know, a true maximum requires that the PS curve should cut the MM curve from above.

(13) We must now, briefly, note how our discussion of Chamberlinian competition is affected once we allow selling outlay to change the elasticity of demand. The main point of departure here is that the action of other firms will not only change the demand for the product of our particular firm, but also the elasticity of the demand curve facing it. Consider the situation discussed in (7) above and in Fig. 3. When other firms have increased their selling costs, demand is once again equal to $\bar{x}$ and the elasticity of demand is higher than it was expected to be. Hence we can no longer be certain that the MM curve will be shifted upwards even when marginal production costs are diminishing, for there are two opposing forces at work. The fact that demand is lower than expected, and hence marginal production costs higher, will tend to raise the price at which marginal gross revenue is equal to marginal production cost. On the other hand, the higher elasticity of demand will tend to lower that price. It is therefore possible that MM will shift downwards or not at all.

Similarly, the PS curve may now be shifted upwards to the right. This is simply due to the fact that the higher elasticity of demand (due to the increased selling outlays of other firms) will tend to make the constant-sales curves flatter than they would otherwise have been.

The result of all this is that after the action of other firms has reduced the demand and increased the elasticity of demand of our particular firm, this firm may further increase its selling costs and further lower price. In other words, we are now much closer to the traditional Chamberlinian picture. Equilibrium is reached when the opposing forces operating on the MM and PS curves just cancel out so that our firm undertakes no further change in selling outlays and prices, even though demand has turned out to be lower and the elasticity of demand higher than expected. The question of whether Chamberlinian competition is uni-directional or oscillatory is thus intimately linked to the effect of competition on the elasticity of demand of the firms partaking in it.

It would not be difficult to construct examples, now that the elasticity of demand has been taken from its strait-jacket, in which a Chamberlinian process never converged to any equilibrium at all. This does not seem worth

while, since, as already argued, that kind of competition must be rare anyway, and probably non-existent if the situation is such that no equilibrium can be reached.

IV

(14) One of a number of uses to which a theory of selling costs may be put is a more realistic analysis of the effects of some kinds of taxes and, of course, to the problem of taxing selling outlays themselves. This section will, rather briefly, examine a number of these problems.

Let us first consider the imposition of an *ad valorem* purchase tax levied on the final price. Assume, for the moment, that the firm is "isolated." We can usefully distinguish between a *substitution* and a *contraction* effect of the tax.

The imposition of the tax changes the optimum way of selling any given output. For a given rise in price will now add less to net revenue *after tax*, which is what our firm is interested in, than hitherto. The gains to be had from selling a given output at a higher price (and selling cost) are diminished. Formally, this means that the optimum (selling) point on any constant sales curve $\bar{x}$ is found, where the slope of that curve is equal to $\frac{1}{(1-t)\bar{x}}$, where t is the *ad valorem* rate of tax. Before the imposition of the tax the optimum was found at a slope equal to $\frac{1}{\bar{x}} < \frac{1}{(1-t)\bar{x}}$. Hence the optimum price and selling cost for any level of output is reduced, and this we call the substitution effect of the tax. Geometrically this means that the PS curve must always shift downwards to the left.

The contraction effect is due to the upward shift in the MM curve. After the imposition of the tax, marginal gross revenue minus marginal tax payment must equal marginal production cost, and so the price which brings about this equality for any given output rises. The contract effect will normally tend to raise prices, but in one instance it will reinforce the substitution effect in lowering prices. This occurs when the pre-tax equilibrium is at a point where both the MM and PS curves have positive slopes. The reader can easily check that in this case a downward shift in the PS and an upward shift in the MM curve will lead to an equilibrium at lower prices. The reason for this is simply that the reduction in output and selling outlay considerably raises the elasticity of demand (and this lowers optimal prices).

It is fairly clear that the presence of selling outlay increases the responsiveness of output to an imposition of purchase tax above what it would otherwise have been. Consider a pre-tax equilibrium such as W in Fig. 2. Suppose a tax to be imposed but selling costs unchanged. The MM curve is shifted upwards and will intersect some constant sales curve $\bar{x}$ (not drawn) vertically above W . This would give us the output and price after tax if selling costs were unchanged. But in fact they are reduced (since PS falls

as a result of the tax) and the intersection of the new *MM* and *PS* curve must occur at an output lower than $\bar{x}$. Thus the fact that selling costs are variable tends to increase the responsiveness of output to the tax.

We must now note that the above analysis probably overestimates the change in output and prices, for the firm is not isolated in practice. Its competitors will also adjust themselves to the tax, and in particular tend to cut their selling outlays. But when all firms act in this manner the reduction in the demand for any particular firm's output will be less than anticipated. From our discussion of Chamberlinian competition, we know that this will normally mean that the *MM* curve will shift up by less and the *PS* curve down by less than would otherwise have been the case. On the other hand, we cannot be certain of this. For the fact that all firms have cut their selling costs may mean that the elasticity of demand for the product of a particular firm is sufficiently smaller than anticipated to reinforce the original shift in the two curves (see (13)).

(15) Suppose that a tax on total selling cost at the rate T is imposed. This will mean that the best way of selling a given output $\bar{x}$ is found where the slope of the constant sales curve $\bar{x}$ is equal to $\frac{1+T}{\bar{x}}$. This is so because the imposition of the tax raises the "cost" of a given price rise for any output. Hence once again the *PS* curve will shift downwards and to the left. But the *MM* curve will not shift at all as long as the firm acts in isolation, since the conditions of production equilibrium are unaffected by the tax. It is easily seen that the result of the tax will normally be to lower output. It will raise prices if at the pre-tax equilibrium *MM* had a negative slope and lower prices if *MM* has a positive slope.

Now it is worthwhile to recall that, since the elasticity of demand depends on selling costs, that *MM* may have a positive slope even if marginal production costs are falling. It would be tempting to argue from this that a tax on selling costs would tend to lower prices more frequently than raise them. But this neglects the actions of other firms. Once again, when they have adjusted themselves, the elasticity of demand will tend to be less than it would otherwise have been, and hence the *MM* curve will tend to shift upwards for that reason. This means not only that prices will tend to rise when *MM* has a negative slope but may also rise if it has a positive one. It is very difficult to say anything *a priori* on the matter, since the final result depends on the magnitudes of opposing forces.

(16) Unfortunately, the above discussion of the two kinds of taxes is at best only a very first step to an understanding of their full impact. It does not enable us, for instance, to discuss at all adequately the consequences of substituting between two taxes. To do so, we would require a general equilibrium model which took explicit account of selling costs and other features of non-perfect competition. Such a model is not available, and we must content ourselves with a number of general remarks:

(i) Hardly anything is known of the effect of selling outlays on the aggregate demand for goods and services. It may well be that the "constant total demand curve" has a similar shape to the constant sales curves of our diagrams.¹ If it has, it must be emphasised that this will be reflected in the supply curve of effort and work of all kind. The plight of the American husband is well known. A tax on selling outlays may or may not lead to a reduction in the price level relevant to the decisions to work. But even if it does, the willingness to work may yet be reduced. Hence a tax on selling outlays may be found to be "inflationary" in the same way as other taxes have been found to have this effect.

(ii) As we have seen in the preceding paragraphs, the imposition of a purchase tax tends to reduce the selling outlay for any given output. It is thus a tax which encourages price competition relatively to selling-cost competition. This is perhaps not sufficiently appreciated. However, while a purchase tax directly affects the conditions of production equilibrium, and hence normally will lead to rising prices, a tax on sales cost does not. Moreover, both kinds of taxes will have similar effects to incentives. In times of inflationary pressure a selling-cost tax may have considerable advantages over further increases in purchase tax. For in such situations most firms will be producing on the upward-sloping section of their *MM* curves, and the effects of selling costs on demand elasticities is likely to be small. It is very unlikely that prices would be increased by the tax. On the other hand, selling costs would be lower. The resources thus saved may be by no means negligible.

(iii) There can be little doubt, and the analysis of the paper confirms this, that selling costs will tend to increase the degree of concentration of industry. By the same token, the scale of operation will be larger, and more advantage can be taken of increasing returns. This, as any economist worth his salt knows, is no argument in favour of selling costs. But we shall not discuss the welfare aspect of this matter, but instead note the interesting fact that a tax on selling costs may, paradoxically, increase, rather than decrease, the degree of concentration. The reason for this is not far to seek. When we say that selling costs tend to increase concentration we are comparing a position of zero selling costs with one where they have been (optimally) chosen. But a tax on selling costs affects marginal outlays, and only rarely will cause the complete abandonment of all selling efforts. It may now happen that the less-efficient and financially weaker firms find that after the imposition of the tax there is no price-selling-cost combination which will allow them to earn normal profit given the probable actions of their rivals. Indeed, in the case of Chamberlinian competition, the tax must always reduce the number of firms. But even in other cases,

¹ See K. W. Rothschild, "A Note on Advertising," *ECONOMIC JOURNAL*, March 1942, p. 112.

in so far as the tax stimulates marginal-price competition, it may well lead to the bankruptcy of smaller firms.

There are a host of other problems to be discussed and analysed, not least the role of selling costs in the dynamics of a capitalist economy. But our knowledge at present is quite insufficient for the task, and the above general remarks lack all rigour. It is to be hoped that one day we will be able to be as precise about the world as it actually is as we are now about the world as we should like it to be.

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MATHEMATICAL APPENDIX

(i) Notation. Let x = amount demanded, P = price and S = selling cost. Let $C(x) = C$ represent the relation between output and production cost. We write x_P, x_S for $\frac{\partial x}{\partial P}, \frac{\partial x}{\partial S}$ and x_{PS}, x_{PP} , etc., for $\frac{\partial^2 x}{\partial P \partial S}, \frac{\partial^2 x}{\partial P^2}$.

(ii) We wish to maximise

$$\pi = Px - C(x) - S \quad (1)$$

given the demand relation

$$x = x(P, S), [x_P < 0, x_S \geq 0] \quad (2)$$

And so

$$\left. \begin{aligned} x &= \mu x_P & (a) \\ P - C'(x) &= -\mu & (b) \\ -1 &= \mu x_S & (c) \end{aligned} \right\} \quad (3)$$

where μ is a Lagrangean multiplier. (3) may be rewritten equivalently as

$$\left. \begin{aligned} \frac{1}{x} &= -\frac{x_S}{x_P} & (a) \\ P + \frac{x}{x_P} &= C'(x) & (b) \end{aligned} \right\} \quad (4)$$

From (2) we see that the right-hand side of 4(a) measures the slope of our constant sales curves and 4(a) is the condition discussed in (2) of the text. Condition 4(b) simply states the equality between marginal gross revenue and marginal production costs. The reader can easily verify that when (4) is satisfied, then so necessarily are all the conditions of (3).

(iii) Let

$$v = \begin{bmatrix} 0 & -1 & x_P & x_S \\ -1 & C'' & -1 & 0 \\ \frac{x_P}{\mu} & -\frac{1}{\mu} & x_{PP} & x_{PS} \\ \frac{x_S}{\mu} & 0 & x_{SP} & x_{SS} \end{bmatrix} \quad (5)$$

Then for a true maximum V must be the discriminant of a negative definite quadratic form under constraint. In particular, it is easily verified that $V < 0$ (where $V = |v|$).

Now our PS curve is given from 4(a) by the equation

$$d \left[-x \frac{x_S}{x_P} \right] = 0 \quad (6)$$

And the MM curve is defined by

$$d \left[P + \frac{x}{x_P} \right] - dC'(x) = 0 \quad (7)$$

In the case discussed in Section I of the text, where the demand elasticity is constant throughout, we find the slope of (PS) from (6) to be

$$\left(\frac{dP}{dS} \right)_{PS} = \frac{-x \frac{x_{SS}}{x_P}}{x_S \left(1 + \frac{1}{e} \right)} \quad (6a)$$

where e is the given elasticity of demand. Since by the second-order conditions $x_{SS} < 0$ and $x_P < 0, x_S > 0$, it follows that PS has a negative slope ($|e| > 1$).

The slope of MM is found from (7) to be

$$\left(\frac{dP}{dS} \right)_M = \frac{x_S C''}{1 + \frac{1}{e} - C''_{xP}} \quad (7a)$$

Hence the slope of MM will be positive when marginal production costs are rising and normally negative when they are falling.

It is easily found by evaluating V that

$$\left(\frac{dP}{dS} \right)_{PS} - \left(\frac{dP}{dS} \right)_M = V < 0 \quad (8)$$

which is condition (5) of the text.

(iv) If selling costs affect the elasticity of demand in the manner described in the text, the construction of the MM curve requires the preliminary construction of the m curves. For any marginal production cost the equation of mm is

$$d \left[P + \frac{x}{x_P} \right] = 0 \quad (9)$$

From this we easily find the slope of the mm curve to be

$$\left(\frac{dP}{dS} \right)_m = -\frac{x_S}{x_P} \left[\frac{1 - \frac{x_{PS}}{x_P x_S} x}{1 + \frac{1}{e}} \right] \quad (10)$$

Both the nominator and denominator of the fraction in square brackets are positive, since $|e| > 1$ and an increase in selling costs reduces the elasticity of demand. (This can be checked by differentiating $e = \frac{x_P}{x} P$ with respect to S .)

In the text we assumed that the fraction in square brackets was less than unity. Differentiating e with respect to S , we find this assumption equivalent to

$$-\frac{x}{x_S} \frac{e_S}{e} - \frac{1}{e} < 1 \quad \left(e_S = \frac{\partial e}{\partial S} \right) \quad (11)$$

Hence, unless e_S is quite large in absolute value or e very small, the assumption is reasonable.

(v) When the elasticity of demand is allowed to vary, the slope of PS becomes

$$\left(\frac{dP}{dS} \right)_{PS} = \frac{-\frac{x_S}{x_P} \left[1 + \frac{x x_{SS}}{(x_S)^2} - \frac{x_{PS}}{x_S x_P} x \right]}{\left[\frac{x x_{SP}}{x_P x_S} + \frac{1}{e} \right]} \quad (6b)$$

When the elasticity of demand is constant this reduces to (6a), since then as is easily checked $\frac{x_{PS}x}{x_S x_P} = 1$. Since $x_{SS} < 0$ by the second-order conditions, the numerator of (6b) will diminish as x increases and eventually become negative, while the reverse will happen in the denominator. For as the effectiveness of selling cost in reducing the elasticity of demand declines $\frac{x x_{SP}}{x_P x_S}$ approaches unity. For low x and S , however, the fraction multiplying $-\frac{x_S}{x_P}$ may be positive.

Lastly, in this more general case, the slope of MM becomes

$$\left(\frac{dP}{dS}\right)_M = \frac{-\frac{x_S}{x_P} \left[1 - \frac{x_{PS}x}{x_P x_S} - C''x_P\right]}{1 + \frac{1}{e} - C''x_P} \quad (7b)$$

and it is obvious that this may be positive when $C'' < 0$.

(vi) With a purchase tax at rate t and selling-cost tax of rate T , (1) becomes

$$\pi = (1-t)Px - C(x) - S(1+T) \quad (1a)$$

And the condition (4) for a maximum now are

$$\frac{1+T}{(1-t)x} = -\frac{x_S}{x_P} \quad (a)$$

$$(1-t) \left[P + \frac{x}{x_P} \right] = C'(x) \quad (b) \quad (4a)$$

If V_{ij} is the co-factors of the element in the i^{th} row and j^{th} column of V , the

$$\frac{\partial P}{\partial t} = -P \frac{V_{23}}{V} + \left(-\frac{x}{\mu} \frac{V_{33}}{V} \right) \quad (12)$$

The second term of (12) is negative by the second-order conditions ($\mu > 0$), and we have called it the substitution effect. We easily find

$$V_{23} = \left[\frac{x_{SS}}{(x_S)^2} x - \frac{x_{PS}x}{x_S x_P} + 1 \right] \frac{x_S^2 x_P^2}{x^2} \quad (13)$$

The term in square brackets is the same as the square bracket terms in (6b). It is positive, V being negative, the "contraction term" is also negative. This is the case discussed in the text, where PS has a positive slope at the point of equilibrium. Lastly,

$$\frac{\partial P}{\partial T} = \frac{1}{\mu} \frac{V_{43}}{V} \quad (14)$$

and

$$V_{43} = \frac{x_S x_P}{x} \left[1 - \frac{x_{PS}x}{x_S x_P} - C''x_P \right] \quad (15)$$

The square bracket of (15) corresponds to the numerator of (7b). If it is positive then, since $x_S x_P < 0$, $V < 0$, $\mu > 0$, it follows that $\frac{\partial P}{\partial T} < 0$. That is the result in the text, where we found that a tax on selling costs will reduce prices provided MM has a positive slope.

THE POSSIBILITY OF WARRANTED GROWTH

I. INTRODUCTION AND SUMMARY

Is the growth of a free-enterprise economy regulated by effective demand, or by the supply and efficiency of its primary resources? In Harrod's terminology¹ the question falls into two parts: (i) Does the rate of growth of income tend towards the "warranted" rate, at which capital is increasing *pari passu* with capital requirements, or towards the "natural" rate made possible by the growth of population and technical knowledge? (ii) Is there a tendency for these two rates—natural and warranted—to approach each other in the long run?

To ask this question is not to ask whether there is a long-run tendency towards full employment of manpower in a growing economy. Full employment may be conjoined with warranted growth below the natural rate. The "unused" resources allowing demand-regulated growth would consist of a fund of as yet unapplied new techniques of production. This would imply that in the past the rate at which new technical discoveries have been made has exceeded the rate at which they have been drawn into use. In these circumstances, even though the employed population were stationary, output could expand more rapidly, if demand grew faster, so as to make it worth while to draw more rapidly on the accumulated fund of new techniques.

In a recent article² Kaldor relies upon the flexibility of profit margins to bring into ultimate coincidence the warranted and natural rates, so that ultimately supply conditions limit the rate of growth. But he further argues that, even before this coincidence is achieved, the actual rate of growth must be regulated by supply conditions rather than by demand. Accepting Harrod's thesis that the warranted rate of growth is surrounded by a field of centrifugal forces, he points out that this entails that, if growth is a persistent feature of the economy, it must be at a rate in excess of the warranted rate. (For if the actual rate is below the warranted rate, long-run growth is impossible; and growth at the warranted rate may be ruled out as an improbable fluke, in the absence of any forces tending to hold the economy on the warranted path.) But if the rate of growth is above the warranted rate it must increase until it meets the ceiling of supply limitations.

By combining his assumption of a tendency to equality of the natural

¹ R. F. Harrod, *Towards a Dynamic Economics*, Macmillan, 1948; "An Essay in Dynamic Theory," *Economic Essays*, Macmillan, 1952; "Notes on Trade Cycle Theory," *Economic Journal*, June 1951. These will be called respectively *Towards*, "Essay" and "Notes" in subsequent references.

² N. Kaldor, "A Model of Economic Growth," *Economic Journal*, December 1957.

and warranted rates with Harrod's centrifugal forces surrounding the latter, Kaldor achieves, in addition, a neat integration of trend and cycle theories. A fall in the rate of investment, due, for example, to a temporary fall in the rate of technical progress, may encounter a short-run rigidity of profit margins, and so carry the actual rate of growth of income below the warranted rate into the field of downward-spiralling forces.

The two foundation stones on which Kaldor's structure rests are, then, the long-run flexibility of profit margins and the instability surrounding the warranted path of advance.

The main purpose of the following discussion is to appraise the second of these, with its implication that long-run growth must be at the natural rate. My concern is not with the relation between the warranted and natural rates. I shall therefore assume, with Harrod, a constant proportion of income saved and desired ratio of capital to output, and ask whether, in these circumstances, there is an inherent tendency of the rate of growth of income to increase when it is above the warranted rate and to decline when it is below it, or whether, on reasonable assumptions, the warranted rate of growth may have the properties of a stable equilibrium of advance.

After a brief consideration of the meaning to be assigned to warranted growth (Section II), and an exposition of what I conceive to be Harrod's argument (Section III), it will be maintained that the instability theorem rests, not on the explicit assumptions made about entrepreneurial decision-making, but on an implicitly assumed lag between investment decisions and capital outlay; when this lag is removed, but the other assumptions are retained, the instability disappears. The possibility of demand-regulated growth therefore depends on the plausibility of allowing capital outlays to be affected by contemporary decisions, and it will be argued that to allow this is more convincing than to exclude it (Section IV). A model of stable demand-regulated growth is then developed, which meets a major objection which may be brought against the preliminary model presented in Section IV (Section V). It is shown that the same assumptions which secure stability in the simple aggregative system serve to secure it also when the economy is broken down into numerous industrial sectors (Section VI). Finally, it will be suggested that, in spite of the model's stability, its features are not without interest for the analysis of disturbance (Section VII).

II. THE MEANING OF WARRANTED GROWTH

Several commentators have pointed out that Harrod uses two definitions of the warranted rate of growth which are not equivalent. By one definition it is the rate which is necessary if the representative entrepreneur is to be content with the policy he is pursuing; by the other it is that rate which, if adhered to, will be maintained in the absence of fresh disturbances.

In the original version of his theory¹ the warranted rate satisfies both definitions, but in a later version² Harrod seems to cleave exclusively to the second meaning: the warranted rate is here simply a constant rate of growth. There is a growing shortage of capital experienced along the warranted path itself.

Nevertheless, centrifugal forces are supposed to surround this path. It is difficult to understand why they should. If there is a growing shortage of capital when output is advancing at the warranted rate there will be a growing shortage, too, if its rate of advance is slightly less than warranted. There is no cause here for retrenchment. If, on the other hand, the warranted rate were defined to involve growing capital redundancy, rates of advance slightly above the warranted path would not suffice to set off boom conditions. Unless the definition of the warranted path involves entrepreneurial satisfaction, the centrifugal forces cannot operate on both sides. The instability theorem therefore requires that the warranted path be defined as a path of advance on which plans are realised, and it is in this sense that the term will henceforth be used.

Is it *also* to be defined as a path of constant percentage growth? I do not think so. For constancy of the rate of growth at which plans are realised is merely a consequence of the simplifying assumptions that it is convenient to make as a first approximation. If the economy is divided into numerous sectors there is no reason why the path of advance which realises plans should also be one of constant proportionate growth in all outputs. But if centrifugal forces exist, they come into operation if and only if plans are going awry, whether or not they displace a regime of proportionate growth of all sectors.

III. THE INSTABILITY THEOREM

The operators $D \equiv d/dt$ and $G \equiv d \log/dt$ will be used. Thus Dx and Gx are respectively the absolute and percentage rates of change in x , and DGx is the absolute rate of change in Gx .

Before coming to special assumptions it is convenient to set out some relationships which hold through most of the subsequent analysis. Let x and y denote respectively capital and net income (or output) measured in terms of money of constant purchasing power or a *numeraire* good, relative prices and interest rates being assumed constant. Let c be the desired ratio of capital to output, and s be the proportion of income saved, both positive constants. (Capital requirements not associated with current activity, and therefore giving rise to "autonomous" capital outlays, will be assumed absent.) Total capital requirements are cy . Define a shortage of capital (v) as the excess of capital requirements over capital, so that

$$(1) \quad v = cy - x$$

¹ *E.g.*, *Towards*, pp. 81-2.

² *E.g.*, "Notes," pp. 274-5.

Of course, $v < 0$ means capital redundancy. Differentiating (1) with respect to time, we obtain the rate of change of capital shortage,

$$(2) \quad Dv = cDy - Dx$$

cDy , marginal capital requirements, is what Harrod calls "justified investment."¹

Capital accumulation is the same as saving. Therefore

$$(3) \quad Dx = sy, \text{ or } Gx = sy/x$$

This relationship permits the expression of v in terms of the percentage growth of capital (Gx), and of Dv in terms of the percentage change in income:

$$(4) \quad v/x = \frac{Gx - s/c}{s/c} \quad \text{and}$$

$$(5) \quad Dv/y = cGy - s$$

Evidently s/c has the characteristics of a warranted rate of growth. For only if both capital and income are growing at this rate can capital requirements be continuously fulfilled. If capital grows faster than this it is in short supply, and if it grows more slowly it is redundant. If income grows faster than s/c there is either a growing shortage or a declining redundancy of capital; if it grows more slowly there is either a growing redundancy or a declining shortage of capital.

Growth of income and capital at the warranted rate is necessary for equilibrium, but it need not be sufficient. A further necessary, and potentially independent, condition is that demand for goods should be continuously equal to production (net output). Let u stand for the excess of demand over production, and i for capital outlay (planned investment). Then

$$(6) \quad u = i - sy$$

and equilibrium growth requires that $i/y = s$ and that $Gi = Gy$ (i.e., that $u = 0 = Du$).

Harrod assumes that an entrepreneur in a growing economy bases his induced investment decisions on: (i) the relation of his stocks and equipment to the requirements of his current productive activity, and (ii) the recent trend of production. He will order more: (i) "the shorter are his stock and equipment in relation to current requirements," and (ii) "the greater has been the upward trend of his recent put-through."² Harrod suggests that his diagnosis of instability rests on these two assumptions.²

In *Towards* (pp. 81-6) the two assumptions are combined in the following way. Suppose we start from a situation in which capital is adjusted

¹ Strictly speaking cDy is to be identified with "justified investment" only if $v = 0$. For an elucidation of this point see the latter part of the present section.

² "Notes," p. 273.

to total requirements, i.e., $v(0) = 0$. If at the same time $Dv = 0$, capital requirements are growing in step with capital accumulation. In these circumstances entrepreneurs are content with the rate at which their capital outlays are growing, and it is assumed that they will, unless prevented by physical limitations, maintain a constant rate of growth of i , increasing it in the succeeding period in the same proportion as it has just been increased. But if $Dv > 0$, capital requirements are outstripping accumulations, and it is assumed that entrepreneurs will attempt to meet the incipient shortage by increasing capital outlays more than in proportion to the previous increase. Likewise, if $Dv < 0$, there is an incipient redundancy of capital, persuading entrepreneurs to increase their outlays less than in proportion to the last increase.¹

This amounts to assuming $DGi \geq 0$ according as $Dv \geq 0$. But it is more convenient to connect DGi with Dv/y than with Dv itself, because it is then possible to obtain a model which is linear in the rate of growth. Therefore the above assumptions about capital outlay may be expressed in the form:

$$(7) \quad DGi = \kappa Dv/y$$

where κ is a positive constant. By substitution from (5)

$$(8) \quad DGi = \kappa(cGy - s)$$

Apparently Harrod conceives long-period development to proceed as a continuous succession of short-period Keynesian equilibria. At each point of time demand equals supply, and so income is determined by the investment multiplier. Thus the condition $u = 0$ is continuously fulfilled, and consequently $sy = i$, $Gy = Gi$ and $DGy = DGi$. Substituting this last identity in (8), we obtain

$$(9) \quad DGy = \kappa(cGy - s)$$

which is a first-order linear differential equation, with constant coefficients, in the variable Gy . Its solution is

$$Gy = s/c + \{Gy(0) - s/c\} e^{\kappa c t}$$

where $Gy(0)$ is the initial rate of growth of income. If income is initially growing at the warranted rate, s/c , it will continue to do so. But if it is initially growing faster than this, its rate of growth will continuously increase, and capital shortage will grow; and if it is initially growing more slowly, its rate of growth will continuously decline, capital redundancy will grow and income will soon begin to decline.²

¹ "Notes," pp. 271-2. Also "Supplement on Dynamic Theory," *Economic Essays*, pp. 282-4.

² Certain ambiguities in Harrod's exposition, especially in the "Essay," have led most commentators to a rather different version of the instability theorem from the one given here. The economy is not assumed to develop with continuous equality between demand and supply. On the contrary, the primary source of instability is in the effect of excess demand or supply on production decisions, not in the effect of growing capital shortage or redundancy on investment

As it stands, the model does not answer all the questions one would like to pose it. It has been constructed on the supposition that $v(0) = 0$. But suppose $Gy(0) = s/c$ and $v(0) \neq 0$. Would it be reasonable to assume that the economy continues to grow at the warranted rate? Growth at the warranted rate is necessary for equilibrium, but is it sufficient? Again, suppose, for example, that $Gy(0) > s/c$, but $v(0) < 0$, *i.e.*, that there is, initially, diminishing capital redundancy. The situation could arise if the government were to succeed in encouraging private consumption sufficiently to push the warranted rate below the actual rate of growth, soon after the appearance of recession but before income had begun to fall. In view of the continuance of capital redundancy, it is unlikely that this would be enough to turn the recession into a boom.

Harrod is, of course, aware of the difficulty about v . He proposes to deal with it by distinguishing the marginal from the average capital coefficient. The two are equal only if $v = 0$. When $v > 0$ the marginal coefficient, c' , stands above the average, c , and justified investment is $c'Dy > cDy$. When v is < 0 , $c' < c$, and so justified investment is less than cDy . The warranted growth-rate is s/c' , and is consequently abnormally high in a depression and low in a boom.¹ Presumably, therefore, one would have to argue that the Government's success in reducing s would be counterbalanced by the reduction in c' .

Nevertheless, the possibility remains of a reduction in s of sufficient magnitude to bring about the situation envisaged above, in spite of the

decisions. Entrepreneurs quicken or retard the rate of growth of their output according as excess demand is positive or negative. Thus

$$(a) \quad DGy = \epsilon u/y$$

where ϵ is a positive constant. From (6),

$$(b) \quad u/y = i/y - s$$

Investment decisions are in turn identified with Harrod's justified investment, *i.e.*,

$$(c) \quad i = cDy$$

Combination of (a), (b) and (c) yields an equation in Gy of exactly the same form as (9). The superficial resemblance of the two versions, and the possibility of confusion, is increased by the fact that, from (b) and (c), $u = Dv$, so that the warranted rate of this alternative version is not only such that demand equals supply, but also necessary to the continuous equality of capital to requirements.

Harrod's clarification of his position in his more recent writings leaves little doubt but that the version of the text is the closer to his thought. He rejects the notion that he intends a divergence between income and expenditure ("Notes," p. 270), or that justified investment is the same as intended investment (*ibid.*). And from "Notes," p. 273, it is clear that investment orders are the source of instability.

Considered on its own merits, the alternative version is not a very convincing argument for instability. Baumol's criticism of equation (a) (*Economic Dynamics*, pp. 53-4) appears to be a sound one. It makes entrepreneurs unreasonably optimistic or pessimistic. An excess of supply over demand, for example, is not a sufficient reason to cut back the growth of production if your sales are expanding rapidly. Equation (c) is, moreover, quite inadequate as a hypothesis about capital outlays. It allows capital shortage (or redundancy) to bear on investment decisions only through its prior effect on the course of production.

¹ "Essay," p. 267, and *Towards*, pp. 83-4.

tendency of c' to sag, and we are left with the implication that entrepreneurs could be stimulated to expansion merely by experiencing a taking-up of some of the slack in their productive capacity.

However, one does not wish to make too much of this point, particularly as Harrod has indicated¹ that he does not regard his instability theorem as depending on the special assumption about entrepreneurs' behaviour expressed by (7).

IV. THE CAUSE OF INSTABILITY

I mentioned in the last section Harrod's ascription of his centrifugal forces to the dependence of investment decisions on the recent trend of output and the surplus or deficiency of capital. It will now be shown that this dependence is not destabilising on its own, but becomes so when it is made to operate in conjunction with a time interval, implicit in Harrod's formulation of his theory, between investment decisions and capital outlays. It is therefore not the case that the instability "has nothing to do with the effect of lags."²

It is easy to see that a lag is involved in (8). It means that the entrepreneur decides to-day what his future capital outlay is to be, by comparing the current increment in requirements with to-day's capital outlay. Similarly, to-day's outlay was determined by decisions taken, and orders placed, in the past. It cannot be modified in the light of the current out-turn of events—that can bear only on to-day's orders, future investment.

But let it be assumed, on the contrary, that current capital outlay is influenced by current events. If there is no shortage or redundancy of capital (*i.e.*, if $v = 0$) outlay will be planned to equal the increment in requirements prognosticated for the immediate future, the prognostication being made by projecting into the future the rate of growth of the firm's output undertaken in the immediate past. If there is a shortage of capital, planned outlays will be larger than this, in proportion to the extent of the deficiency; and if capital is surplus to requirements, planned outlays will be smaller, in proportion to the redundancy. These assumptions are incorporated in the following relation, to replace (7).

$$(10) \quad i = cDy + \kappa v$$

where v is defined as in (1), and κ is a positive constant. Maintaining the assumption that $sy = i$, we have

$$cDy - sy + \kappa v = 0$$

$$\text{But by (2) and (3)} \quad cDy - sy = Dv$$

$$\text{Therefore (11)} \quad (D + \kappa)v = 0$$

¹ *E.g.*, "Notes," pp. 271-3.

² *Towards*, p. 86. Cf. also "Notes," p. 262-4.

Since supply is always equal to demand, the only condition of equilibrium is that capital should be continuously equal to requirements. From (11) it is apparent that $v = 0$ entails $Dv = 0$: hence if the economy is on its equilibrium path, it will stay there in the absence of external disturbance.

$Dv = 0$ is both necessary and sufficient for equilibrium. But, by (5), $Dv = 0$ if, and only if, $Gy = s/c$. It follows that growth of income at the warranted rate is both necessary and sufficient for the continuous fulfilment of capital requirements.

All this is very similar to Harrod's theory. The difference is in the system's motion when it is initially growing at an unwarranted rate. If $v \neq 0$, (11) may be written

$$Gv \equiv Dv/v = -\kappa < 0$$

Hence $Dv \geq 0$ according as $v \leq 0$. If v is positive it is decreasing, and if it is negative it is becoming less so. There is an inherent tendency for the shortage or surplus of capital to be eliminated. This, of course, also means that, if growth is initially unwarranted, it tends to approach the warranted rate as time goes on. The forces at work on either side of the warranted path are centripetal.

Since $Gy \geq s/c$ according as $Dv \geq 0$, it follows that $Gy \geq s/c$ according as $v \leq 0$. Here is, perhaps, the most striking contrast with Harrod's model. A rate of growth above the warranted rate indicates an excess of capital, and a rate below it indicates a deficiency. This is because an excess of capital, for example, persuades entrepreneurs to keep investment (and therefore y) low in relation to the growth of their requirements (as determined by Dy), and the consequence is that Gy ($\equiv Dy/y$) is abnormally high. A higher-than-warranted rate of growth is the consequence of steps taken with the intention, and effect, of eliminating redundancy, whereas in Harrod's theory it is a sign of growing shortage, and a cause of steps taken with the intention of reducing this, but with the effect of magnifying it.

The upshot of the foregoing analysis is that, if the instability theorem is to be upheld as displaying an inherent property of development unlimited by scarcity of primary resources, current capital outlay must be wholly determined by decisions taken in the past. Wholly determined, because one can allow past decisions to play a part, together with current ones, without upsetting the stability of growth. Indeed, they do play their part in (10), which gives expression to the fact that investment projects take time to complete.

What has to be shown, therefore, is that there is necessarily a significant interval between taking a decision and starting the outlay (or series of outlays) it gives rise to. The interval might occur either between the decision itself and the placing of orders, or between the placing of orders and the beginning of the outlay.

In the latter case what must be envisaged is a lapse of time between the order and the commencement of work upon it. For otherwise there is no interval. If work begins at once, the outlay has *ipso facto* begun. The question of the time of payment is irrelevant here. Whether payment is in advance or on delivery is purely a matter of finance, of who seeks credit in the first instance, the manufacturer of the equipment or its prospective user.

I can think of no reason why a delay *must* elapse between receiving an order and beginning work upon it. It may, of course. But, if it does, that is a sign that demand exceeds supply. To assume an interval between orders and the beginning of construction is equivalent to assuming a production lag. If I have interpreted him correctly, Harrod makes no such assumption. But even if it were to be made, the result would not necessarily be instability. This will be shown in the next section.

There remains the possibility that the interval is between the investment decision and the placing of orders. Presumably this would mean a significant lapse of time, after the completion of the plans and before the issue of instructions, during which no external event would be allowed to modify those plans in any way. For the decision cannot be said to be complete while its only manifestation is a still plastic structure of intentions. It is hard to believe that such a rigidity of procedure is at all common in the world of business.

In his book *The Trade Cycle*¹ Harrod put in a central position "the interval that elapses between placing an order for, or beginning to undertake the construction of, capital goods and their use in the productive process."² This could be interpreted to mean not merely that current investment is partly dependent on past decisions, but that current deliveries and installations of new pieces of equipment are wholly the result of previously given orders. Does this admittedly reasonable proposition provide support for the instability theorem? I do not think so. The assumption on which the theorem actually relies is that current decisions for future investment are taken by comparing current (marginal) requirements with actual accruals of capital, not with new installations only. And there is a good reason for not replacing "accruals" by "installations" here, which Harrod himself recognises: "The rise or fall of goods in the pipeline above or below the required level may be just as big a factor as the margins of unwanted equipment or shortage of equipment in depressing or stimulating the system."³ Growth of markets will have a less encouraging effect on future investment the greater the rate of increase of equipment under construction, not only the greater the flow of finished equipment into use. While, therefore, the lag in question would provide a formal basis for the theorem, its use in this context would be unsound.

It must be concluded that the assumption on which the instability theorem depends lacks justification.

¹ Oxford, 1936.

² *Op. cit.* p. 88.

³ *Towards*, p. 85.

V. A MODEL OF STABLE GROWTH

There is, however, a legitimate objection to be brought against the argument of the last section. It is that the very assumption that secures the stability of warranted growth makes the economy incapable of proceeding along the path of development assigned to it there.

That path involved the continuous equality of demand and supply. As in Harrod's theory, the economy was supposed to be in short-run Keynesian equilibrium all the time, whether it was in long-run equilibrium or not. But this is reasonable only if the stability conditions of short-run equilibrium are met.

Harrod's system satisfies the requirement of short-run stability, notwithstanding its centrifugal forces operating in the long run.¹ Capital outlay is a datum in the period of its occurrence, having been determined by decisions taken in the past. The condition of short-run stability is therefore only that the marginal propensity to save (s) be positive.

But if capital outlay depends on current income it is highly unlikely that the condition for short-run stability will be met. It is impossible to say precisely what this condition is without constructing an explicit model of short-run equilibrium, but it seems clear that it will require s/c to exceed κ . But κ , the proportion of capital deficiencies which firms are currently planning to make good, may well be as large as 50%, while s/c is unlikely to exceed one-tenth of this. Has one to conclude that demand-regulated growth is, after all, impossible, on the ground that there is bound to be instability, if not of long-run, then of short-run equilibrium?

Fortunately the implication is not inescapable. Long-run stability can be preserved if one is prepared to sacrifice the notion of a short-run equilibrium developing under the pressure of long-period forces. This line of escape does, in any case, commend itself, by promising to compensate with its greater realism for what it loses of simplicity. Short-run adjustments are not sealed off from the interference of the longer-run forces. Their separation will continue to be a useful approximation for some purposes, but it cannot be regarded as a canon of procedure in the analysis of growth.

If discrepancies between demand and supply are allowed, an assumption must be made about the formulation of production plans and their dependence on excess demand and supply. It will be assumed that, in the absence of excess demand or supply, entrepreneurs plan to increase (reduce) their production in the immediate future in step with the rate at which they expect their demand to expand (contract), their prognosis of the rate of change of demand being made by projecting into the future the trend of demand experienced in the immediate past. If there is excess

¹ The contrary opinion expressed by some (e.g., H. Brems, "Stability and Growth," *Economic Journal*, December 1955, pp. 612-25) appears to be due to an imperfect appreciation of Harrod's position.

demand, however, production will be planned to increase faster (or decline more slowly) than this; and if there is excess supply, production will be planned to increase less rapidly (or decline faster), in proportion to the extent of the excess. In comparison with the assumption of the "alternative version" of Harrod's theory previously referred to,¹ this avoids giving undue weight to excess demand or supply, by assigning to the trend of demand an independent influence on production plans.

In symbols this is interpreted by the relation:

$$(12) \quad Dy = \epsilon u + Dz$$

where Dz is the rate of change of demand (z), and ϵ is a positive constant. Demand is the sum of consumption and capital outlays, *i.e.*,

$$(13) \quad z = i + (1 - s)y$$

The complete system is specified by adjoining to these the equations (1), (3), (6) and (10).

Since $Du = Dz - Dy$, it follows immediately from (12) that

$$(14) \quad (D + \epsilon)u = 0$$

Also from (2), (3), (6) and (10) it is easy to derive the relation

$$(15) \quad (D + \kappa)v = u$$

There are now two independent conditions of equilibrium, that supply equals demand and that capital equals requirements (*i.e.*, that $u = v = 0$). From (14) and (15) it is evident that $u = v = 0$ entails $Du = Dv = 0$. This means that, once on its equilibrium path, the economy will continue in equilibrium unless dislodged by an external force.

Neither $u = 0$ nor $v = 0$ is alone sufficient for equilibrium. But, from (15), it is both necessary and sufficient that capital should be continuously equal to requirements, *i.e.*, that $v = Dv = 0$. Consequently, referring back to (4) and (5), we observe that equilibrium growth is achieved if, and only if, both income and capital are growing at the warranted rate, s/c , neither of these conditions being enough on its own.

The question of stability is rather more complicated than it was when demand and supply were assumed to be in continuous balance. It is obviously necessary that both u and v should tend to zero as time goes on. But, for reasons to be given presently, this is not enough. First, however, it will be shown that u and v do in fact tend to disappear.

From (14) the solution for u follows at once:

$$u = u(0)e^{-\epsilon t}$$

and therefore there will be a constant tendency towards the elimination of excess demand or supply. Since (14) was derived directly from (12), the action which entrepreneurs are assumed to take, in order to bring

¹ P. 317 above, footnote 2.

their production into line with the state of their markets, tends to achieve its aim no matter what factors determine demand conditions.

If (15) is differentiated with respect to time, and u and Du are eliminated from (14) and (15), we obtain:

$$(D + \kappa)(D + \varepsilon)v = 0$$

whence

$$v = V_1 e^{-\kappa t} + V_2 e^{-\varepsilon t}$$

in which V_1 and V_2 are constants depending on initial conditions. Since the exponents of both terms are negative, v must tend to disappear in the long run.

Nevertheless, instability is possible. For the disappearance of u and v may occur only after income and capital have become negative. In economic terms this is to say that u and v may fail to be eliminated before income falls to zero and the system breaks down. The solution in y takes the form:

$$y = Y_1 e^{s_1 t} + Y_2 e^{-\kappa t} + Y_3 e^{-\varepsilon t}$$

and if y is to be positive in equilibrium we must have $Y_1 > 0$. This condition places a restriction on the initial rate of decline of income. It is met if, and only if,

$$(16) \quad Gy(0) > -\{\varepsilon + \kappa + \varepsilon\kappa/Gx(0)\}$$

Thus a too precipitate initial fall in income could, theoretically, plunge the economy into a downward movement from which it could be rescued only by the failure of one or more of the assumptions of the model.

In practice, however, the possibility may be disregarded. The constant ε is likely to be considerably greater than 100%. The critical rate of decline of income is therefore too large to constitute a danger point.

VI. THE STABILITY OF INTER-INDUSTRIAL DYNAMIC EQUILIBRIUM

No allowance has yet been made for the varying composition of the national output and the national capital, or for the inter-relations between industries. This must now be done, and it will be shown that, if entrepreneurs' behaviour follows the pattern assumed hitherto, the same centripetal tendencies are to be expected.

(a) Gross and Final Product

If one wishes to include inter-industrial transactions on current as well as on capital account, gross and final product must be distinguished. The final product of industry J (y_j) is its gross product (p_j) less the absorptions as current input by all industrial sectors, including J itself. It is assumed that industry K's current outlay on J's product is in constant ratio (b_{jk}) to K's own gross product. Thus

$$(17) \quad y_j = p_j - \sum_k b_{jk} p_k$$

where each b_{jk} is ≥ 0 .

(b) Excess Demand and Capital Shortage

The term "supply" will be taken, in this section, to stand for gross product. Hence demand (z) will also be defined gross, as inclusive of inter-industrial current outlays. The excess demand for J's product is therefore

$$(18) \quad u_j = z_j - p_j$$

Each product will be regarded as potentially usable both for immediate consumption and as a capital item. In general, therefore, each industry accumulates stocks both of its own product and of the products of other industries. K's capital requirement of J's product is assumed to be in constant ratio (c_{jk}) to K's own gross product. K's shortage of stocks or equipment of type J (v_{jk}) is thus measured by the excess of that requirement over K's actual stocks of J's product (x_{jk}):

$$(19) \quad v_{jk} = c_{jk} p_k - x_{jk}$$

where $c_{jk} \geq 0$ and not all $c_{jk} = 0$.

(c) Demand

The gross demand for J's product consists, in general, of three parts, consumption outlays, current industrial outlays and net capital outlays. Consumption outlay is assumed to be a constant proportion (a_j) of aggregate net income ($\sum_k y_k$). Capital outlay is the sum of the net accumulations of J-product planned by each industry, K's planned accumulation of J-product being denoted by i_{jk} . Hence

$$(20) \quad z_j = a_j \sum_k y_k + \sum_k b_{jk} p_k + \sum_k i_{jk}$$

where $a_j \geq 0$ and $0 < \sum_j a_j < 1$.

(d) Production

The production plans of entrepreneurs in each industry are assumed to be based on the relation between the demand for, and supply of, its product and the recent trend of that demand, after the manner of (12) above, except that the decisions are in regard to gross, not net, product. So

$$(21) \quad Dp_j = \varepsilon_j u_j + Dz_j$$

where the ε_j are positive constants.

(e) Capital Accumulation

The capital outlay of industry K on J's product is, in conformity with the assumptions expressed by (10),

$$(22) \quad i_{jk} = c_{jk} Dp_k + \kappa_{jk} v_{jk}$$

the κ_{jk} being positive constants.

If there is excess demand or supply, not all plans for capital accumulation can come off. It is assumed that each industry holds buffer stocks of its own product, so that the capital outlays of other industries on J-product are realised, but J's accumulation of its own product will fall short of (exceed) what is planned by the extent of the excess demand (supply). Thus

$$(23) \quad Dx_{jk} = i_{jk}, j \neq k$$

$$(24) \quad Dx_{jj} = i_{jj} - u_j$$

The total accumulation of J's product by all industry will then equal, as it should, the unconsumed part of J's final output, and total accumulation in the system as a whole will consequently equal net saving, the unconsumed part of aggregate net output.

(f) *Dynamic Equilibrium*

The above equations define the dynamic motion of the economy, whether it is in equilibrium or not. In equilibrium, however, each v_{jk} and u_j is zero, and the warranted path is therefore defined by the equations

$$y_j = p_j - \sum_k b_{jk} p_k$$

$$p_j = a_j \sum_k y_k + \sum_k b_{jk} p_k + \sum_k i_{jk}$$

$$i_{jk} = c_{jk} D p_k$$

This is a variant of Leontief's closed dynamic system.¹ Study of its nature, and of the assumptions required to make the equilibrium reasonably well-behaved, is too complex a matter to be undertaken here. There is, of course, no reason why aggregate net income should grow at a constant geometric rate along the warranted path.

(g) *Stability*

From (18) and (21) we have at once that

$$(25) \quad (D + \epsilon_j) u_j = 0$$

Also by (19), (22) and (23) that

$$(26) \quad (D + \kappa_{jk}) v_{jk} = 0 \quad \text{for } j \neq k$$

Again by (18), (19), (22) and (24) we have

$$(27) \quad (D + \kappa_{jj}) v_{jj} = u_j$$

It is unnecessary to discuss the solutions of these equations in detail, because they are exactly analogous to (14), (11) and (15) above. It is at once apparent that each u_j and v_{jk} must tend to zero as time goes on, and con-

¹ W. Leontief and others, *Studies in the Structure of the American Economy* (New York, 1953), Chapter 3.

sequently that the motion of the system is ultimately towards the warranted path.

VII. TREND AND DISTURBANCE

Having the assurance that industrial inter-relationships are not essentially destabilising, one may return to the aggregative analysis in which such complications are set aside. In this last section some further consequences of that analysis will be explored.

(a) *Deviations from Warranted Growth*

It was shown in Section IV that, when capital outlay is partly determined by contemporaneous decisions and supply is continuously equal to demand, a rate of growth above the warranted rate is a sign of capital redundancy and a rate below it is a sign of deficiency, these results being in strong contrast with the features of Harrod's theory. However, when excess demand or supply are allowed into the picture, the contrast becomes less vivid. Growth below the warranted rate, for example, may be a sign either of declining capital shortage, as before, or of growing redundancy. In the latter event it will be characterised by the presence of excess supply. Thus, as in Harrod's theory, the economy may be moving away from a balance between capital and requirements. The stability of the system ensures, not that it is always tending to eliminate shortage or redundancy, but that it will eventually do so.

(b) *Recovery from the Slump*

Both Harrod¹ and Kaldor² rely on the fact that gross investment in fixed capital cannot become negative to provide a "floor" beneath which output cannot sink, and on the recovery of replacement expenditures, after capital erosion has proceeded some way, to give the impetus to a resumption of growth.

If the surplus of capital is to be reduced by capital consumption, net saving must become negative in the course of the slump. Although this is quite possible, it does not seem to me to be a factor on which great reliance should be placed. In the first place it is not certain that dissaving will occur. Secondly, its magnitude is not likely to be such as to make serious inroads into the capital surplus except after a very long period of depression.

It might, however, be held that the recovery of replacement outlays depends not so much on the occurrence of aggregate net disinvestment as on disinvestment in fixed capital. But it is doubtful if this will be enough to reduce the level of planned disinvestment in the economy as a whole. If net saving is still positive, the accumulation of unsold stocks will be an offset to the stimulus provided by plant-scraping, and it is not clear why

¹ *Towards*, p. 91.

² *Op. cit.*, p. 623.

the latter should be the more powerful. It appears, therefore, that aggregate dissaving is a necessary precursor of this type of revival.¹

Perhaps aggregate dissaving and the dismantling of fixed equipment may together constitute a sufficient encouragement to capital outlay. However that may be, it is certainly safer not to have to depend on the effectiveness of this conjunction.

If growth is stable, a revival of activity will take place even though no aggregate consumption of capital precedes it. In a slump capital is surplus to requirements, income is falling and there is excess supply. But because entrepreneurs succeed in cutting output more rapidly than demand is shrinking, excess supply is reduced. There will therefore be a tendency for output to decline less rapidly, and this factor will eventually impart an upward turn to capital outlay and demand, in spite of the continuing growth of redundancy. (For while the growth of redundancy depresses planned investment, the deceleration of output encourages it, and in time this latter influence must become the more powerful.) This in turn will lead to a resumption of the upward trend of output. The growth of output having reached and surpassed the warranted rate, capital redundancy declines.

(c) *The Paradox of Saving*

The instability theorem may be regarded as a development of the Keynesian paradox of saving, lifting it from the confines of static theory into the wider domain of dynamics. An increase in the supply of saving (the proportion of income saved) or a decrease in the demand for it (capital required per unit of output) raises the rate of growth of income warranted by economic conditions, but does nothing, in itself, towards raising the actual rate of growth. If the result is to push the warranted rate above the actual rate of growth, the change is positively inimical to the continuance of advance.

Rejection of the theorem is in one sense a rejection, also, of the extension of the Keynesian paradox to the dynamic sphere. Stability of growth entails that an increase in s or a decrease in c not only raises the warranted rate but also brings into play forces which tend to raise the actual path of growth to the higher rate. In the long run capital accumulation is favourable to growth of income.

But there is another sense in which the paradox is retained. The short-run effect of an increased propensity to save or a reduced capital coefficient will certainly be to weaken, and may be to disrupt, the progress of the economy. The paradox is transmuted into a contradiction between

¹ Kaldor also suggests (*loc. cit.*) that revival will be facilitated by the cumulative effect of the accrual of unexploited new inventions. But this factor could hardly operate on its own. So long as there is a growing superfluity of equipment and a surplus of manpower the economic climate is unfavourable to the exploitation of new capital projects.

the short- and long-run effects of increased saving and capital productivity.

However, the method whereby the above conclusion in regard to short-run effects has been arrived at deserves separate discussion. For it is not readily deducible from the properties of the model developed in previous sections. That model was designed to display the characteristics of continuous development from an arbitrarily selected initial situation. The short-run problem, on the other hand, is that of determining the initial situation itself, after an abrupt change in one or both of the parameters s and c , given the magnitude of that change and the situation on which it supervened.

Let us concentrate on the effect of a change in s , since no difference of principle is involved if c changes. Expressing the system in terms of x, y, z and their derivatives, we have

$$(28) \quad z = cDy + \kappa(cy - x) + (1 - s)y$$

$$(29) \quad Dy = \varepsilon(z - y) + Dz$$

$$(3) \quad Dx = sy$$

An abrupt change in s constitutes, via (28), a discontinuous jump in demand (z). On the other hand, there is no reason why it should cause any discontinuity in either x or y , which are historically determined by decisions taken prior to the jump in s . Now the equation of y 's motion being of the third order, three initial values are required to start the system off from the point of time at which the break occurs, $t = 0$, say. Two of them, $x(0)$ and $y(0)$, are historical data, and it therefore remains to determine one more.

One thing is clear. The required initial value cannot be the value of z obtained by putting the new value of s in (28) and leaving not only x and y as they were before the jump, but also Dy . The economic logic of (29) is that Dy is causally dependent on z . A discontinuity in z must therefore entail a discontinuity in Dy . Thus if $Dy(-0)$ and $Dy(+0)$ denote the derivative of y at $t = 0$ before and after the break respectively (or the left-hand and right-hand derivatives of y at $t = 0$), $Dy(+0)$ is to be found.

For this purpose a distinction must be made between the meaning of Dy in (28) and its meaning in (29). Equation (28) shows demand as dependent, *inter alia*, on the recent trend of output. Here, then, Dy represents the left-hand derivative. On the other hand, (29) describes entrepreneurs' plans for future production, and so here Dy is the right-hand derivative.¹

¹ It is because of the necessity of drawing this distinction, which disappears in continuous analysis, that it is not possible to use here the procedure for evaluating initial conditions described by A. Porter (*An Introduction to Servo-Mechanisms*, London: Methuen, 1950, pp. 50-3), and recently applied in economics by A. W. Phillips ("Stabilization Policy in a Closed Economy," *ECONOMIC JOURNAL*, June 1954, pp. 319-20, footnote).

But there is still a difficulty barring the way to a determination of $Dy(+0)$. It concerns the value to be assigned to Dz in (29) at the point of time when s changes. Dz here represents entrepreneurs' expectations of the future trend of demand, which we have supposed to be based on the experience of the recent past. Clearly it is impossible to take $Dz(+0)$. That is known neither by the economic analyst nor by the entrepreneurs until $Dy(+0)$ is known. It cannot therefore be used to determine $Dy(+0)$. On the other hand, there is no strong reason for taking $Dz(-0)$. It is possible, of course, that entrepreneurs will expect a resumption of the smooth trend of demand experienced before the jump, but it is at least as probable that their expectations of trend will be influenced to some extent by the direction and magnitude of the jump itself. In view of these considerations the safest procedure appears to be to introduce a parameter, σ , to replace Dz at $t = 0$ in (29), standing for the expectations of the trend of demand at that juncture.

At $t = 0$, then, (28) and (29) become

$$\begin{aligned} z(0) &= cDy(-0) + \kappa\{cy(0) - x(0)\} + \{1 - (s + \delta s)\}y(0) \\ Dy(+0) &= \varepsilon\{z(0) - y(0)\} + \sigma, \end{aligned}$$

where δs is the assumed change in s .¹ But

$$z(-0) = cDy(-0) + \kappa\{cy(0) - x(0)\} + (1 - s)y(0)$$

Therefore

$$\begin{aligned} z(0) &= z(-0) - y(0) \delta s & \text{and} \\ (30) \quad Dy(+0) &= \varepsilon u(-0) - \varepsilon y(0) \delta s + \sigma \end{aligned}$$

Assume that the system was in equilibrium before s changed. Then (30) becomes

$$(31) \quad Dy(+0) = -\varepsilon y(0) \delta s + \sigma$$

Although there is no way of determining σ in (31), economic considerations suggest an upper limit to the values it can take if s increases. Entrepreneurs will scarcely expect a more favourable outcome than are sumption of the previous trend. Thus $\sigma \leq Dz(-0)$. But if equilibrium existed before disturbance, $Dz(-0) = Dy(-0)$. Hence $\sigma \leq Dy(-0)$. Consequently the new trend of production must be lower than the old, though it is not necessary that a decline of production should occur.

If, however, entrepreneurs form their expectations of the future in the face of abrupt change in the same fashion as they have been assumed to form them under a regime of smooth development, by prognosticating a

¹ $z(0)$ is the level of demand at $t = 0$, to be distinguished both from $z(-0)$, the level immediately prior to the change in s , and from $z(+0)$, the level immediately after it. There is a double break in the level of demand, the primary change from $z(-0)$ to $z(0)$, brought about by the sudden change in the flow of consumers' spending, and a secondary change from $z(0)$ to $z(+0)$, caused by the influence of the abrupt change in production plans on capital outlay.

continuance of the most recently experienced trend, the primary fall in demand from $z(-0)$ to $z(0)$ will lead them to expect a further fall. σ will be negative, and so will $Dy(+0)$.

Probably their prognosis will lie between these two extremes, depending on the magnitude of δs . A minor reversal of previous trends may well be discounted heavily, so that σ will lie close to $Dy(-0)$. The larger is δs , however, the more weight is likely to be given to the immediately experienced shrinkage of markets, and therefore the more probable is a decline of production.

A fall in c is more apt to precipitate a slump than is a rise in s of comparable magnitude, because the former reduces capital outlays, both by causing redundancy and by diminishing the rate of growth of requirements, and may therefore be expected to produce a larger primary fall in demand.

The view of trade fluctuations implied by the preceding analysis is as lapses from a state of steady growth. In this it agrees with the conclusions of Kaldor's recent paper (*op. cit.*, pp. 623-4). It agrees, too, with his contention that the occurrence of slumps is neither regular nor inevitable. On the other hand, it sees the system as liable to slumps not "when the economy . . . needs to be 'switched back' to a more moderate rate of growth" (*loc. cit.*), but because, when the underlying conditions require a *higher* rate of advance, the economy can achieve it only after a preliminary deceleration or decline.

The causes of breakdown may be "natural" (*e.g.*, the exploitation of capital-saving improvements) or man-made. Warranted growth, being consistent with full employment of manpower, may well be accompanied by inflation. A vigorously pursued anti-inflationary or anti-speculative credit policy might be enough to bring on a slump.

(d) *Warranted and Natural Rates of Growth*

If the instability of warranted growth cannot be relied on to drive the economy towards a maximum rate of advance, the onus falls on those factors which may tend to bring the warranted and natural rates into coincidence. Even if warranted advance fully employs the economy's manpower, it is clearly desirable from the standpoint of efficiency that a maximum rate should be attained. Otherwise the economy is not making full use of its fund of "know-how." It is therefore important to be able to assess the degree of self-regulation in this respect of which the system is capable, and the extent to which Government intervention may be required.

There is no space for a full discussion of the question, but one may, perhaps, point out that the self-regulatory device on which Kaldor relies—the flexibility of profit-margins—appears to depend in an essential way on the instability theorem. The tendency of prices to rise in relation to wages is presumably brought about by the persistent shortage of capital experienced when growth is above the warranted path. But if the actual rate

of growth is attracted to the warranted rate, not driven away from it towards the natural rate, there is no persistence of capital shortage to inflate margins of profit.

Kaldor's analysis on p. 606 of his article seems to suggest, however, that margins rise because of excess demand when manpower is fully employed, rather than because of capital shortage. It is not clear why full employment of manpower should be accompanied by a rise of prices in relation to wage-rates. One would have thought that this is just the situation in which money wages tend to rise in proportion to prices.

H. ROSE

eter.

THE RATE OF INTEREST, MARGINAL EFFICIENCY OF CAPITAL AND INVESTMENT PROGRAMMING —A REJOINDER

I

IN a recent article in this JOURNAL ¹ W. H. White has re-interpreted certain findings from our study ² of the relevance of the marginal efficiency of capital concept to contemporary programmes of large manufacturing firms for selecting their investment projects. Dr. White has previously indicated ³ that he believes the importance of the interest rate upon investment decisions has been appreciably underestimated. In the article under discussion here, he takes particular issue with our observation that the market rate of interest is of "evident unimportance" ⁴ in business planning. There are several other points made in our study with which he has disagreed, and upon these we shall offer some additional comment.

With regard to the latter, it should be noted that Dr. White's 1956 "interest inelasticity" study and ours are only incidentally concerned with the same subject. Our survey results on investment demand sensitivity to the rate of interest were subordinate to our main purpose: namely, to define meaningfully the *marginal efficiency of capital* concept and to demonstrate how it may be applied in investment programming. Collateral to this, we polled business firms with regard to: (a) determinants of future income; (b) degree of detail with which future returns are estimated; (c) sources of funds for new investment; (d) evaluation of alternative investments, both internal and external to the firm; (e) consideration given to *time* as a factor affecting the profitability of investment; and finally, (f) the relevance of the market rate of interest in profits estimates. Even the last-named subject of inquiry was secondary to an analysis of methods for calculating the discounted value of future income, including a suggested procedure for computation.

In these circumstances it is Dr. White's principal contention that we have misread ⁵ the results of our survey of the investment planning practices of fifty-seven large and medium-sized manufacturing firms with regard to the influence of interest rates; instead, by means of an ingenious but we think gratuitous deduction concerning the minority of firms who resort (frequently

¹ W. H. White, "The Rate of Interest, The Marginal Efficiency of Capital and Investment Programming," *ECONOMIC JOURNAL*, Vol. LXVIII (March 1958), pp. 51-9.

² M. D. Brockie and A. L. Grey, "The Marginal Efficiency of Capital and Investment Programming," *ECONOMIC JOURNAL*, Vol. LXVI (December 1956), pp. 662-75.

³ W. H. White, "Interest Inelasticity of Investment Demand—The Case from Business Attitude Surveys Re-Examined," *American Economic Review*, Vol. XLVI (September 1956), pp. 565-87.

⁴ Brockie and Grey, *op. cit.*, p. 666. Cf. White, "The Rate of Interest, The Marginal Efficiency of Capital and Investment Programming," pp. 51-2.

⁵ *Ibid.*, pp. 51 ff.

or only occasionally) to the capital markets for part of their investment funds, he interprets our results in such a way that investment demand appears to be significantly more interest elastic than we had concluded.¹ But here our position remains unchanged. Certainly there is no indication in Federal Reserve figures for loans by "all banks," "all commercial banks" or "all member banks" from September 1957 to April 1958 that "easier money" Federal Reserve policy has improved the case for interest elasticity. Over this period the actions of the Federal Reserve system resulted essentially in a downward revision of the structure of interest rates by:

- (1) Reducing Federal Reserve bank discount rates, beginning in mid-November 1957, from the 3½% November high to 1¾% (2¼% in two districts) by April 1958.
- (2) Reducing member bank reserve requirements a total of 1 to 2 percentage points (depending upon class of bank) on net demand deposits in successive steps between March 1 and April 24, 1958.

As a partial result of these activities, the rate on new 91-day Treasury Bills declined from an October 1957 high of 3.591% to a current (May 29, 1958) rate of 0.673%. Over roughly the same period, the rate on three to five-year U.S. Government notes and bonds fell from 3.99 to 2.30% (as of the week ending May 3, 1958). Similarly, the rate on Moody's Aaa Corporate bonds moved downward from 4.12 to 3.55%, while Baa rates declined from 4.93 to 4.66%.

Meanwhile, upon examination of changes in commercial and industrial loans to manufacturing and mining firms by weekly reporting banks, we find little evidence that the afore-mentioned Federal Reserve discount rate and reserve requirement policies stimulated any appreciable rate of change in demand for new capital during these eight months. Neither do figures for new flotations of corporate bonds and common stocks indicate any significant sensitivity to the altered structure of interest rates over the like period.

Taking a somewhat longer-run view, if one compares the sums of new capital raised by manufacturers since 1952 with the changes in the structure of interest rates induced by Federal Reserve activities, the case for high interest elasticity of investment demand appears even weaker than some have proposed; and this is especially true if one contrasts the first quarters of 1957 and 1958.

Beyond this, of the \$18 billion decrease in U.S. gross national product occurring between the third quarter of 1957 and the second quarter of 1958, 14.7 billion is accounted for by a 3 billion decrease in purchases of producers' durable equipment along with a 12 billion decrease in inventory investment (the latter two figures sum to \$14.7 billion when a +0.3 billion investment in new construction is added algebraically). And this fall-off in gross

¹ Brockie and Grey, *op. cit.*, pp. 665 ff.

private domestic investment occurred despite the Administration's "easier money" policy which began in the fall of 1957.

Actually, it is as a result of this sort of experience that confidence, or lack of confidence, in the monetary approach as an *exclusive* pro-recovery measure in engendered. The case for this kind of monetary policy must be judged in circumstances such as those prevailing over the recent past. If the coefficients of interest elasticity of investment demand are as high as some assume, then current monetary management, plus neutral fiscal policy would perhaps be "adequate" for combating recessions of even greater severity than that of 1957-58. But our contemporary experience could well lead one to deny the validity of this assumption. And this, fundamentally, is what the disagreement is about; in other words, is the potency of monetary management as cyclically symmetrical as an *a priori* conditional theoretical approach might lead us to believe, or instead, is its utility as a stabilising influence to some extent limited to the prosperity phase of the cycle?

Finally, the additional fact remains that among the firms we investigated, by far the largest part of investment funds was not obtained from the capital markets during the years immediately preceding our study (prior to 1955). Had our investigation been made a year or two later, this specific finding would doubtless have been modified somewhat inasmuch as the volume of new business investment coming from external sources increased in both 1956 and 1957. Moreover, it is a reasonable conjecture, in view of the apparent reluctance of some firms to obtain investment money from external sources, that the depletion of accumulated surpluses has been a subsidiary reason for the decline in *aggregate* investment outlays.¹ Like the rate of interest, however, the precise significance of this phenomenon is obscured by numerous other influences. Thus, while we cannot concur with Dr. White's general proposition that most theorists have underestimated considerably the degree of exposure of business investment to the ratiocinations peculiar to capital markets, we do not, of course, deny that for some firms external sources of funds may be extremely important.

II

At another point Dr. White has questioned our reading of Dr. Roos' conclusions with regard to the lagged investment-profit hypothesis.²

In this part of our paper we were concerned with the role of profits in shaping the scheduled marginal efficiency of capital. As we attempted to make clear,³ Dr. Roos at this juncture is most explicit. He writes:

"Although the writer in 1927-29 gave considerable attention to expected returns and their discounted present value as an element in

¹ For further discussion or reservations regarding external financing, see pp. 340 ff below.

² White, "The Rate of Interest, The Marginal Efficiency of Capital and Investment Programming," p. 56, n. 7.

³ Brockie and Grey, *op. cit.*, pp. 665, 671.
No. 274.—VOL. LXIX.

determining investment, his later studies indicate that actual investment lags returns or profits rather than leads them. Thus, past rather than future profits determine investment, and so Keynes has overemphasized the role of expected profits and consequently produced a somewhat unrealistic idealized theory.

"In the present paper the profits for the year ending six months previous are used in place of the series of annuities employed by Keynes. Thus the marginal efficiency of capital is the interest or discount rate which would make the capitalized value of profits for the year ending six months previous equal to the current supply price."¹

Furthermore, in his later work (referred to by Dr. White)² Dr. Roos still maintains that actual investment *lags* corporate profits after taxes.³ For predictive purposes over the years since 1942 he has decided that "corporate profits" *alone* are ". . . now neither valid, nor necessary . . . as a 'proxy variable' for corporate *purchasing power*" (italics ours).⁴ Prior to 1942, corporate profit as a single "proxy variable" was adequate,⁵ but the special amortisation "certificates of necessity" which were introduced during the Second World War and the Korean War⁶ have at least temporarily required an adjustment of the original "proxy variable." Thus, Dr. Roos revised measure of corporate purchasing power is ". . . defined as corporate profits retained + new financing + depreciation and obsolescence."⁵

Apparently Roos' determination that new investment shows a high correlation with past profits has gained wide acceptance as a predictive device. For example, the chief economist of the National Industrial Conference Board in the United States has explained:⁶

"At our shop we have been developing what is almost an equation or rule of thumb for correlating capital appropriations with profit performance. With rising profit margins and rising profits we get a pattern of expanding capital appropriations; with shrinking profit margins but stable profits we get a neutral pattern of capital appropriations; but with the continued negative sign of lowered profits, we get still a third pattern or sharply contracted capital appropriations. There may be three-months' or six-months' lag, but in an unfavourable situation, even among companies with an excellent long-range future, immediate implementation in the way of capital appropriations shrinks."

Moreover, in another statistical and corroborative study, Gehrels and Wiggins have developed regression formulæ linking profits and lagged investment which produce a good fit for the empirical data "whether" the former are "taken for the six months immediately preceding the investment, one

¹ C. F. Roos, "The Demand for Investment Goods," *American Economic Review*, Vol. XXXVIII (May 1948), Supplement, p. 313.

² *Loc. cit.*

³ C. F. Roos, "Survey of Economic Forecasting Techniques," *Econometrica*, Vol. 23 (October 1955), pp. 363-95.

⁴ *Ibid.*, p. 392.

⁵ *Loc. cit.*

⁶ "Planning Capital Expenditures—and the Company's Future" (a symposium), *Business Horizons* (Spring 1958), p. 78.

period earlier than this, or two periods."¹ Similarly, they investigated the relationship to aggregate investment of, alternatively, investment goods prices, liquidity, and various classes of interest rates. With suitable restrictive conditions it was demonstrated that interest rates were related to the level of investment.² Gehrels and Wiggins do not find these results to be of much significance for monetary policy, however.³

Gehrels and Wiggins are themselves persuaded of the validity of the "credit-rationing version of the interest-investment relation" whereby interest charges do not figure directly in the determination of investment but are relegated to the position of concomitant variable.⁴ Further evidence associated with this interpretation will be discussed at a later point.⁵

It may be that other considerations are also operative respecting interest rates and investment. While Gehrels and Wiggins were unable to find "the expected causal connection between liquidity and investment outlay" in their particular analysis,⁶ Meyer and Kuh, on the other hand, did conclude that ample liquidity is an important prerequisite to investment.⁷ Accordingly, it might be postulated that an apparent correlation of interest rates in the recent past and aggregate investment is significant to managers as an indication of impending changes in liquidity.⁸ While many firms may not be particularly concerned with the rate of interest *per se*, they will not be able to ignore the operations of the capital markets altogether, even if depreciation allowances and retained earnings are their normal sources of investment funds, because of the effect the interest rate may have upon the

¹ F. Gehrels and S. Wiggins, "Interest Rates and Manufacturers' Fixed Investment," *American Economic Review*, Vol. XLVII (March 1957), p. 85.

² In summary they state: ". . . we shall present evidence that with the proper combination of explanatory variables, the appropriate time lags, and sufficiently short periods, an interest-investment relationship is present . . . as well" (*Ibid.*, p. 80).

³ ". . . the advocates of active monetary policy may derive only limited comfort from the evidence. For the time lags which seem to give the highest significance for interest rates are great enough to imply a difficult timing problem for monetary measures" (*loc. cit.*).

⁴ *Loc. cit.* Also the comment by R. F. Henderson: "Instead of considering the rate of interest by itself as one of the determinants . . ., we should broaden our approach and consider the availability of funds on reasonable terms." "Industrial Investment in Fixed Capital: A Reconsideration," *Scottish Journal of Political Economy*, Vol. 3 (October 1956), p. 178.

⁵ Pp. 341 ff.

⁶ *Op. cit.*, p. 91.

⁷ J. Meyer and E. Kuh, "Acceleration and Related Theories of Investment: An Empirical Inquiry," *Review of Economics and Statistics*, Vol. XXXVII (August 1955), pp. 229-30.

⁸ Moreover, even though the predictive usefulness of models linking selected variables with investment changes may be considerable, this does not mean that investment decisions are actually made on so simple a basis. The maximisation of the firm's position is of such complexity, after all, that even comparatively elaborate workable models seeking to reflect the realities of the decision-making process must fall short of encompassing more than segments of the problem. For example, in the "realistic" programme of "summer maneuvers" conducted by the American Institute of Management for the upper echelons of corporate executives in which an electronic computer keeps "score," the problems are limited to only one facet of managerial rationality—the maximisation of *net worth*. See J. McDonald and F. Ricciardi, "The Business Decision Game," *Fortune* (March 1958), pp. 140 ff.

liquidity of owned assets at book value (*i.e.*, the discounts on near cash and other holdings).

Even if the interest rate is credited with a determinative role in firm policy, it may not operate with equal effect upon long-term as compared with short-term investments:

"The standard argument that credit contraction affects long-term rather than short-term investment rests, of course, on the relative effects of an increase in the costs of borrowing on investment projects, the expected return from which is assumed to remain unaltered. This is quite compatible with the view that in certain circumstances—for instance an adverse balance-of-payments position generally expected to be temporary—credit contraction by the monetary authorities may do more to depress expectations as to the profitability of business over the next year than those of profitability over the next ten years. The reaction of leading British motor car manufacturers to credit restrictions in the winter of 1955–56—reducing *current* output but *continuing* with their *long-term* expansion plans—seems to be an example of this."¹

There is, of course, the possibility that not only the interest–investment relationship has been overemphasised, or that it is not of so great importance as in the past, but that the sensitivity to profit changes may also be less than formerly because of the insulation depreciation funds may afford.²

We, in common with some other observers, found certain differences in the investment behaviour of public utilities as compared with large manufacturing corporations—among them greater use of rational capital programming methods and greater recourse to the capital markets.³ We cannot share Dr. White's optimism, however, that this evidence may presage the discovery of a correlation especially "favourable to the prospects for investment control through monetary policy" in so far as utilities companies are concerned.⁴ The reason is that the motives for investment in the case of utility companies are not the same as in the case of other enterprises, and they may involve their own unique elements of insensitivity to interest rate changes. As explained by Henderson:

"The distinction between industry and the public utilities would not be worth stressing if investment in each were governed by the same considerations. But that is surely not so. Nor does the method of ownership seem crucial in this connection. Whether publicly or privately owned, electricity companies *have to invest to meet a long-term growth of demand.*"⁵

While we regarded it with some question, we, too, found the utilities respondents to our survey stressing that they must, because they are regulated,

¹ Henderson, *op. cit.*, p. 183 (italics ours).

² Cf. *Business Horizons*, *op. cit.*, p. 78.

³ Brockie and Grey, *op. cit.*, p. 662 n.

⁴ White, "The Rate of Interest, The Marginal Efficiency of Capital and Investment Programming," p. 59.

⁵ Henderson, *op. cit.*, p. 179 (italics ours).

invest to meet the expansion of demand for their services. This legal requirement and other considerations peculiar to the utilities business compel them to be more active in the capital markets than most firms for various reasons elect.¹

Like other industries, the utilities field, in its investment planning, appears to be much more responsive to factors other than changes in the interest rate. What its greater exposure to the capital markets does tend to mean to a marked extent, however, is mobility among the various alternative sources of outside financing. For example, during the 1957–58 business recession in the United States, business generally contracted its borrowings at banks as it reduced overall investment outlays. In the case of utilities, which expanded their capital expenditures slightly, this contraction of bank debt was accompanied by a shift to insurance companies and other alternative sources which had become more propitious.²

III

The above discussion is concerned with the relationship of interest rates (and other variables) to *aggregate* investment. Questionnaire studies, such as that we conducted, have attempted to *qualify* the relative importance of the rate of interest as among *different* firms. Firm size is one basis of observable difference. *A priori*, two contradicting comments might be made respecting the inter-relationship of size (we used number of employees as an index of firm size) and the firm's sensitivity to the market rate of interest:

(1) On the one hand, large firms, presumably possessing a greater degree of oligopolistic or monopolistic authority, and therefore stability in their respective markets, will be able to undertake greater refinement in their management planning, including capital programming. By contrast, the smaller firm may necessarily discount so highly for risk that all other allocative criteria pale to insignificance, or else the firm may be so starved for new funds that it has little reason to perfect devices of project selection.³

(2) On the other hand, the small firm is usually more dependent upon the capital markets and is, as a consequence, more affected by changes in interest rates on borrowed money. This viewpoint may be supported by the contention that its more competitive position leaves it with smaller retained earnings, thereby rendering it more dependent upon the capital markets. It also has been observed that the young firm has a greater demand for funds than the older firm because it is

¹ Concerning small, non-utility firms in this regard, see p. III (2) below.

² "Financing Business Investment," *Federal Reserve Bulletin* (June 1958), pp. 644–5.

³ Concerning the latter point, A. D. H. Kaplan and P. H. Banner observe that, "As long as small business gets its capital in a haphazard way, it will think in this manner." "Adequacy of Small Business Financing: One View," *Financing Small Business*, a Report to the Committees on Small Business, Parts 1 and 2, 85th Congress, 2nd Session, April 11, 1958, p. 123.

expanding more rapidly in size and, *ipso facto*, the young firm is generally a small firm.

Finally, as between borrowed funds and equity sources, the small firm tends to have a stronger preference for the former, being more afraid of loss of management control if outsiders are brought into the ownership group.¹

Our survey disclosed that, on balance, the small firm shows more concern with the interest rate than the large. Subsequent to our study, and also that of Dr. White, investigations by the Federal Reserve System have raised important new questions respecting the efficacy of the interest rate as an allocative device in this one area, the smaller firms, where it is presumably of greater importance. It was found, on the basis of a comparison of results of two surveys conducted among member banks by the Federal Reserve System in October 1955 and again during the same month in 1957, that interest rates rose less for smaller-sized firms than for larger ones in this period of generally rising interest rates, while, at the same time, the volume of borrowing by smaller firms also tended to increase much less than the borrowing by larger enterprises as shown in the following table:

*Percentage Increases in Interest Rates and Amounts Borrowed at Member Banks.
Data for Business Firms Classified by Size (Total Assets), 1955 and 1957.**

Firm size (thousands of dollars of total assets).	Average increase in bank interest rates.	Increase in amounts borrowed.
Under 50	12.1%	* 3.0%
50-250	11.8	16.7
250-1,000	17.4	24.8
1,000-5,000	24.4	21.3
5,000-25,000	29.7	24.7
25,000-100,000	32.4	51.1
100,000 and over	36.4	66.4

* "Member Bank Lending to Small Business," *Financing Small Business*, *ibid.*, pp. 375, 388.

The exact causes of a generally negative correlation between interest-rate increases and total amounts borrowed within each size class were not ascertained by the survey. Superficially, this relationship may be explained in terms of simple supply-demand relationships. This, of course, supports the proposition stated earlier that the interest elasticity for small firms is greater, since a relatively modest increase in interest rates was accompanied by a much smaller increase in total borrowing among the small firms than among the large, which experienced much greater interest rate increases. However, there is some reason to question that so simple a causal relationship actually was operative. One conflicting consideration is the institutional rigidities which prevent interest rates from being completely freely fluctuat-

¹ *Ibid.*, pp. 118-19. Also see J. Clark, Jr., "Management Problems of Small Manufacturing Enterprise in Relation to Financing," *ibid.*, p. 324.

ing.¹ As a consequence, the credit-worthiness of the borrower may assume primary importance in credit allocation.²

IV

White comments that our survey disclosed "much more precision and rationality of investment programming" than was indicated by the earlier surveys,³ and he notes further that our results indicate "longer horizons" than were "previously thought to exist."⁴ That advancement along this line is being made is confirmed by other writers. Six surveys of manufacturing firms between 1927 and 1951 "... show, that within the limits of sampling error, there has been an unmistakable trend in American industry toward the use of longer and longer payoff criteria for the purchase of manufacturing equipment."⁵ Comparison of earlier results with our findings confirms a continuation of this trend:

Length of Pay-out Period on Manufacturers' Equipment

	1927.*	1951.*	1955.†		
			All firms.	Number of employees.	
				7,500 or more.	Under 7,500.
Mean . . .	3 years	5.1 years	5.6 years	7.3 years	4.7 years
Median . . .	3	3-5	5	6	5
Mode . . .	3	3-5	5	5	5

* Smith, *loc. cit.*

† From authors' survey of 57 large manufacturing firms.

¹ "There are some indications that 6% may represent, in the case of some banks at least, an effective rate ceiling on loans to small borrowers. . . . In many areas of the country usury laws and banking tradition prevent nominal or stated rates from going above 6%.

"One possible result of such limitations during a period of rising interest rates is to reduce the relative profitability of loans previously made at ceiling levels as rates on loans previously made below these levels are raised" (*ibid.*, p. 390).

² "[B]eyond a certain increase in demand, loans are usually rationed among borrowers, to a large extent on the basis of credit worthiness. This would suggest that during the period of tight money the banks rationed a smaller number and amount of loans to the smallest borrowers than they did to the largest borrowers. . . .

"[T]hat interest rates did rise relatively less for the smallest group can be interpreted as meaning that bank lenders were reluctant to lend to the smallest borrowers during the period. That is, instead of increasing interest rates as an expression of their reluctance, they raised their standards of credit-worthiness and scrutinized small borrowers more closely. This means, as indicated in the interviews which accompanied the survey, that bankers eliminated the riskiest small loans on which they were charging the highest rates in 1955, and raised the rates on credit-worthy small borrowers by a larger factor than shows in the cold figures for 1957" ("Monetary Restraint and Business Loans in the Twelfth District, 1955-57," Federal Reserve Bank of San Francisco, *Monthly Review* (May 1958), p. 80).

³ White, "The Rate of Interest, The Marginal Efficiency of Capital and Investment Programming," p. 51.

⁴ *Ibid.*, p. 55.

⁵ V. L. Smith, *A Theoretical and Empirical Inquiry into the Economic Replacement of Capital Equipment*, unpublished Ph.D. thesis (Harvard, April 1955), pp. 5-35.

A recent statement by Dexter M. Keezer (Vice-President and Director of the Economics Department, McGraw-Hill Publishing Company, which has regularly surveyed business plans for investment over the past ten years) suggests that the average length of the planning period for all firms, instead of just the time intervals of firms indicating they do any investment programming at all, has been increasing as systematized capital expenditure has become more prevalent.¹

Our criticism of the pay-out method has not been based solely, or even principally, on the time span used in its calculation. Nor do we agree with White that a major distinction is to be made according to whether or not the future earnings estimates used in calculating profitability are gross or net of depreciation. The varieties of techniques ". . . to determine the economic worth of capital appropriation projects," instead, ". . . run the gamut from the simple pay-back method to the more complicated present-worth method."² As Hansen points out, *time discounting* of the income stream is the "distinctive feature" of Keynes' formulation.³

It is true that in Dean's schema, "rate of return" on investment (*either* gross or net depreciation) is posed as a major alternative to the pay-out, which for most firms he considers "an inadequate measure of investment worth."⁴ It is clear, however, that he does not consider average rate of return as really of the same genus as the discounted rate of return for which he expresses a distinct preference.⁵

¹ "When we started there were maybe five companies who could give us any idea what their investment would be beyond the current year. Ten years later over 90 per cent of the companies had what they thought was a plan; at least they were able to put down some figure."

"This was a very much larger number of companies than we started with and unless somebody's deceiving himself, there has been in the last three years, a very large increase in the planning of capital investment by business firms" ("Planning Capital Expenditures—and the Company's Future," *op. cit.*, p. 73).

² H. R. Lammie, "Return on Capital Employed," *Journal of Accountancy*, Vol. 106 (August 1958), p. 40.

³ "The distinctive feature about Keynes's rigorous definition of the marginal efficiency of capital is that he rightly takes cognizance of the entire *series* of annual 'prospective yields' over the whole anticipated life of the capital good. Commonly, in discussing the marginal product of capital, economists had concentrated attention on the *current* marginal product, *i.e.*, the absolute annual product, after deducting the running expenses and depreciation for the current year" (A. H. Hansen, *A Guide to Keynes* (New York: McGraw-Hill, 1953), p. 123).

⁴ J. Dean, "Measuring the Productivity of Capital," *Harvard Business Review*, Vol. 32 (January-February 1954), p. 124.

⁵ *Ibid.*, p. 129.

We have already cited the findings of Roos and others, which suggest that the profit rate is that factor which will just equate the sum of the discounted past income stream with the asset supply price. Dean has not dealt specifically with this question, but as a technique of capital rationing it is consistent and perfectly reasonable to suppose that future income will be estimated as a kind of mirror image of past experience, with the recent period being reflected in the forefront of these calculations. Though at one point he does state that "Return on old, sunk investments has only historical interest and no relevance to decisions on new investments" (J. Dean, *Capital Budgeting* (New York: Columbia, 1951), p. 21).

He points out that—

"... a serious drawback to the use of the accounting method average rate of return is that it is insensitive to variations in the time pattern of investment outlays and earnings. By taking an annual average of earnings over the life of a project this method ignores the earnings *trends*, which may be quite important"¹ (*italics ours*).

It is curious that Dr. White seeks to minimise the importance of time discounting, inasmuch as he appears anxious to rehabilitate monetary policy and maintains that business decisions are more responsive to interest rate changes than we have judged them to be. By adoption of the methods we have discussed, business investment could scarcely fail to become more sensitive to adjustments in the rate of interest than recent history records.

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¹ Dean, "Measuring the Productivity of Capital," *op. cit.*, p. 128. Also, compare his statement that "average return on old and new investments is badly misleading" (*Capital Budgeting, loc. cit.*).

R. J. Reul has proposed an elegant and detailed method for programming all types of capital expenditures. Its essential feature is that it takes "into consideration variations in time pattern." "Profitability Index for Investments: Applies to Any Capital Expenditure," *Harvard Business Review*, Vol. 35 (July-August 1957), pp. 116-32.

REVIEWS

Economic Backwardness and Economic Growth. By HARVEY LEIBENSTEIN.
(New York: John Wiley (London: Chapman & Hall), 1957. Pp.
xiv + 295. 54s.)

DR. LEIBENSTEIN has had the interesting idea of marrying an economic growth model (recognisably of the Harrod-Domar family) with a population growth model, of the kind familiar to demographers. Though he has something to say on other aspects of development, and underdevelopment, I do not think one does him an injustice if one treats this "economic-demographic" model as the core of his book. Certainly it is the heavy emphasis that is laid upon this aspect which differentiates his treatment from that of other writers on his general subject.

It is crucial to Dr. Leibenstein's theory that the natural increase in population can be ascribed to similar forces as those which are supposed (by the accelerationists) to govern the accumulation of capital; and that the reaction of population increase on economic growth can be analysed in similar terms. The model which is based upon these contentions may be summarised very roughly as follows.

Apart from the population reaction, the balance of saving and investment is fundamentally unstable; if a "random disturbance" increases investment, capital begins to grow, output grows with it and the growth of output sets up an incentive to further expansion. It is by a mechanism of this sort that developed countries have developed; why is it that others (the underdeveloped or backward economies) have failed to develop in this manner? The answer, according to Dr. Leibenstein, is to be found in the population reaction. If one starts from a high level of output *per head*, the rise in output (just specified) does not call forth any significant rise in population; the accelerator can therefore work unimpeded. If, however, one starts from a low level, the expansion in population that will be called forth will be so large as to drown the expansionary effect of the increase in aggregate output—so far, at least, as real income *per head* is concerned. Expansion in aggregate output will accordingly result in a mere maintenance, or possibly in a decline, of real income *per head*. The failure of *per capita* income to expand is then taken to be a retarding influence on the general expansion. This braking effect is so powerful, in the case of underdeveloped countries, that their natural state is one of "stable equilibrium." They are unable to grow, because their growth would be checked by the population increase which would be induced by any expansion in net income.

The practical conclusion that is drawn from this analysis is that to

break the jam there must be a disturbance, an "effort," which is greater than some "critical minimum." The initial rate at which income rises must be large enough to offset the population check; indeed, if the initial growth is sufficiently rapid, the population increase will itself be damped down. Once a maintainable rate of growth in *per capita* income can be established, the hurdle is overcome; the backward economy passes out of its "stable equilibrium" into a condition of automatic growth.

The idea of a "critical minimum" is, of course, by no means peculiar to Dr. Leibenstein's theory; but no one else, to my knowledge, has formulated it in such exclusively populationist terms. Can one really ascribe to the population factor quite so dominating a role in the causation of underdevelopment?

There are, I think, two things, and two only, which in this field do stand really firm. One is the awe-inspiring increase in population which is at present taking place in a large number of underdeveloped countries, especially in the tropics; the other is the sequence of an expanding, and then a contracting, phase in the rate of population increase which most of the more developed countries have historically experienced. These are the foundations on which Dr. Leibenstein has raised his theory. But do they bear the weight that he has put upon them?

One may well be prepared to go along with him in his economic explanation of the two phases in the population history of the developed countries. One can understand that the willingness to bear children, and the power to rear them to maturity, can be influenced by economic forces in such a way as will go far to explain the historical facts. But this, for his purpose, is by no means enough. He has to show that the increase in population, which is admittedly a frequent accompaniment of economic growth in its early stages, does in fact act as a braking factor. On all this side of the matter he seems to me to be quite unconvincing.

I have indeed searched in vain for a clear account of the way in which the model is supposed to work at this point. I have tried to mend it by inserting a bit of Ricardo; Ricardo certainly did get an answer which from Dr. Leibenstein's point of view is the right answer, but the mechanism on which Ricardo relied is quite foreign to Dr. Leibenstein's, who (throughout his work) pays the minimum of attention to distributional considerations. The Keynesian considerations (effects of increasing population on the incentive to investment) that can easily find a place among Dr. Leibenstein's curves, work (as Keynes himself showed) the wrong way. The only thing that is left (and I suppose that it is from this that the brake is supposed to come) is a deficiency of saving. But if the underdeveloped country is always trying to invest ahead of its savings, its situation should be chronically inflationary. It can only be an excessive attention to the special problems of South America at the moment (to which North American writers on underdevelopment are undoubtedly prone) which can

make anyone imagine that the "static equilibrium" of the underdeveloped has any special inflationary potential about it.

Something, surely, has gone wrong. It seems to me that what is wrong is not merely the odd view of history which is implied throughout the whole of this work; there is a bit of a mix-up on the theoretical level also. In an early chapter a good deal is made of the distinction between a *stable* and a *quasi-stable* equilibrium—roughly speaking, the one is a condition where nothing changes, the other is a condition where some things change, but others don't. The only bearing that this distinction can have upon the later work is to enable the author to regard a condition in which population is increasing, but real income per head is not increasing, as a *quasi-stable equilibrium*. That, of course, is a mere matter of definition; but it turns out to be a dangerous definition, because it enables him to proceed as if the full equilibrium, in which there is no growth in any direction, and the quasi-stable equilibrium, in which the growth of other resources does no more than match the growth in population, were very much the same thing. That, surely, is just nonsense. If we accept it, the condition of England from 1750 to 1850 (the very archetype of all "take-offs") is made indistinguishable from that of, say, Portugal during the same period. Surely the fact is that to be able to accommodate a large growth in population, without fall in real income per head, does itself demand a most important *growth* in resources; it cannot readily be achieved excepting in an economy which is already geared to extensive change.

To an economy of this latter type the dynamic theory of Harrod and Domar (or something akin to it) may well have become applicable; but to an economy which has not learned to grow, even in that way, it is surely not applicable at all. It is just not true that the main thing which stops such economies from going forward is potential population pressure. (How, incidentally, would Dr. Leibenstein explain the Roman Empire?) Nor, for that matter, is the population pressure from which so many underdeveloped countries are at present suffering a consequence of abortive economic development; it clearly springs from other causes. If Egypt or Ceylon (to name two countries where population pressure has already reached the stage of political explosion) could achieve the "quasi-stable equilibrium" of early-nineteenth-century Britain, they would be over their immediate hurdle. Problems of this sort are left by Dr. Leibenstein, as a result of the way he has posed his questions, right out of account.

I have tried, in this review, to concentrate on central issues; and since I have become convinced that the book is quite wrong on central issues, I have doubtless done less than justice to the incidental virtues which it possesses.

Dr. Leibenstein is an artist in diagrams; when he applies his technique to simpler problems, such as the effect of changes in real wages on the

individual supply of labour, the result is often distinctly helpful. But it may well be that on the main issue it is his excessive reliance on the diagrammatic method which has led him astray. If he had endeavoured, more frequently, to set out the more intricate parts of his argument in plain prose, he might possibly have got more idea of what a queer tree it was that he was climbing up.

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The Economics of Under-developed Countries. By P. T. BAUER and B. S. YAMEY. Cambridge Economic Handbooks, under the general editorship of C. W. GUILLEBAUD and MILTON FRIEDMAN. (London: James Nisbet and Cambridge University Press, 1957. Pp. xii + 271. 10s. 6d.)

Economic Analysis and Policy in Underdeveloped Countries. By P. T. BAUER. (London: Cambridge University Press, for the Duke University Commonwealth-Studies Center, 1957. Pp. ix + 145. 22s. 6d.)

THESE books give a lucid and luminous account of a wide range of development issues. They are most significant, however, for their criticism of several propositions and prescriptions that have become standard elements in the development literature.

The Cambridge handbook begins by examining the conceptual and practical difficulties of measuring national income, capital and the labour force. The discussion then ranges over several topics grouped in terms of natural resources, human resources and capital. The remaining third of the book delimits the role of government in influencing development.

The second book is the published version of three lectures delivered at the Duke University Commonwealth-Studies Center. The first lecture deals with the scope, method and possibilities of economics in the study of underdeveloped countries, as well as with the repercussions on economics of the increased interest in underdeveloped countries. The second deals with some characteristic features of the economic landscape in underdeveloped countries, and the third examines policy issues. Except for the first lecture, there is considerable overlap with the Cambridge study.

Using an appealing blend of systematic analysis and personal observation, the authors suggest in the Cambridge book that the most important obstacles to development are associated with the skill, resourcefulness and thriftiness of the population, the effects of social institutions and restrictions on the movement of people or on the acquisition and exercise of skills, the state of production techniques, and the magnitude and character of capital formation.

In conformity with their view of development as essentially "the widening of people's access to alternatives," the authors adopt as their criterion of good governmental policy that it should extend the efficient

range of alternatives open to producers and consumers; this leaves the government "only a limited role as an active propulsive agent in development in the capacity of saver or entrepreneur, or as controller or director of the economic activities of others."

It is claimed that the possibilities of development through controlled inflation are commonly exaggerated, and the dangers unduly minimised. Taxation for compulsory saving is also rejected as impinging unfavourably on productive effort, growth of specialisation and exchange, and private saving. It is further argued that if the funds for development are compulsorily gathered by statutory marketing boards the burden is particularly inequitable and the disincentive effect very high.

To raise efficiency in agricultural production, efforts should be directed to increasing the volume of capital and skill, rather than to changing the institutional setting—tenancy or ownership—which does not by itself come to grips with the underlying problem of the ratio between various classes of resources. Proposals that rents or debt charges should be scaled down, or that land should be transferred to the small-scale tenant cultivators, are believed unlikely to bring about significant net benefits, and may even reduce agricultural output.

The external economies, disguised unemployment, balanced growth and diversification arguments for assistance to industry are rather summarily dismissed as being justified only in very special circumstances and as being difficult to apply in practice. On the other hand, it is proposed that more assistance may be given to industry through the withdrawal or relaxation of restrictions on immigration, avoidance of "premature attempts to introduce so-called labour and social welfare legislation" which inflate labour costs and avoidance of policies that control the charges or profits of public utilities or foster uneconomic branches of agriculture.

Although all the individual points are of interest, and each is presented forcefully, their total effect does not come across in an impressive way. The reader will still want to know just what are the forces that initiate and maintain development. And he may well believe that some problems of the poor countries have been underestimated, their potential for development overestimated and the role allotted to government unduly circumscribed. These reactions are possible because the authors restrict their discussion mainly to the contemporary period, to the domestic aspects of development and to an analysis conducted primarily in terms of marginal conditions.

History is used simply to provide an occasional illustrative example—not for the more significant purpose of explaining the beginnings of growth, or to account for the differential rates of growth among nations. Indeed, this fundamental task is ruled out by the belief expressed in the Duke book that "the quest for a fundamental cause or causes of development, and in particular for reasons explaining why some countries were caught

up in the stream of material progress sooner than were others . . . may be largely a fruitless quest."

On the crucial question of why the poor countries have not developed more rapidly, only slight attention is given to the repercussions of international forces. When there has been so much recent emphasis on the unequal distribution of gains from trade between rich and poor countries, and the historical tendency for increasing international inequalities, one may regret the failure to appraise these arguments. The general tenor of their analysis indicates that the authors would have been eminently qualified to expose some fallacies in this modern "exploitation" type of reasoning.

The concern with marginalism has much remedial worth in rectifying the neglect of prices in some discussions of development. At the same time, however, it denies some interesting questions, such as those raised by Professor Leibenstein's "critical minimum effort thesis," or Professor Rosenstein-Rodan's "big push" theory. Nor is the marginal analysis used adequately for a dynamic analysis of the multi-period requirements for optimal resource allocation.

The limited role allowed to the government is a corollary of marginalism. A major contention is that restrictive measures which come from government intervention affect adversely both the allocation of resources and the growth of resources and of income. Accordingly, the agenda for state action, as summarised in the Duke book, does not extend far beyond influencing the institutional framework of economic activity, providing social overhead capital, operating agricultural extension services and possibly establishing capacity if there are important economies of scale.

The development process, however, is unlikely to have the continuity and flexibility implicit in marginal analysis. Imperfect markets, indivisibilities in production functions, low supply elasticities, market prices that necessarily reflect only present conditions, and a social structure and value system that contradict individual action—all these limit the efficiency of the price mechanism in achieving the requirements for development. Although one may sympathise with the authors' political philosophy, he may still have to conclude that the present situation in many underdeveloped countries requires more extensive government action than these books are prepared to acknowledge.

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Possibilities of Economic Progress. By A. J. YOUNGSON. (London: Cambridge University Press, 1959. Pp. x + 325. 32s. 6d.)

IN recognition of the inherent complexities of the process of economic growth, there is a wholesome and gathering tendency to seek general insight,

if not formal theory, by examining the comparative historical experience of different societies. Professor Youngson's book is an interesting exercise in this essentially biological approach to economic analysis.

The argument is developed in three distinct sections.

Part I examines a portion of the literature concerned with growth, historical and contemporary. It concludes by isolating these three groups of factors judged to be of "first-rate importance": "first, inventions and the adoption of new ideas, especially such as to encourage extensive further innovation . . .; secondly, the supply of enterprise; thirdly, the drift towards massive production based largely on the realisation of economies of scale." He then defines his model (pp. 96-7), assuming as initial conditions that "a country [is] well-governed, not devoid of natural resources, on the whole anxious for economic progress; but with production still small and the people poor." The model suggests that an initial widening of the market, either spatially or by technological innovation, can lead on to a self-sustained growth process in which further technological innovations, enterprise and the organisation of large-scale production play their roles.

Part II, embracing about half the text, examines periods of accelerating economic progress in four societies: Great Britain, 1750-1800; Sweden, 1850-80; Denmark, 1865-1900; the American South, 1929-54.

Part III draws conclusions based on uniformities or insights to be derived from the historical cases; and then provides four observations, rooted in the author's historical perspective, on the contemporary problems of under-developed areas.

The major uniformities perceived by Professor Youngson are: "the advantages of international contacts; the long history and pervasive importance of agricultural reform; and the complexity and significance of inter-trade and inter-sectoral relationships," notably the interweaving of agricultural and industrial development. His prescriptions are, broadly speaking, that governments in under-developed areas should try to conduct liberal trade relations with the rest of the world and avoid committing themselves to excessive tariffs; that they should concern themselves less with specific projects in the fields of both industry and overhead capital and more with the broader environment of the economy (*e.g.*, education and fiscal policy); that they should not neglect agriculture; and that they "should give more attention than they usually do to the creation of a more tolerant and flexible social system, and one which will be more sympathetic to economic progress."

The quality of this exercise is weakened by several characteristics of the exposition. First, the four cases examined are distinctly odd, if the author's objective was to conclude with lessons relevant to the strategic problems of the contemporary world of under-developed areas. The four historical passages examined relate to countries or regions where, on the whole, the transition from a traditional to a modern society was relatively easy; and,

even then, the most relevant periods in the history of these societies, for contemporary analysis, lie in earlier phases than those he presents. Professor Youngson is describing what I would call four take-offs; but most of the under-developed areas are still in the process of creating the preconditions for take-off. The ardent and difficult transformations going forward in Asia, the Middle East and Africa, upsetting and supplanting traditional societies, have by no means yet created that good government and consensus concerning economic progress which Professor Youngson blandly assumes in his initial conditions. If one is to bring the richness of historical experience to bear on the contemporary scene, one must look, for example, to seventeenth- rather than to eighteenth-century Britain; France and Germany before 1850; Russia before 1890; to certain of the Latin American cases before the mid-1930s; to China before 1949; and, above all, to the story of Japan before 1880.

Second, there is a distinct gap—a lack of linkage—between Professor Youngson's formal model and his historical narratives. Although it is exceedingly difficult to tell the story of a living economy in terms of any model, the lack of analytical shape in Part II is excessive. It might easily be read as summaries of the existing literature, without any clear sense of the variables which Professor Youngson regards as key to the growth process. One misses, particularly, incisive accounts of how developments in one sector lead on, through economies of scale and the other Marshallian long-period forces (which the author correctly emphasises in Part I), to new levels of technology and organisation.

Finally, the author's model is not sufficiently well formed to make quite persuasive some of the legitimate conclusions he draws. As any historian must, he reacts against analyses which deal with investment in the aggregate; and he looks to sectoral developments and their inter-connections. He senses that industrial development depends on prior or concurrent expansion in three non-industrial sectors: agriculture, foreign or inter-regional trade, and social overhead capital. It is possible to state the broad inter-connections between these sectors, in the stages of preconditions and the take-off, with reasonable precision; and each of his four cases might have been used to illuminate those interconnections with a clarity which is now lacking. In short, with a somewhat different (but not inconsistent) model, and historical narratives more rigorously disciplined around the questions it posed, the issues Professor Youngson raises in Part III might have flowed more continuously from Parts I and II.

On one point Professor Youngson must make up his mind. He evidently finds the turbulent political and social changes and conflicts that accompany the break-up of a traditional society and the gradual creation of a modern successor confusing and, even, a shade repellent. Men in these transitional societies do not always behave with the clarity of purpose and Victorian good sense which Book IV of Marshall's *Principles* would suggest is likely to maxi-

mise the rate of economic growth. The author focuses, in the end, on the more antiseptic aspects of the process of economic growth; and he has chosen his cases and periods to make that possible. He has every right to exercise this discrimination; but, as an historian as well as an economist, it will deny him access to the periods and problems which precede the take-off.

If he should examine these earlier passages in the life of societies he is likely to modify his prescription for the limited role of government investment in underdeveloped areas; for in the preconditions period of most societies emerging from traditional status, the Government must play a much larger role than he suggests in building social overhead capital; in stimulating a productivity revolution in agriculture; in generating exports; and in taking the first steps, in certain sectors, in industry as well.

Cambridge.

W. W. ROSTOW

The Keynesian Theory of Economic Development. By KENNETH K. KURIHARA. (London: Allen & Unwin, 1959. Pp. 219. 21s.)

IF I eat too much I grow fat. D = correct diet; E = what I eat; W = my weight; s = weight increasing propensity of excess food. Then

$$\Delta W = s(E - D)$$

The mathematical form of expression is supposed to increase precision. As it is used by many economists nowadays it is whitewashing begged questions. We may be told that K is a stock of physical capital, Y a flow of physical output and S net saving (which can only mean income in value terms, after allowing for depreciation, less the value of expenditure on consumption). We accept $S/Y = s$, and go gaily on to $sY = \Delta K$ and so forth. But it is really too much when we come to $Y_a/K_a < Y_i/K_i$, where K_a is the physical capital in agriculture and K_i is industry (p. 130). It would be much better to use pictograms. If we had to draw corn over ploughs and cloth over looms we should see that, while proportionate changes in them are numbers that can be subject to equality or inequality, the ratios themselves are not.

Mr. Kurihara is not to be blamed. This is the way economics is taught nowadays. Keynes' theory has been reduced to the same idle patter of definitions (written out to look like equations) as the Quantity Theory which it superseded.

Mr. Kurihara has a genuine concern for the problems of underdeveloped economies, and when he allows common sense to break in he draws attention to a number of important points—for instance, that under *laissez-faire* conditions underdeveloped economies are liable to suffer from cyclical "Keynesian" unemployment on top of chronic "non-Keynesian" unemployment, and that it is deleterious to use remedies for the first which will make it harder to deal with the second. His criticism of the United Nations report is forthright and shrewd.

He is subject, however, to a number of blind spots. With given productive capacity, and given propensities to save, a higher level of investment entails a lower real-wage rate and a higher ratio of profit to income. He appears to take this to mean that low real wages are a cause of investment so that, for instance, an increase in monopoly promotes accumulation (p. 77).

He is unable to distinguish between a technique which is superior to others because it offers a higher output both per unit of labour and per unit capital, and one which is either less mechanised than another, and offers a higher output per unit of capital against lower output per head, or more mechanised and offers a higher output per head against less output per unit of capital. This leads him into a number of confusions and absurdities (though he is sufficiently candid to quote several critics who have already pointed this out, p. 95).

He considers as optimal the rate of growth which just prevents unemployment from increasing, and so is against a policy of reducing the open or disguised unemployment that already exists (Chapter III and *passim*). The last doctrine, presumably, he does not really mean. He has evidently been trapped by his formulæ into saying something that he cannot possibly believe. This also could be avoided by using pictograms.

JOAN ROBINSON

Cambridge.

Economic Policy: Principles and Design. By J. TINBERGEN. (Amsterdam: North Holland Publishing Company, 1956. Pp. xxviii + 276. 53s.)

THIS book is best described as an attempt to show how economic analysis can contribute to the design of economic policy. Professor Tinbergen classifies policies into *quantitative* policy (for example, a change in tax rates), *qualitative* policy (a change in the tax structure) and *reforms* (for example, a new social-security system). In the first case, both the means and ends of policy can usually be defined quantitatively; and most of the book is concerned with analysis of the relations between such ends and means. The main objects of such policy happen to be the maintenance of high employment, a degree of price stability and a certain balance in the balance of payments. In effect therefore the book is concerned largely with "stabilisation policy"; and derives additional interest by reflecting the experience in these matters of the Netherlands authorities, of whose Centraal Planbureau Professor Tinbergen was long Director.

His present book is not easy to criticise. It appears to be intended chiefly as a kind of manual for the use of "many young economists working in the field of economic policy and planning." It is not laid out for ease of continuous reading. The text discusses in turn some twenty algebraic models of increasing elaboration; the models themselves, however, are set out separately in a lengthy appendix; and the symbols employed in them are defined in yet another appendix. The emphasis is on the form of the

equations, and the methods of solving the set problems. If numerical values are attached to the parameters, they are presented either as well-known or as plausible assumptions. At times therefore difficult matters are passed over very briefly: no one would guess, for example, from what is here said about the effects of changes in interest-rates and in the volume of money (the *latter*, not the *former*, are stated to affect expenditure), how debatable it is.

This general method of discussing stabilisation policy, detailed criticism notwithstanding, has over-riding advantages. The effects of economic policy depend upon the reactions of the economy which the authorities have to "play": Professor Tinbergen's method consists in specifying these reactions fairly precisely.

His more sophisticated models may be transcribed roughly as follows. Output depends on home demand and on world demand for Dutch exports. The volume of imports and employment depends on output. Prices depend on import prices, labour costs and the level of output. Export prices depend also on world prices, and the volume of exports depends also on their price. An increase in direct taxes (or a cut in Government expenditure) will thus reduce output, employment, imports and prices, and so increase the volume but reduce the price of exports—on balance improving the balance of payments. Holland is probably the only country this side of the Iron Curtain where wages are a "policy variable": a wage cut reduces consumption (and thus imports), and by reducing prices to some extent favours exports. Exchange depreciation has somewhat similar effects except that it raises internal prices, and, while affecting export receipts more, also increases the cost of imports.

Consideration of a system of relations of this sort enables Professor Tinbergen to deduce certain formal rules for the design of economic policy—a line of thought which has since been independently followed by Dr. Bent Hansen in part of his *Economic Theory of Fiscal Policy*. It will be clear from the account already given that *each* of the "instrument variables" considered (tax rates, exchange rates, wage rates and the volume of money) affects two or more of the "targets" (price stability, full employment, balance on external account). First, then, it is impossible to regard one "instrument" as *the* means of achieving any particular object. Second, the "efficiency" of different instruments with respect to different targets differs. Hence the optimum combination of measures depends on the exact combination of objects it is sought to achieve: where the authorities have many ways of influencing the economy, the choice is quite complex.

The truth, however, deserves to be stated with some emphasis that for most of the time the problems of "stabilisation policy" are in fact logically very simple. Most governments cannot fix wage levels directly; and changes in exchange rates can only very rarely be made. Most governments therefore have only *one* route to influence the "balance" of the economy.

Both fiscal and monetary policy work chiefly *via* their effect on the level of total demand; and the balance of payments and prices (the possibility of indirect tax variations notwithstanding) are subject to influence chiefly in this way only. A problem of *this* type, as indeed Professor Tinbergen here admits, hardly requires algebra. In practice, too, the problem is often cruder still. Year-to-year developments in the economy usually require only relatively minor changes in policy, the more remote effects of which must therefore be very small. The possibilities of exact calculation are also greatly reduced, not only by partial ignorance of the effects of changes of policy, but even more by the inexactness of economic forecasts. If it is uncertain how rapidly prices would rise or what the balance of payments would be on *present* policies, it is impossible to calculate back from a specified "target" to an exact definition of the change required in policy.

While Professor Tinbergen's discussion shows how economic policy depends on a fairly complex interaction between numerous relationships, it does not bring out the important fact that our knowledge about these relationships is still poor. No one who values Professor Tinbergen's own labours at research could doubt his awareness of the need for much more: nevertheless, an impression is apt to emerge that most of the empirical propositions discussed have now been well established. It at present may almost be said that the art of economic policy lies less in knowing, than in knowing how far to distrust, what is known. In application some pretty tall edifices may have to be built on slender foundations; and it would have been most valuable to have had from Professor Tinbergen an assessment of the firmness with which various practical conclusions can be drawn.

Most of the above criticisms apply with far less force if the book is considered not as a treatise but as a class book, or manual of examples. There is little doubt that a student working through the examples would get a feel of the behaviour of the economy; would begin to see national-income aggregates not as arbitrary totals, but as part of an inter-related system; and would pick up a good impression of the probable size of the various responses.

The more general aspects of economic policy fit less happily into Professor Tinbergen's mathematical mode of treatment. The opening discussion of the *aims* of economic policy is apt to appear out of place in a book that adopts the decimal system of paragraphing (for instance in a discussion of "inconsistencies": "Generally speaking, too many young people choose too little education, and care too little about family planning . . ."). The discussion of *qualitative* policy and *reforms* is perforce far less comprehensive than that of *quantitative* policy; and deals in turn with various unrelated questions, which sometimes cannot be very precisely treated, of which quantitative restrictions, and the price policy of public monopolies, are examples.

It is therefore as a discussion of stabilisation policy that Professor Tinbergen's book must be judged. The old subjects of "fiscal policy" and

"monetary policy" have by now long since burst their banks; and whatever criticism may be made of it, this is a thoroughgoing and valiant attempt to treat systematically the vast territory into which they have spread. Essentially it is an expansion of two earlier booklets (*On the Theory of Economic Policy*, 1952, and *Centralization and Decentralization in Economic Policy*, 1954), of which less notice appears to have been taken than they deserved.

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Policy against Inflation. By R. F. HARROD. (London: Macmillan, 1958. Pp. xiii + 257. 24s.)

SIR ROY HARROD, finding that his views on the matters referred to Lord Radcliffe's Committee on the Working of the Monetary System would run to volume length, decided very sensibly to put them before the public in a volume.

Following the lines of Keynes's *General Theory*, he nevertheless departs from Keynes (as in his article in the *ECONOMIC JOURNAL* for March 1956) in laying stress on unsatisfied demand, manifested in "long delivery dates and backlogs of unfilled orders" (p. 102), as a symptom of inflation, when there may be no such rise of prices as Keynes assumed.

He also qualifies Keynes's use of the rate of interest, and extends it to mean "the difficulty of borrowing" (p. 69). He even supposes the difficulty of borrowing to be measurable, so that a high cost of borrowing, "to cover all the incidental costs and the imponderables," might be 15%, or a low cost 10% (p. 71). And he supposes the marginal yield of investment to be equated to such a cost. Except for small short-term borrowings or for borrowers of weak credit, the "incidental costs" cannot be considerable, and the "imponderables" which Sir Roy Harrod has in mind must be the reluctance of lenders. Banks are reluctant to lend when they fear that their advances may exceed the limit allowed (in conformity with prudence or practice) by their liquidity proportion. If they then keep down the total of advances by refusing some applicants, "difficulty of borrowing" results.

A "credit squeeze" limited to a restriction of advances does not meet Sir Roy Harrod's views. He would rely for monetary control on changes in the quantity of money. Such changes "have an effect on the difficulty of borrowing, and this may be expected to bear upon investment orders and also on orders for consumer durables" (p. 235).

The practice of inducing a reduction of the quantity of money by restricting the banks' cash basis has ceased to be effective, because the banks can replenish their cash by reducing their holdings of Treasury bills. It is not the cash proportion but the "second line liquidity ratio" (cash, *plus* bills), that is now relevant (p. 182). Sir Roy Harrod would deprecate "the imposition of a compulsory and variable second line liquidity ratio," since

1959]

it would compel the banks to restrict advances; "this is precisely the system that we want to get away from" (p. 237). His solution would therefore be a reduction of the supply of Treasury bills. That can be done either by a budget surplus or by funding. A budget surplus would only act slowly (pp. 235 and 240). Even funding, if pressed too fast, might so depress the prices of Government securities as to impair the country's credit in foreign eyes (pp. 185-6).

A policy against inflation working through "the difficulty of borrowing" seems very imperfect. One is tempted to comment that its imperfection is due to the exclusion of the *direct effect* of the rate of interest. If the bare rate of interest has to be supplemented by "difficulty of borrowing," recourse must be had to relatively slow-acting budget surpluses and funding, with the drawback of awkward reactions on tax-payers and the investment market. Scepticism as to the effect of the rate of interest on borrowing for the purchase of goods for the replenishment of stocks goes back to Keynes. But as Sir Roy Harrod points out, investment, as understood by Keynes, "includes stocks, which also have their prospective yields, net of recoupment and the cost of storage" (p. 69). Surely the bare rate of interest is not negligible in comparison with this yield. It is because a rise of Bank Rate reacts quickly on the turnover of stocks, while the effect of long term capital outlay (whether direct or through the long-term rate of interest) is slow and uncertain, and in the end not great, that the former effect is the vital factor in credit policy.

Credit restriction is a corrective of an internal inflationary tendency. Sir Roy Harrod also approaches his problem from the external standpoint. His chapter on the Devaluation of Sterling starts by pointing out that devaluation "cannot be regarded as part of a policy against inflation; rather it was an act of policy *for* inflation" (p. 125). By intensifying the demand for exports, it increases "the aggregate demand with which the supply potential of the home economy is confronted" (p. 127). There follows a masterly analysis of the effects of the devaluation on prices and wages, in the course of which he points out the adverse effects of delivery delays and the under-pricing of exports on the balance of payments. In 1952, when there were rumours of an upward revaluation of sterling, he wrote: "failing revaluation, prices, wages and other costs are likely to pursue an upward course" (p. 140). A statistical comparison of British and American prices and wages (p. 225) appears to show that British exports are still under-priced. Nevertheless, he "would not affirm dogmatically that the British wage and price structure is out of equilibrium (too low)" (p. 216), and he has dropped any reference to an upward revaluation as an alternative to a further rise of prices and wages.

Sir Roy Harrod might perhaps have pursued the subject of the relation of the wage level to the rate of exchange a little farther. It is deeply involved in permanent policy. "It has been said," he writes, "that in place

of the gold standard we now have a wages standard," and he observes that "there is an element of truth in this idea" (p. 209). Perhaps a stable wage-level, with a monetary policy just maintaining full employment, would be the most desirable form of stabilisation. But it would not be consistent with adherence to some other standard. It would not be consistent with unvarying rates of exchange, unless the stable wage standard were adopted internationally. In fact, so long as we tie the pound to the American dollar at a fixed rate, we preclude ourselves from having any other monetary standard, and it rests with the American authorities to determine whether we are to suffer from inflation or depression. Sir Roy Harrod does not face this issue. He rejects a "freely floating sterling," on the ground that unless there are fixed rates of exchange a high Bank Rate will not attract foreign money. Surely not a good reason, for hot money is but a precarious reinforcement of the reserve.

A closely reasoned chapter deals with the foreign-held sterling balances, and a most illuminating table (p. 243) shows the progress of a reserve calculated from gold and dollar holdings, net of sterling held outside the Sterling Area, from 1945 to 1957.

If Keynes's monetary doctrines are still counted as orthodoxy, this book is an able and lucid exposition of orthodox theory, with modifications essential to adapt it to the conditions of the present time.

RALPH HAWTREY

London.

Public Principles of Public Debt. By J. M. BUCHANAN. (Homewood, Illinois: Irwin, 1958. Pp. xii + 223. \$5.00.)

THIS book is a significant contribution to debt theory literature. Even if one has doubts about the importance of some of the propositions the author is concerned to refute or establish, there can be no two opinions about the clarity of reasoning or exposition, which are both of an exceptionally high order.

The author explains that after a year's sojourn in Italy studying the works of the great Italian writers in public finance, he has come to reject much of what he thinks is currently accepted debt theory. His basic concern is to establish the "primary real burden" resulting from a marginal issue of debt. He analyses in turn the classical full-employment world and the Keynesian under-employment situation. In each case he considers the relative effects of raising a given sum of revenue by taxation, debt-creation and currency-creation. In addition, the case of debt-creation to finance extra expenditure is examined at some length.

The principal conclusion which comes out of the analysis is that the burden of debt rests on future tax-payers. As a corollary, Professor Buchanan argues that we must reject three commonly accepted propositions in debt theory: first, that the primary real burden of debt is borne by the

current generation; second, that public debt is different in nature from private debt; and third, that external debt is different in nature from internal debt.

These are sweeping conclusions, and perhaps one or two illustrations of the reasoning should be given. In a full-employment world, the author argues, the creation of public debt to finance additional government expenditure will simply mean that some people who would have lent funds to the private sector will lend instead to the public sector. It is impossible to say that lenders suffer thereby, as it is a voluntary transaction; nor is there any burden on the present generation of tax-payers. It is concluded therefore that the burden of the debt must rest on tax-payers in future years, who will have to pay more in taxation than would otherwise have been the case. In a Keynesian under-employment situation the additional government spending necessary to push up the level of employment could be financed either by debt creation or currency creation. If the Government chooses the former method, a "primary real burden" will fall on tax-payers in future years; and this would not be so if the latter method of finance were chosen. The internal-external loan comparison can be summed up in this way: internal borrowing may increase capital formation in the public sector at the expense of that in the private sector (under full-employment conditions), whereas an external loan will allow additional public capital formation without reducing that in the private sector. But the additional income generated thereby will be exactly offset by external interest payments, and so it is concluded that external and internal loans are essentially similar in nature.

Various other conclusions follow from this sort of reasoning. Some of the most penetrating are to be found in Chapter 12 ("When should Government Borrow?"), where it is argued, for instance, that it is not appropriate to finance government current expenditure by borrowing, on the grounds that the burden of such borrowing will rest on future tax-payers and it is therefore an act of political chicanery to finance expenditure benefiting the current generation in this way.

Enough has been said to illustrate, if not to catalogue, the subject matter of this book. What sorts of objections can be raised against these kinds of arguments? First, it is not altogether clear what the author means by the often-repeated phrase, "the primary real burden of debt." Presumably he means to concentrate on those who suffer adversely in any income re-distribution resulting from the marginal issue of debt. But the concept of income (personal, private and so on) is not closely specified; and it is difficult to see what is the significance or usefulness of "primary"—surely it is just as important to bring changes in pre-tax incomes as well as tax impositions into account. Secondly, it must be asked whether the sharp distinction between taxation necessary to finance the service of the debt and the corresponding transfer payments of debt interest is always enlightening.

The author seems to rely on two arguments here: first, that future interest payments will tend to be capitalised while future tax payments will not, and second, that in any case the two things are entirely separable. Neither is wholly convincing. Thirdly, the author does not face up to a case where his reasoning seems to break down. Imagine a full-employment situation where the net result of public borrowing is to substitute public for private debt and public for private capital formation. If the public capital formation yields a normal return to the Government there obviously need be no additional taxation to finance interest charges. It is perfectly true that this situation is the *joint* result of government borrowing and spending, but it seems just as legitimate to single out this case as that (p. 40) where government expenditure is wasteful and produces no social income at all.

Another point which the author might profitably have explored is to ask whether in practice the location of the debt burden is all that important. If we assume as a rough hypothesis that debt stays more or less constant in peace-time, then although the sins of the present generation rest on their descendants, they themselves are the legatees of the sins of their own ancestors. It might be plausibly argued on this sort of basis that there is no net handing down of burden from one generation to another generation—leaving war-time out as a different matter. Finally, there is little discussion in the book of all the institutional problems associated with public debt—the fact that a good deal is often held by government departments, the role of short-term versus longer-dated issues, etc. It is never entirely fair to criticise an author for not discussing a different subject from the one he does discuss, but in this case more attention to points of this sort might have been rewarding.

Despite these reservations about some facets of the author's arguments, there can be no question that this is a bold and stimulating book in which the depth of the thought is matched by the clarity of the style.

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A. R. PREST

The Competitive Process. By JACK DOWNIE. (London: Duckworth, 1958. Pp. 199. 25s.)

THIS stimulating and lively book must give satisfaction to people working on the problems of competition and the development of industrial structures. A number of recent industry studies have dealt with the relationship between structure, cost reduction and innovation, and Mr. Downie has now produced a theory which is concerned with these very features. He has turned attention to two particular aspects of economic performance, the dispersion of efficiency between firms, and progress. He contends that, owing to the assumption that technical knowledge is given, economic analysis has failed to furnish a useful framework for thinking about problems of competition and

restriction, and he now offers a new pattern. This owes a great deal to Schumpeter and had a somewhat neglected forerunner in Part I of J. Steindl's book *Maturity and Stagnation in American Capitalism*.

The theory turns upon the "transfer mechanism" and the "innovation mechanism." The transfer mechanism is the process whereby shares of the market are transferred from the less-efficient firms to the more efficient. The more efficient grow mainly by using their higher profits for investment, but they also reduce prices in order to obtain a larger share of the market. The "innovation mechanism" is the process whereby firms change in efficiency by innovating. Here Mr. Downie relies upon the idea that innovation is largely the result of competitive pressure, and he argues that it is the less efficient who innovate most actively because the efficient are pre-occupied with expansion. The theory is that while the transfer mechanism tends to result in greater concentration, this tendency is checked by the innovation mechanism which, by producing new leaders, results in the expansion of different firms at different times.

The theory, while open to much criticism, has a great deal to commend it. It illuminates the analysis of industries in which there is rapid change, with new firms growing, flourishing and then being conquered by other firms which burst into prominence. The book is exciting, and it is attractively written in a light almost conversational style, but it is also provoking, elusive and difficult to interpret. The difficulties, invariably found in this subject, lie in the sweeping generalisation and the fact that each concept covers so much; innovations range from potato peelers to nylon, the transfer mechanism may take a few months or many years. The most serious difficulty is that while the transfer mechanism will, immediately after a major innovation, work smoothly and fast, it may become very slow long before the inefficient have been eliminated. In some industries the transfer mechanism works so strongly as a result of innovation and is so weak in face of market imperfections that concentration is really the result of innovation, and the continued existence of a multi-firm industry is due to the absence of innovation.

The "innovation mechanism" itself is also uncertain; while there are industries in which innovations come from firms which are under pressure to change, there are many in which development comes from firms of the I.C.I. type with large research departments and the financial and technical resources to buy, develop and exchange patents. Where innovations are of this type they are likely to lead to greater concentration rather than off-set it. Although Mr. Downie recognises many of these points, his general thesis is that multi-firm industries remain because innovations are made; it seems equally reasonable to argue that multi-firm industries survive where progress is slow.

Application of the theoretical ideas to the question of restrictive practices suggests that unstable price agreements may well be in the public interest

because, by protecting the profits of the expanding firm during periods of slack demand, they help, rather than inhibit, the transfer mechanism. This argument has much force provided that it can be assumed that the efficient firm will pursue an active expansionist policy and will drive out the inefficient. Mr. Downie's analysis is important in that it questions the general theoretical condemnation of price agreements.

The second part of the book deals with practice, and the broad theoretical propositions are tested against census data on samples of firms from thirty trades. Mr. Downie says that, "This part of the book is to be regarded more as an illustration of the kind of empirical approach which the theory suggests than as anything in the nature of a conclusive demonstration of the validity of its conclusions." This modest claim is well justified, but the data, interesting in themselves, are difficult to assess, and a lot more work could usefully be done to find out the extent to which both the particular history of each industry and the heterogeneity of products and types of firms affects the interpretation of the statistical data.

It is rather easy to criticise this book, but although I constantly found myself disagreeing and arguing, I was, equally often, nodding enthusiastically. This book is a valuable contribution to competitive theory; it gives new impetus to the changes in approach which are taking place.

P. LESLEY COOK

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Price Theory. By W. J. L. RYAN. (London: Macmillan, 1958. Pp. ix + 396. 31s. 6d.)

DR. RYAN offers this book as an account of the pricing process for the student who intends to specialise in economics, and to this end he first describes a model in which all buyers and sellers accept market prices as given facts and plan their own activities in the light of these prices. The treatment is essentially a re-working of the Walrasian system of price determination in terms of graphical methods, modified to take account of the major developments in cost and preference theory of the past thirty years. The account concludes with a useful brief re-statement of the model in terms of algebra.

Then the model is modified to allow buyers and/or sellers to become price makers instead of price takers, and market behaviour is examined on the basis of the new assumptions. This section contains an interesting analysis of oligopoly using Cournot reaction functions, first stating the problem in terms of quantity decisions and then in terms of quality decisions. The translation of all quality differences into a cardinal measurement may not be easy, but the approach should give the student a comprehension of the oligopoly problem which other lines of attack on the problem may have failed to give him.

In the final chapter Dr. Ryan attempts what is surely the impossible task of giving an account of activity analysis and the theory of games in the space of nine pages. It is difficult to believe that the reader meeting these problems here for the first time will find the exposition intelligible.

The general level of treatment suggests that the book is designed as a text for students in the second of a three-years honours-degree course in economics, and, the argument being closely reasoned, so that every step demands comprehension of what has gone before, it is vital that an even level of treatment be kept. The nature of points raised sometimes puts unnecessary strain on the student (*vide* the discussion of the elliptical shape of isoquants on p. 53) and on occasion comment tends to be cryptic. There is surely an easier way of putting the underlying idea than to talk of a zero value of the variable, "the importance of legal and institutional barriers" (p. 276).

Dr. Ryan's chapter on the long-run supply curve shows how very unsatisfactory is the state of this corner of theory, despite the attention it has received since Clapham's "Empty Economic Boxes." Dr. Ryan appears to accept the view that decreasing cost is due to the existence of indivisible agents in the productive process but he has some reservation about what he calls "cost indivisibilities," cases such as that of a 100-h.p. motor, which costs less than 100 times the price of a 1-h.p. machine. His long-run cost curve seems to be that of Marshall, the entrepreneur is never bound by his past actions, and all agents except top management can be varied by any amount, however small, though given unlimited time even management should be variable.

Now, if there is but one indivisibility to be eliminated, it would appear that the average cost curves for different-sized lumps of the indivisible agent would be U-shaped and minimum average cost would in each case be the same. If by any means the agent can be made perfectly divisible the separate curves are replaced by a horizontal straight line and a condition of constant cost has been achieved. If there are two indivisibilities the average cost curves drawn on the assumption of fixed doses of the more divisible agent will hang in a catenary whose shape is determined by the more indivisible agent. Eliminating the lesser indivisibility will give an envelope curve which in effect is the first of a series of single indivisibility curves. Ryan follows the usual practice and stops here, but if the firm really has all eternity to play with, then presumably this indivisibility can be overcome and a constant cost curve emerges.

The only hope of getting a continuous but variable relationship between cost and output seems to lie in Professor Robinson's adoption of Haldane's ideas on "being the right size." Apart from this, we must abandon the assumption of the timelessness of the process and consider changes in technique and in the rest of the economy to arrive at an historical record of the past extrapolated for the future. In addition, the Marshallian long-run

supply curve lacks the essential feature of a supply curve, reversibility. Knowledge gained by handling larger outputs is useful for smaller outputs; institutions once created continue perhaps with changed purpose. The dictum that by-gones are by-gones is not invariably true, and a more rapid time-rate of increase of output may increase the level of costs for periods which are long in relation to production plans.

Even less defensible is the concept of a long-run demand curve, for none of the conditions which define the curve can be taken as constant for long periods of time. We must content ourselves with forecasts of the future positions of short-run curves and the Marshallian long-run curve were better discarded.

The book will be welcomed and used in all schools of economics where competence in the handling of the tools of analysis is demanded of the student.

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Essays on the Welfare State. By R. M. TITMUSS. (London: Allen & Unwin, 1958. Pp. 232. 20s.)

THIS book consists of lectures which were delivered on various occasions between 1951 and 1957, of which four are printed here for the first time. They cover a wide range of subjects: the position of women in society, changes in the pattern of marriage and family life, the social effects of industrialisation and the division of labour, the effects of the recent scientific revolution in medicine on the doctor, the hospital and the patient, the influence of war on the growth of the conception of the welfare state, amongst others.

A recurrent theme that runs through many of them is that of the role and significance of the social services in a modern industrial community. Professor Titmuss is concerned, as he says in his preface, to emphasise the view that "The social services (however we define them) can no longer be considered as 'things apart'; as phenomena of marginal interest, like looking out of the window on a train journey. They are part of the journey itself. They are an integral part of industrialisation."

It can truly be said that there is no subject of which he treats in this book that he does not illumine, and this is not so much by the conclusions which he draws (which are few) as by the questions that he raises—often very disturbing questions, the answers to which for the most part lie still in the future.

In the three concluding lectures, delivered at the Law School in Yale University, he deals with some aspects of the National Health Service in England. Both in content and treatment they were clearly conditioned by the American audience to whom he was speaking. But as they touch on

issues of great importance, such as the professional freedom of doctors, the impact of scientific and technical advances in medicine, the doctor-patient relationship, they are deserving of attention on this side of the Atlantic also.

Professor Titmuss' book should have a special interest for economists, because of the light that it throws on some of the more important possible social implications of those economic forces and changes with which they as economists are primarily concerned.

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Value in Social Theory, A Selection of Essays in Methodology. By GUNNAR MYRDAL. Edited by PAUL STREETEN. (Routledge & Kegan Paul, 1958. Pp. xlv + 269. 32s.)

THIS extremely interesting volume is a companion to Gunnar Myrdal's *Political Element in the Development of Economic Theory*, written in 1930 and published in an English translation in 1953. It consists of a collection of essays, most of them concerned with the relations between value premises (implicit or explicit) and analysis of facts in social investigation. They range from an early essay on "Ends and Means in Political Economy," first published in 1933, to the appendix to *An International Economy* (1956). The central core is a series of extracts from *An American Dilemma* (1944), which viewed narrowly was an exercise in methodology. "Ends and Means" was searching but negative in its conclusions; *An American Dilemma* was constructive. "We attempted to work with explicit value premises, to base them as far as possible on observation," Dr. Myrdal writes, "to stress them throughout the study, and to define the concepts used in terms of the chosen value premises." He recognises that despite the complexity of the subject the task of writing in this fashion about the American negro was simplified by the existence of a single uniform set of value premises. "The real difficulty, however, arises when analysis has to be based upon a number of different sets of conflicting value premises." One set of value premises may be explicitly stated—this is a logical advance on leaving them uncovered—but they are thereby given a strategic advantage. The advantage is put in perspective when it is recognised that the premises are not doctrines but assumptions which can be tested for relevance and significance.

The arguments used by Dr. Myrdal in this collection of writings are illuminated and set in their context by a lucid and scholarly introduction by Paul Streeten and a fascinating and revealing autobiographical postscript by Dr. Myrdal himself. Mr. Streeten starts with "is" and "ought," examines what Dr. Myrdal calls "the crux of all science" ("it assumes in all its endeavours an *a priori*, but its ambition must constantly be to find an empirical basis for this *a priori*"), discusses the basic distinction between and inter-relations of "programmes" and "prognoses," as set out not in a work

on economics but in *An American Dilemma*, and summarises the tasks of the social scientist. Dr. Myrdal himself, who allows little place to history in this collection of methodological writings, contributes in his postscript such a vivid piece of history—the story of his own development seen against the background of Swedish academic and political life—that this volume is worth reading for this brief essay alone. He is explicit about the psychological inspiration of the work of the younger generation of Swedish economists of the 1920s. “The main inspiration was negative: the need to protest against the intellectual domination of the older generation. It led me to a critical scrutiny of the value problem, because I was aware that this was their weak spot.” He goes on to sketch with a few deft strokes the main characteristics of the great men of both the older generation and the new. It is extremely interesting to compare and contrast Swedish development with the different line of development in both Britain and the United States. From the 1930s onwards, however, and particularly since 1939, there have been more and more features in common.

In an essay which stands somewhat on its own, as part survey, part exhortation, “The Relation between Social Theory and Social Policy,” which was delivered as the opening address of the Conference of the British Sociological Association in 1953, Dr. Myrdal considers some of the leading questions relating to the role of the social scientist in post-1945 conditions. That the atmosphere has already changed since 1953 does not detract from the relevance of the conclusions or the statements about long-term trends. Dr. Myrdal, in the light of his varied experience as well as his thinking, directs attention to three facets of the social scientist’s work—his need for “independent status”; his duty to promote “rationality” (a duty which might well be laid down in a code of professional ethics); and his “influence,” how and where it can be exerted. Most of the other extracts in this volume are concerned with method, but in this lecture the case is argued not only for “persistent and incisive methodological scrutiny” but also for “uninhibited public discussion on the highest level of the broad issues involved.” There are two sides to the social scientist’s communication. “We must not only master the art of writing well and forcefully, and sometimes do so in terms so simple that we can be understood by the general public, but also, at other times, become so involved and intricate that we cannot possibly be followed by others than our peers.”

This is a searching and stimulating book which puts economics in its place in the social studies, explores values, and touches on questions of “judgement.” It is, as Dr. Myrdal would see it, a product not of political economy but of the “Enlightenment.” Yet the modern world has been fashioned by many other forces. The social studies are still unequally developed and imperfectly related to each other, in practice they often lend themselves to manipulation rather than to understanding, prejudices move men and ideologies conflict, social forces erupt. Against this background

there is need to assess Dr. Myrdal’s approach. “By abandoning the claim to absolutism, universality, and pure ‘science,’ a way is opened to a more limited but also more rational, to a more modest but also more objective, approach to the study of society.”

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The Theory of Committees and Elections. By D. BLACK. (London: Cambridge University Press, 1958. Pp. xiii + 242. 30s.)

THE first part of this book contains a systematic and formal account of the theory of committees and elections. In the second part Professor Black enumerates the contributions made to the subject since it first attracted interest in the eighteenth century. If the earlier discussion is probably the more valuable, the latter is the more entertaining, especially when concerned with the issues that excited the Reverend Charles Dodgson to make his substantial contributions on the eve of meetings of the Governing Body of Christ Church.

The theory of committees can be made most explicit when the committee members all possess “single peaked” preferences. This means that motions before the committee can be arranged in a certain order, such that movement away from the most-preferred motion of each member in either direction leads towards motions that are steadily less attractive. Preferences are likely to satisfy this condition when the motions are concerned with possible sizes of some entity, or when they vary steadily in political complexion from Left to Right. It is, perhaps, a weakness of Professor Black’s discussion that he does not discuss in more detail the full conditions necessary to ensure single-peakedness of preferences.

If single-peakedness is assumed, the geometry of the analysis is comparatively simple, and Professor Black shows that, if any one of three committee procedures are used, the ultimate decision of the committee will be the same. Broadly speaking, the decision reached will be that most favoured by the “middle” member of the committee, when due allowance has been made, when necessary, for the chairman’s casting vote. The outcome of voting becomes much less definite if for most members there is a range of most preferred motions between which they are indifferent.

When there are no restrictions on the preferences of members, we at once run into a much richer field of possibilities. With the committee voting by a simple majority on all pairs of motions in turn, there is no assurance that the committee itself will possess a transitive system of preferences. The phenomenon of cyclical majorities may manifest itself, with the committee preferring A to B, B to C but C to A. With normal committee procedure a motion enters the voting only once and is discarded if another motion beats it by a simple majority. In such circumstances the later a motion enters the field, the greater is its chance of ultimate adoption. Furthermore, some

members of the committee may be able to obtain a result more favourable to them, than would otherwise be obtained, by voting contrary to their own system of preferences.

The resolution of difficulties on a committee is not insuperable, because members can allow their voting to be influenced by others so that deadlock or unsatisfactory compromise is avoided. The problem is much less easy in elections involving large numbers of electors. What candidate should any voting system aim at electing? Professor Black accepts, without much question, that it should be the candidate who stands the highest *on the average* in the electors' preferences. A candidate who could obtain a majority over each of his rivals would satisfy this requirement, but such a candidate does not necessarily exist. Condorcet favoured the election of the majority candidate; but, when there is no such candidate, some account ought to be taken of the size of each candidate's majority or minority when put against each of his rivals. This is done by a system of marks by the so-called Borda criterion.

When voters' preferences are single peaked, exhaustive voting—voting on all candidates pair by pair—returns the majority candidate; but the single transferable vote does not ensure his election nor does it permit a voter to show indifference between one candidate and another. Professor Black is unable to arrive at a satisfactory scheme of proportional representation. The use of the (Droop) quota is helpful but not infallible, and he is critical of the heterogeneity and arbitrariness involved in the later stages of the procedure, due to the mixing of first, second, third, etc., preference votes.

In the remainder of the first part of the book Professor Black discusses the theoretical modifications required when committees need a special majority to arrive at a decision. He also makes use of the concept of elasticity when examining the sensitivity of a committee's decision to alterations in the size of the special majority or in members' preference schedules. I do not feel that the analogy with economics is altogether happy, and it seems curious to suppose that these two chapters will be comprehensible only to readers with a knowledge of economics. The effect of complementarity of motions is then discussed; this emphasises the drastic assumptions about preferences which have been necessary to ensure earlier simplicity of treatment. Finally, a short chapter discusses the implications of the earlier analysis to international agreements and cabinet responsibility.

In the very interesting account in Part II of the contributions made by Borda, Condorcet, Laplace, Nanson, Galton and Dodgson, Professor Black severely criticises Condorcet for hiding his substantial addition to knowledge under an unnecessary superstructure of probability theory. This criticism may, in the context in which it is delivered, be well-founded, but Professor Black himself seems to be open to an opposing counter charge. Committees must make decisions; and we can scarcely hope that infinite time and effort can be devoted to achieving what appears to be full justice to the differing

views and preferences of their members. A practical scheme of committee or electoral procedure must surely aim to minimise the chance of serious injustice for a given cost. It seems therefore that probability theory is not wholly irrelevant to the subject under discussion.

Professor Black is aware of the limitations of the assumption that voters go to the polls or committee rooms with a fully determined system of preferences. Is there not room for the supposition that, apart from motions on which they possess strong feelings, chance may well influence their voting on other motions? A voter's strategy at a committee may well be conditional on the anticipated voting behaviour of other members; is not the probable revision of his strategy as business proceeds susceptible of treatment with the aid of probability?

It is unfair to expect answers to these difficult and intricate questions, but it is not unreasonable to suggest that future progress in theoretical development may depend on the re-introduction of some probabilistic assumptions. As it is we must be most grateful to Professor Black for a comprehensive and incisively clear account of the existing deterministic theory.

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The Life of Knut Wicksell. By TORSTEN GÄRDLUND. Translated from the Swedish by NANCY ADLER. (Stockholm: Almqvist & Wiksell, 1958. Pp. 355. 30 kr.)

Selected Papers on Economic Theory. By KNUT WICKSELL. Edited with an Introduction by ERIK LINDAHL. (London: Allen & Unwin, 1958. Pp. 292. 30s.)

SCHUMPETER, in his *History of Economic Analysis*, says that Wicksell, apart from his activities as a vigorous champion of neo-Malthusianism, lived the life of "a quiet and retired scholar." This happens to be the reverse of the truth. Wicksell was a fearless and passionate advocate of a number of unpopular causes, an *enfant terrible* who was in trouble of one kind or another throughout his long life. He had the courage of his convictions to an extent which at times made him the despair of his friends. Anyone who judged him merely from the mathematical austerity of his contributions to economics would have no inkling of his real character.

At last we have in English a most revealing study of the life of Wicksell. Professor Gärldund's book is a slightly abridged version of the admirable biography which he published in Swedish in 1956 under the title, *Knut Wicksell, Rebell i det nya riket*. His most valuable sources were the Wicksell papers in the University of Lund, including over 2,000 letters to Wicksell from about 500 correspondents and about the same number written by him, mainly to his wife, his sisters and brother and his two closest friends.

Knut Wicksell was born in Stockholm in 1851, the youngest son of a provision dealer. His mother died of consumption when he was seven years of age, and he lost his father when he was fifteen. Writing of his childhood years when he was fifty, all he could say was that they were "not too bad on the whole." Sensitive and deeply religious, he went up to Uppsala University in 1869 and graduated in 1872 with distinctions in mathematics and astronomy. At the age of twenty-three, after a profound religious crisis, he became a free-thinker and was strongly attracted by the ideas of Ibsen. During these years he suffered from a sense of loneliness and of not belonging. In a paper written in 1887, *How I Became a Free-thinker*, he wrote of his intense preoccupation with the problem of finding a moral code not based on religion. A turning-point came in 1879, when he read a Swedish translation, published anonymously, of Drysdale's *The Element of Social Science*, with a characteristic sub-title, *Physical, Sexual and Natural Religion: An Exposition of the True Cause and Only Cure of the Three Primary Social Evils—Poverty, Prostitution and Celibacy*. He was soon the centre of violent storms as a result of lectures he gave on "The Most Common Causes of Habitual Drunkenness and How to Remove Them" and on "Celibacy or Prostitution?," in which he stressed the evils of overpopulation and advocated birth control. On many subsequent occasions his forthright denunciations of conventional morality aroused the most bitter controversies and made him numerous enemies. Neo-Malthusianism became his religion, and he seems to have suffered less from fits of depression and emotional tension when, in his own words, he "had found something to live for."

What led this rebellious Ibsenite to turn to economic theory? After a careful scrutiny of letters which have only recently come to light, Professor Gårdlund reveals that at the end of 1884 Wicksell, then living with his friend, Öhrvall, fell desperately in love with Mrs. Öhrvall. This was the reason why he went abroad to London in the summer of 1885 and stayed there for a year. He spent months in the British Museum reading widely in the classics, particularly Malthus and J. S. Mill, Jevons, Walras and Sidgwick's *Principles* ("an excellent book which does justice to everyone"). He then went to Vienna and made contact with Menger and got hold of a copy of Böhm-Bawerk's *Geschichte und Kritik des Kapitalzins-Theorien*. If it had not been for the emotional crisis of 1884, the world would probably never have seen *Värde, kapital och ränta enligt nyare nationalekonomiska teorier* (the first version of *Über Wert, Kapital und Rente*), the lectures delivered to the Workers' Association in Stockholm in 1889. Like Marx, Wicksell found his intellectual inspiration in the British Museum; but unlike Marx, he was driven there by unrequited love.

In campaigning for causes dear to his heart, Wicksell was stubborn, tactless and insensitive to the pain he inflicted on members of his family and his friends. For example, when his sister begged him not to publish a pamphlet mocking various religious beliefs, he wrote to a friend that, whilst it would

not be pleasant to feel afterwards that his sister had gone out of her mind over the affair, "I nevertheless feel that it would not be *right* to withdraw; I have fewer ties than most people, and any way the battle has to be fought. It will simply become more bitter and demand more victims, the longer it is delayed." When he married Anna Bugge, he absolutely refused to have a wedding ceremony. "I believe," he said, "I should have many more unhappy half hours in my life if I acted against my convictions now." In applying for a permanent Professorship at Lund, he refused to write "Your Majesty's most obedient servant." For uttering blasphemous remarks in a lecture in 1908 he was sent to Ystad prison for two months. (The prisoner's cuisine was more than adequate; in the Wicksell Archives there is a letter from the Hotel Prins Carl, which supplied his meals, asking "whether the Professor desires goose and black broth on Michaelmas Eve? If black broth is not desired, we will send dessert instead.") It was against the grain for Wicksell to promise his wife, in reply to her appeals, "to be a scholar . . . and leave the affairs of the country to take care of themselves."

It is not surprising that this stormy petrel had reached the age of fifty before he had been admitted even to a provisional Professorship and he had to wait until he was seventy before he obtained any mark of academic honour in Sweden. He must have been bitterly disappointed that he was never made a member of the Academy of Science. When he got his first appointment in Lund, he wrote, "it is so strange to be, as it were, a cog in society's wheels, instead of being an outsider looking in." Wicksell's greatness as an economist had been recognised on the Continent when *Über Wert, Kapital und Rente* appeared in 1893. Walras declared that Wicksell had "taken his place in the foremost rank of the new generation of mathematical economists, among whom we can already reckon Irving Fisher in New Haven and Marquis Vilfredo Pareto, my successor at the University of Lausanne." His famous *Interest and Prices*, published in German in 1898, was not well received. "The only consolation," writes Professor Gårdlund, "was that the ECONOMIC JOURNAL which, in a supercilious article a few years earlier, had dealt summarily with his book on price theory, now printed a warmly appreciative review of the new work." It was a long time before the full significance of Wicksell's achievement had dawned on the English-speaking world. Edgeworth, in his day, had denied him an opportunity of defending himself and Böhm-Bawerk against an attack in the ECONOMIC JOURNAL; and in the 1920s Keynes refused a long article by him. In the summer of 1925 Wicksell wrote to his wife: "Yesterday evening and today I have been reading a long article on Marshall by Keynes, quite interesting but so absurdly 'English,' as if no economists existed outside England."

Professor Gårdlund has produced a fascinating portrait of a remarkable personality. The readable quality of the original Swedish has been maintained in the excellent translation by Nancy Adler. The author has done more than portray the many-sided character of Wicksell: he has indirectly

revealed the nature of the political and intellectual forces which were transforming Swedish society in the period of its industrialisation.

Included in *Selected Papers on Economic Theory* are a sample of Wicksell's articles hitherto not available in English. Professor Lindahl has fittingly prefaced them with a translation of his survey of Wicksell's life and work which was published in *Ekonomisk Tidskrift* in 1951 to mark the centenary of his birth. Not only does this give an authoritative assessment of the place of Wicksell in the history of economic thought but it also throws invaluable sidelights on his character.

Since the main writings have already been translated into English, the choice of material for this volume must have been a problem for the Editor. The papers that are new to English readers are "Ends and Means in Economics" (1904), reviews of the work of contemporaries, *i.e.*, Pareto's *Cours* and *Manuel*, Böhm-Bawerk's *Theory of Capital*, Menger's *Grundsätze* and Bowley's *Mathematical Groundwork of Economics*, and three essays on international economics, "The Riddle of Foreign Exchanges" (1919), "Protection and Free Trade" (1924) and "An Object-Lesson in the Tariff Question" (1925). Connoisseurs will find all these much to their taste. The review of Bowley's book under the title "Mathematical Economics" (1924) is still worth reading as a statement by a great pioneer of the mathematical method. The famous inaugural lecture, "Ends and Means in Economics," delivered at the University of Lund in 1904, is the testament of an optimistic utilitarian. He blamed "scientists who do not come to the defence of scientific truth with sufficient strength and ardour—obviously because their hearts are not always on the same side as their heads, their sympathies being in conflict with their scientific convictions." These "scientific convictions" meant something to him, because he believed that there was "a goal of economic activity . . . with which everyone is in formal agreement." He ends in a cloudy peroration:

"if, as I hope and believe, one day we recognise that our goal here on earth is to extend the greatest possible happiness to all, of whatever class of society, race, sex, language, or faith they may be—then the pleasant discovery will be made that the economic side of this problem has already been solved in its essentials, and that the solution only awaits practical application."

Wicksell was convinced that his prescriptions for economic policy were based on abstract truth: he could not see that they rested on a particular value postulate.

The rest of the volume is taken up by articles on "The Influence of the Rate of Interest on Commodity Prices" (1898), "Marginal Productivity as the Basis of Distribution" (1900) and "The Problem of Distribution" (1902). The ideas in these papers are well known in their more elaborate form in *Interest and Prices* and in the *Lectures*; it would have been more satisfying to have statements of Wicksell's views on the fundamental difficul-

ties involved in these theories. For example, an acute criticism of the argument in *Interest and Prices* was made by Davidson; it is a pity that a translation of Wicksell's reply, *e.g.*, "Penningränta och Varupris" (*Ekonomisk Tidskrift*, 1909) was not included in this book. Then, on marginal productivity Wicksell seems to have had doubts towards the end of his life. He became worried about whether one can measure the investment period in terms of an "average" and whether it is possible to collect together in a precise measure the waiting periods of different "original" factors. This scepticism shows itself in his analysis of Dr. Gustaf Åkerman's *Realkapital und Kapitalzins* (*Ekonomisk Tidskrift*, 1923) reproduced in English in *Lectures on Political Economy*, edited by Professor Lionel Robbins, Vol. I, and in the article published posthumously in *Die Wirtschaftstheorie der Gegenwart*, Vol. III, 1928. One would have liked to see one of these pieces included.

Wicksell's views on trade-cycle theory are to be found in "Krisernas Gata" (1907), a translation of which appeared in *International Economic Papers*, No. 3, under the title, "The Enigma of Business Cycles"; many readers will be disappointed that this interesting essay was left out. Finally, recalling that Wicksell once said that "an economist who is not completely saturated in Malthus is like a sailing ship without rudder, compass or ballast," we must regret that room was not found in this volume for one of his contributions to population theory.

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The Economic Development of Detergents. By W. J. CORLETT. (London: Duckworth, 1958. Pp. 208. 25s.)

THIS book is concerned with the "circumstances which predispose an industry towards accepting innovations eagerly or with reserve." In successive chapters Mr. Corlett considers the nature and uses of detergents, the structure of the detergent industries in Britain, the change in processes in the soap industry, the developments leading to the rapid growth of synthetic detergents and the problems of marketing. He gives a useful summary of the growth of the Lever combine and of its failure to get full control of the soap industry, and shows that the high concentration of the industry has not been greatly changed by the rise of synthetic detergents: some of the small soap producers have dropped out, but new firms have entered to produce synthetics, and the chemical industry supplies the synthetic surface-active agents and indeed the major part of the liquid synthetic detergents for industrial use. His study of structure is, however, weakened by the insufficient attention to financial analysis.

The oil-refining and chemical companies played a large part in the development of synthetic detergents, and they produce the surface-active agents. As the methods of producing synthetic detergents are not specially

suitable for the soap industry, why has it retained the main part of the production and marketing, at any rate in the domestic field? Mr. Corlett argues that explanation must "rely largely on the fact that the marketing problems which are involved in the distribution of detergents are different from those with which the other possible producers of the synthetic products have normally been faced" (p. 166). He argues that this advantage does not derive mainly from control of the final point of sale to the consumer, but from the possession of a sales force that is accustomed to dealing with the appropriate retailers and experience in advertising, couponing and other sales-promotion schemes for detergents.

This is a plausible explanation, and it may be that such marketing considerations were decisive for the oil and chemical industries who might have become producers of household detergents; but though in a study of the interactions of structure and innovation it is important to establish this, Mr. Corlett does not appear to have made a serious attempt to do so. It is a weakness of this book that very little is said about methods of investigation. There is reference in the Preface to "those who have thought the inquiry to be on the wrong lines, or who have felt unable to give the information for which I asked," and on p. 55 to "discussions with numerous people connected with the industry." These statements do not tell us much about the amount of information Mr. Corlett was able to collect from firms, though they seem to suggest that some firms at least were not co-operative. That he was unable to gather sufficient information is suggested by the number of conditional or vague conclusions such as: "it is possible that the change in relative prices has given some incentive to the new methods of production" (p. 103); "in some cases it is believed the reduction in the output of soap has led to difficulties in operating continuous soap-making equipment" (p. 186); "many people think that the rate of expenditure on sales promotion" cannot long continue (p. 201); "if the industry consisted of small firms the tendency might be to introduce cheaper materials and thus, possibly, to worsen the performance of the product" (p. 203).

The discussion of innovations and their origins, in both soap and synthetics, is also very general—it reads as if it has been compiled rather too much from text-books and journals and rather too little from case studies of the capacities and interests of the firms involved. Some of these firms may be outside the industry. Mr. Corlett does refer to the innovating role of the engineering firms supplying equipment to the Italian soap industry; but it does not get us far to suggest (p. 189) that the same might have happened here if oligopoly had not developed in the soap industry. (In the cement industry in Britain the main process innovations did come from the engineering firms.) In dealing with the reaction of the soap industry to synthetic detergents Mr. Corlett suggested that the post-war willingness to try out the new products on a larger scale was due to the shortage of fats and oils, "to the hope of getting a large share of their competitors' market for detergents

by getting in first . . . and the fear that, if they do not get in first, their competitors will" (p. 193). Was it not possible to get more evidence on this—on how and when it affected research plans and investment plans of the firms concerned?

Though the book is both interesting and useful in formulating hypotheses, I found it curiously inconclusive about the posed problems.

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✓ *Labor Problems in the Industrialization of India.* By C. A. MYERS. (Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. Pp. xvii + 297. 52s.)

INDUSTRIALISATION at a quick pace constitutes the keynote of the Indian programme for rapid economic development. Industrial labour, which occupies a small though vital and strategic place in Indian economy, will greatly increase both in number and in importance as a result of this programme. The present labour problems and unrest are likely to be magnified, unless they are properly diagnosed, and adequate remedies prescribed and adopted. A study of these problems by a recognised expert in industrial relations of Dr. Myers' calibre will be widely welcome at this juncture.

The major requirement of industrial progress is a conscientious, contented, disciplined and efficient labour force, but in view of India's low national income and the imperative need for capital formation, the general economic inducement that can be afforded in this direction is small. On account of dis-unity in labour ranks and leadership of different political parties at the helm of different trade unions, the inducements are likely to be frittered away rather than concentrated in directions where they yield most. Further, since the wastes of strikes and lock-outs cannot be afforded, full development of free collective bargaining with these inevitable concomitants has to be ruled out. State intervention on a wide front to ensure labour rights to the extent thought justified in the wider interests of the national economy and maintenance of industrial peace through conciliation, arbitration and a variety of other methods seem to afford the only way out. Dr. Myers brings out this paradox likely to be met with in all developing economies in all its contradictions. He has no fool-proof solution to offer. Though his sympathies are with the remedy already attempted, he is keenly conscious of the sacrifice of healthy trade unionism that is being made at the altar of economic progress.

Within the limits laid down by this fundamental paradox, however, much can be done by all the three parties—Labour, Management and Government—and much certainly remains to be done. The Indian labour force is still by and large not broken to factory discipline and work. Though the turnover of labour is low, absenteeism is high. Cases of indiscipline are not infrequent. Labour is prone to go on strikes to protest, but often no

advance notice is given, nor are demands clearly formulated. Trade unions get a large influx of new membership in such a period, but the enthusiasm does not last long after the settlement or the withdrawal of the strike. Membership falls as quickly, or still worse fees fall in heavy arrears. Most of the trade unions, therefore, lack funds for any regular continuous work, and have no welfare activities. Their hold over labour tends to be precarious, and there is little scope for development of responsible leadership.

In the final outcome much will depend on labour attitudes and co-operation, but in the first stage, the initiative must largely come from the other two parties. Till very recently, management has done little to discharge adequately its responsibility to labour. Even recruitment was left to jobbers who were untrained for the job and abused the powers given to them by the management. There was no systematic provision for labour training, and at both supervisory and middle-supervisory levels there was a dearth of trained personnel. The need of a proper personnel policy and of personnel officers who could deal with labour problems tactfully was not realised. Even when labour relations were better managed by some employers, they were more likely to be on the *ma bap* or paternal basis. The employers had their own limitations, as they had not themselves received the benefit of a training in the principles of management. Top management was confined to the family circle. Dr. Myers draws this bold picture clearly and forcefully. He is careful to point out that things were never entirely black and that they are changing. He describes the exceptions in great detail. But their impact is yet to be felt, and it will take time before the general situation changes.

Because labour organisations are weak and labour restive, and because India cannot afford at this juncture a prolonged period of industrial unrest, the Government has to shoulder a great deal of responsibility. It has to lay down various detailed requirements that management must fulfil—provisions of welfare services, appointment of labour officers, formation of grievance committees, and so on. Dr. Myers is specially appreciative of the personnel aspects, for a worker is a human being, and much of the ideas and programmes evolved on this basis can be transmitted to him with ease. The Government has also to lay down rules governing rationalisation, prescribing conciliation and arbitration machinery, and compulsory resort to them in some cases. In a few cases, it has gone to the length of laying down detailed rules regarding a “representative” union. It has also been anxious to see provision provided for management and supervisory training. Most of the advances in this direction have been made since the inauguration of the Provincial Autonomy in 1937. It is not possible to move on so many fronts in so short a time as two decades without occasional mistakes or displeasing quite a few people. A critic may certainly point out that not all Government steps are entirely self-consistent, and that there is scope for

changes in certain policy matters. Dr. Myers gives the impression that he holds the view that by and large the Government policy is well conceived, and even enlightened, though it is widely different from that which prevails either in the United Kingdom or in the United States.

There is one omission in the book that is striking: detailed discussion of wage policy. While it is pointed out that the discontentment of labour is due to wages lagging behind cost of living, the outlines of a satisfactory wage policy are not drawn. In view of the complicated network of minimum-wage legislation and the varied attempts of the Industrial Tribunals and Labour Courts to lay down wages, this was surely essential. With this possible exception, the treatment given in the book is fair and eminently satisfactory.

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Royal Commission on Canada's Economic Prospects: Final Report. (Ottawa: The Queen's Printer, 1957. Pp. 509 and map. \$3.00.)

Canada's Economic Development 1867-1953. By O. J. FIRESTONE. (Income and Wealth Series VII.) (London: Bowes & Bowes, 1958. Pp. xxvi + 384. 45s.)

THE Gordon Commission was formed to “inquire into and report upon . . . the probable economic development of Canada and the problems to which such development appears to give rise.” Its method of study was to travel widely holding “on the spot” hearings, and to delegate detailed investigation. Within a year and a half from its initiation in June 1955 it had received 330 submissions, heard over 750 witnesses, digested thirty-four studies prepared at its request (published separately) and completed its Preliminary Report; the Final Report, which followed almost a year later, was in all essentials a repetition but expanded with elegant discussion. The apparent swiftness of the Commission's comprehension in a matter which does not appear to be urgent cannot be entirely dissociated from the pressure of political events (in particular from the general election of June 1957, which was to end twenty years of Liberal rule), and it is clear that some parts of the Report would have gained from more mature reflection.

Assuming there will be no global war, no major slump and no major changes in the economic policies of the Canadian or other governments (which implies a negligible impact on policy of its own suggestions), the Commission projected movements of the main economic variables to the year 1980 (with 1955 as base), discussed its projections by sector, gave detailed attention to certain industries (agriculture, fisheries, forestry, transportation, housing), to certain areas (the Atlantic Provinces and the North) and to other matters (such as the role of the Government). Unfortunately a reviewer cannot discuss the quality of these projections, since most of the figures are quoted directly as conclusions from the special studies. (It was,

however, clear that the Gross Domestic Product was projected on the usual basis of assumed changes in the size and productivity of the labour force, and it seemed that employment in secondary industry was based on projections of productivity and output—the latter being based on the G.N.P. estimate.)

It is the discussion of policy which is most interesting, yet, disappointingly, this reflects not so much rational economic argument as the views evidently impressed upon the Commission by interested parties. A major topic is Canadian–United States relations, in particular the increasing control by Americans of concerns in Canada (already approaching one-third of manufacturing output). The Commission believed it would be “of benefit to all concerned” if such concerns employed more Canadians in key positions, purchased their supplies and equipment in Canada, voluntarily disclosed the results of their Canadian operations, included “a number of independent Canadians” on the boards of directors and sold an appreciable (20–25%) interest in their equity stock to Canadians (and a further objective was to raise the gearing ratio). The proposed incentive was discriminatory taxation. Of course there are dangers in foreign control, but one may regret that Canada, which has benefited so much from a more liberal view, should now—largely for emotional reasons—be feeling towards a change, the costs of which were not discussed. The Commission also thought that competition between American subsidiaries is partly responsible for more diversified and smaller, less-efficient plants than would exist with fewer competitors. They supported a reduction in the number of firms “which a number of manufacturers . . . would welcome,” and recommended (p. 249) that the Restrictive Practices Commission should give considerable weight to achieving the maximum possible economies of scale, and (p. 442) that tariffs should also be adjusted towards this end. They did not, however, suggest how to regulate monopolies to ensure that consumers receive any benefits which result (except to mention the power of import competition—which conflicts with their later recommendation on tariffs); the enormous significance of these proposals evidently escaped them.

The Commission is least convincing over the tariff question. The conclusion that it would be sensible to hold the tariff line on the average at about its present level does not strike the reader as being the result of logical reasoning from the economics of the case (which are well stated earlier in the report). The arguments against lower tariffs seem particularly weak (notably on p. 439, where the discussion is in terms of their complete and sudden removal); the greatest weight seems to be placed on the unlikelihood of reciprocity from the United States if tariffs were reduced. Although the recommendation is for no change in the average level, there is more than a hint (for example in the desire to strengthen anti-dumping measures) that the reductions of recent years have really gone too far.

In spite of these and some other debatable features (for example, the

analysis of the control and in particular the causes of inflation), the Report contains much to interest and to stimulate the reader, for its story of prosperity and enterprise is an exciting one. The main contribution of the Commission probably lies in the special studies undertaken on its behalf, and in the stimulus it may give to further thought and research.

Canadian official statistics date from 1926. Dr. Firestone's contribution, in the final volume of the *Income and Wealth Series*, is to make new estimates for the period 1867–1925 on the same basis as the official figures and to link his series to them. The estimates cover the usual components of national income (including capital consumption) at current and at constant dollars, together with much other material, such as population and families, international trade and the productivity of labour and capital.

In 1867, the year in which the Canadian nation was formed by Confederation, the economy was rural, small-scale and based principally on local markets; with these new estimates it is now possible to trace more clearly than before the emergence of a highly industrialised urban economy marketing nationally and internationally. Rather more than one-half of the book is given to an analysis of the long-term changes 1867–1953 and the intermediate-term changes (in four sub-periods) as revealed by the data. The general thesis (which oddly enough is not stated until p. 173 and is not referred to again) is that “when a nation undergoes an industrial revolution the first generation may make more startling progress in terms of the level of real incomes and standard of living than subsequent generations because real incomes and the standard of living of a predominately agricultural society are usually quite low.” There are places in this analysis where the reader may feel sceptical: Can it be true that the real value of fixed capital “per person working” in manufacturing was almost identical in 1910 and 1950 (p. 230)? Can one really infer from the close correspondence between aggregate net and gross values of production over long periods that the same would be true in individual industries (p. 220)? And at many points, particularly where the capital–labour and capital–output ratios are mentioned, the reader is left feeling that either more discussion or less would have improved the book.

The sources and methods of obtaining the new estimates are described in considerable detail, and they reveal a most thorough grasp of the available material. No doubt some of the procedures for deriving the new data could be improved, but without knowing the reliability of alternatives it is not easy to say which; some areas where further work might be rewarding have already been indicated. Nevertheless, the new estimates are a fine achievement and provide much additional material to specialists in economic development and growth.

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Business and Politics under James I: Lionel Cranfield as Merchant and Minister.

By R. H. TAWNEY. (London: Cambridge University Press, 1958. Pp. 324. 40s.)

THE reign of James I was not a time for greatness and Lionel Cranfield was not a great man. But his career—as merchant, speculator, financier and Royal Minister—is full of interest, illuminating as it does the twin spheres of business and politics within which he worked. Professor Tawney's study is not a full-length biography but an account that is in part biography and in part an account of the context of institutions, thought and custom within which Cranfield worked. The book that has thus emerged from more than a quarter of a century of research will not compete in popular appeal with some of Professor Tawney's earlier books. Even the serious student of the period will not always find it easy going. The construction is not always clear, and the rich baroque of the author's style is not self-evidently the best medium in which to expound the prosaic nature of the merchant's craft. For all that, Professor Tawney has constructed, out of the rich but intractable material of the Cranfield papers, a fresh chapter of seventeenth-century history which no student will henceforth be able to neglect and which will probably outlive some of his more popular works. He has done for Cranfield what has not been done for any comparable figure of the preceding or later period—Gresham or Downing, for example; more than that, he has done it not only with learning and felicity but also with remarkably sympathetic insight into the man and his times.

Part I sets out Cranfield's business as merchant and cloth exporter against the background of European trade and contemporary trade practices. Chapter II describes with admirable lucidity the rising European economy. But whether, as here, he is wielding the large brush for the wide canvas or, as in Chapter IV, the fine nib to etch in a footnote of financial detail, Tawney is always master of his media. He is alert to the changes of emphasis wrought by recent research and opinion in such matters as state intervention in economic affairs. The most recent monographs on taxation and finance have been absorbed and made part of the whole.

Little of Cranfield the man emerges from these early chapters; but in Part II he comes to full stature. Professor Tawney is not blind to his subject's faults—the greed, the truculence, the ostentation—that a lesser historian might have been inclined to labour. But his generosity of mind—for all his anti-capitalist bias—is plainly aroused by the courage and the integrity of his subject. James I put his finger on the reasons for his unpopularity. "All Treasurers, if they do good service to their masters, must be generally hated." And Cranfield was a good Treasurer—better than his master deserved. He had, said Eliot, his opponent, "merited well of the King and had done him that service that few had done." He was brave and dutiful, but he was an executant, not a politician. One can

think of more recent instances where Ministers have been unsuccessful, not because they were inefficient, but because they failed to appreciate the power of political interests and forces, and the need to placate the feelings of politicians. Professor Tawney does splendid justice to the tragic element in the story.

It is customary nowadays to peer into the murky years that preceded the Civil War searching for signs and portents. There are few here. If the King was hard up it was clearly his own fault, not that of his subjects, though there were plenty ready to take advantage of his folly. Did the name of King mean (as Professor Tawney suggests) less because of James's squalid habits? Maybe, though to read Clarendon is to be aware of the dangers of exaggerating even this. No, Cranfield's importance is mainly as a symbol of a lost opportunity and as the first of the line of royal servants—Laud, Strafford, Coventry, Clarendon—whose fate rebukes the ingratitude of their masters.

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NOTES AND MEMORANDA

SOME MARSHALLIAN CONCEPTS¹

PROFESSOR MAXWELL's article in the *ECONOMIC JOURNAL*, December 1958, taken in conjunction with the companion piece by Professor Hague, indicates a welcome revival of interest in Marshallian analysis. If I have contributed at all to this by the lecture² to which they both refer, I shall not mind having made some blunders in the process. I do not think I have made quite as many as Professor Maxwell thinks; but in any case I should like to offer a few disjointed observations on his interesting article, covering these and other points. The references, except where otherwise stated, are to the pages of the December *JOURNAL*.

(1) I do not think it is quite enough to say (691) that the Representative Firm is representative in respect of unit costs; one must specify some of the attributes representativeness in respect of which *makes* it representative in respect of unit costs. These are, I think, at least four—size, age, competence of management and luck.

(2) I fully agree that even for the hypothetical stationary (but not stagnant) state with which Professor Maxwell, following Marshall, starts (692), the conception of the Representative Firm is illuminating. I think Professor Maxwell's label of "short-run normal equilibrium" for this state is unfortunate, since this phrase is generally used with quite a different meaning.

(3) Professor Maxwell has persuaded me (693, n. 2) that *OM* in Marshall's diagram on *Principles*, p. 344, should be read as standing for the output of the whole industry. He has *not* persuaded me that Marshall had any clear conception of a U-shaped long-period cost curve for the firm; it seems to me clear that on *Principles*, p. 810, as in the passage on *Principles*, p. 497, to which I referred in my lecture, the equality of marginal and average costs has nothing to do with that intersection of U-shaped curves to which we have all grown accustomed, but is concerned with an entirely different matter, namely the alternative conceptions of costs as respectively excluding, or including, rents.

(4) I agree, and have indeed emphasised, that Marshall never commits himself to the completely horizontal individual-firm demand curve of the later text-books, and that he specially mentions the possibility of steeply

inclined ones (694). But I think he would have had to agree that in "large and open markets" in non-depressed times (see Professor Hague, 676) such curves, if drawn at all, would have to approximate pretty closely to the horizontal. I do not agree (694, n. 1) that such a horizontal curve "does not show consumer response." Any demand curve can be read alternatively as showing the expected effect on price of a small change in output or the expected effect on consumption of a small change in price, and these individual-firm horizontal curves are no exception. The firm thinks that if it raised its price at all it would lose all its customers, while if it lowered its price at all it would find that it could sell more than it could conceivably produce; and, in many cases at least, if the expectation were put to the proof, the event would show that it was perfectly correct. Consequently, I do not think I was wrong in criticising Frisch for drawing, in the general case in which he was not primarily concerned with conditions of what he called "restrictive strategy," a demand curve for an individual firm, however "representative," as steeply inclined as the demand curve for the whole industry. (Obviously, I did not criticise him, as Professor Maxwell says I did, for drawing a sloping demand curve for the product of the whole industry.)

(5) On 696 Professor Maxwell, and Marshall, take wing from the stationary state—it is Marshall's fault, not his own, that Professor Maxwell describes their flight as "statical"! With all that follows about external and "external-internal" economies I am in full agreement.¹ But I think Professor Maxwell might have considered whether, at any rate in this more dynamic and complicated world, the "life cycle" element in Marshall's thought, the importance of which he has shown earlier (692 bottom) that he appreciates, does not compel the conclusion² that even the firm which at any moment is representative may be "out of equilibrium" and "with unexhausted internal economies," since its vigour or its luck will have passed away before there has been time for it to reap the economies which are theoretically within its grasp, but which will in fact be reaped by someone else.³

(6) From n. 3 on 697 I gather that Professor Maxwell does not disagree with me, that, in this expanding world at least, the supply curve of the representative firm cannot be presented as a mere replica, with change of scale, of that of the whole industry. But that is how Frisch *does* present it; and, as far as I can see, he can only do so by assuming that the link between the two, namely the number of firms, remains constant as total output grows. Professor Maxwell says I am wrong about this, but he does not show me why!

(7) On the top of 698, Professor Maxwell has been wronged by the

¹ I would like, without committing them to its terms, to express my thanks to Professor D. C. Hague and Mr. R. C. O. Matthews for a number of comments and suggestions helping me to improve this note.

² *Economic Commentaries*, No. 1.

¹ Cf. my *Lectures on Economic Principles*, Vol. I, p. 121.

² Which he criticises Mr. Wolfe for reaching (692, n. 1).

³ See my *Commentaries*, p. 23, and *Lectures*, Vol. I, pp. 121-2.

printer—I take it that the comma after “increasing” (line 7) should go, thus making it plain that after “external” (line 6) the word “economies” should be mentally supplied. But surely *increasing* costs play their part already in producing the equilibrium of the stationary state? And surely, in that state, external economies are not non-existent, but only constant? See the first sentence of the quotation from Marshall on 692.

(8) In the next paragraph, Professor Maxwell goes a bit beyond Marshall, who contrasts “a single rifle or yard of cloth” with “a whole process of production,” not with the output of a whole firm.

(9) I find the final paragraph faintly puzzling. I can think of many not wholly complimentary epithets for Marshall’s discussion of decreasing cost, but “elementary” would not be among them. And that discussion is largely devoted to showing that decreasing cost, even if “continued,” does *not* lead to monopoly as often as one might think. But it is surely an exaggeration to say that even in the *Principles* Marshall “showed little concern” about monopoly, though he deliberately reserved his main study of it for *Industry and Trade* (*Principles*, 477).

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A NOTE ON THE TREATMENT OF IMPORT DUTIES IN THE MEASUREMENT OF GROSS DOMESTIC PRODUCT

Mr. J. L. NICHOLSON, in an article in the *ECONOMIC JOURNAL*,¹ and in a later note,² sustains the thesis that import duties should not be “included” in the total monetary figure representing the value of gross domestic product at market prices, although apparently all other indirect taxes (taxes on expenditure), including those levied and collected at the retail level, should so be included. The later note is a reply to a defence by Mr. H. Burton of the traditional procedure of including all indirect taxes (taxes on expenditure).³ In my opinion, Mr. Nicholson does not deal adequately with a basic difficulty, namely, that in trying to measure the value of production *at market prices*, if this is to be done by economic sectors there is no satisfactory alternative to including indirect taxes in the value of the output of that

¹ J. L. Nicholson, “National Income at Factor Cost or Market Prices?”, *ECONOMIC JOURNAL*, Vol. LXV, June 1955, p. 216.

² J. L. Nicholson, “Import Duties and the Gross Domestic Product at Market Prices,” *ECONOMIC JOURNAL*, Vol. LXVIII, June 1958, p. 393.

³ H. Burton, “Expenditure Taxes, Imports and Gross Domestic Product at Market Prices,” *ECONOMIC JOURNAL*, Vol. LXVII, December 1957, p. 644. Mr. Burton’s later note (*ECONOMIC JOURNAL*, Vol. LXVIII, September 1958, p. 585) was received after this note was drafted, but deals with different aspects of the problem.

sector which actually pays the taxes to the Government. Any attempt to use a more refined basis gets into the problem of tax incidence, clearly a matter for analytic investigation and not to be solved by statistical presentation.

The need for having expenditure totals at market prices (including all taxes) for welfare and market analysis is widely recognised. For income and productivity analysis, it is conceded by most authorities that a factor cost (or factor rewards) measure is preferable. However, in measuring production from the expenditure side, for practical purposes the market-price concept can only be applied by product (*e.g.*, automobiles, food, clothing), while the factor-cost method of measuring income (or production) cannot be applied by product, but only by industry or economic sector.

Some countries do apply the market-price measure to output by industrial sector in their official statistics. This they do by subtracting from the sales of each sector the purchases of that sector from all other sectors, both valued at market prices. This is tantamount to including the value of any indirect taxes in the gross output of the sector that includes those taxes in its gross sales figures (or is deemed by the statistician so to include them¹) and actually pays them to the Government.

If this method were applied in the United States, for example, indirect taxes paid by the cigarette industry would be included as part of the output (gross domestic product) of manufacturing (the federal excise tax, paid by the manufacturer) and of distribution (state taxes, ordinarily paid by the wholesale—or retail—distributor). Actually, the incidence of any tax on tobacco falls mainly on the final consumer, in the sense that if the tax were abolished, there would be a substantial reduction in the price of cigarettes. In part, however, the incidence falls on the farmer who grows the tobacco, the manufacturer and, to some extent, on the wholesaler and retailer who distribute either the intermediate or final product. In part—and this is important—the incidence may fall on the foreign producer of tobacco imported by the domestic manufacturer, since if all tobacco excises were abolished, the price of unmanufactured tobacco would undoubtedly rise all over the world.

Once the concept of gross product at market prices has been accepted, there seems to be no logical reason for excluding import duties from the total any more than for excluding any other indirect tax. Mr. Nicholson argues the contrary view, asking: “Since imported tobacco is not part of the net output of the cigarette-manufacturing industry, how can a customs tax on tobacco form part of the gross domestic product of the United Kingdom?” The question is not whether an indirect tax is in any real

¹ In the United States retail sales taxes are generally, by law, collected from the customer and remitted to the taxing authority by the retailer. The law imposes the tax on the customer. Presumably in such cases the retailer does not include these taxes in his own accounts; however, the economic effect is obviously the same as if a 2% tax on sales were imposed on the retailer and the retailer shifted the tax to the consumer.

or final sense a part of the product of any domestic industry (such a tax if fully shifted to the ultimate consumer would, in one sense, not be a part of the product of any industry, even if all component parts were domestically produced), but whether in some logical or useful meaning of the term it is part of the gross product of the economy. The most useful meaning of the term gross product, it seems to me, is one that will permit adding up component parts and getting a total that equals the sum of all final expenditures on domestically produced goods and services whether purchased for use within the country or exported. This can be achieved by summing consumption, investment and exports (at market prices, which include *all* indirect taxes paid to domestic governmental entities), and deducting the value of imports (excluding import duties paid to the domestic government), *i.e.*, purchases *from abroad*.

Mr. Nicholson dismisses the alternative (and to me completely logical) possibility, that customs duties can be regarded as part of the importers' contribution to gross domestic product. He says:¹

"It has been suggested that customs duties, can, alternatively, be regarded as part of the price of importing the goods in question, and hence as part of the importer's contribution to the gross domestic product. But the customs duty is paid by, not to, the importer, on the goods which he purchases. There can be no case for treating part of the *input* of an industry as part of its net *output*. There is, in short, no reason to treat customs duties differently from other indirect taxes."

But Mr. Nicholson *does* want to treat them differently, by excluding them from gross domestic product! While there can be no case for treating part of the *input* of an industry as part of its net output, it seems to me that *inputs* can be most usefully regarded as purchases from other businesses or from the rest of the world, not including any taxes that may be levied by the domestic government on the industry making the purchases.

One can agree that if an indirect tax has been paid by the seller, and is therefore part of the purchase price, it is not ordinarily included in the purchasing sector's gross *or* net product, but is deducted along with other intermediate purchases. However, if internal excise taxes paid by the cigarette manufacturer are not to be regarded as "part of the price of purchasing the goods in question," *i.e.*, the tobacco, why should customs duties paid by importers be considered as part of the cost of their purchases from foreigners? Suppose there is an excise tax on raw copper, and part of the transforming industry's supply is imported and part is purchased from domestic mines. Should not the whole excise tax be included in gross domestic product? And as part of the product of the transforming industry? And if the tax is a customs duty, and levied on imports only, and even only

¹ ECONOMIC JOURNAL, Vol. LXV, June 1955, p. 223.

on imports of completely finished goods, is it any less a tax on the industry (including the distribution industry, which Mr. Nicholson says "is, of course, assumed to be part of the production process") which uses (distributes) the imported goods?

The illogical nature of Mr. Nicholson's treatment may be illustrated by a reversal of one of the arguments he uses to support it. He suggests¹ that, if import duties are included in gross domestic product, the latter will rise (fall) if "we import more (less) of the goods that attract customs duties." (By the way, this would be true only under special assumptions; actually if there were no change in incomes, either the higher tariffs would have to be absorbed—presumably by reducing profits—along the way, with no resulting rise in gross product, or the total volume of real expenditures reduced.) I presume he would also object to the fact that raising tariff rates on existing imports would also have the effect of increasing domestic product. But suppose that a customs duty were raised or imposed, and that the full effect fell on the foreign exporter, who made an exactly equivalent reduction in his price. The effect of this reduction in price would be to enable the importing economy to obtain a larger amount of foreign goods in exchange for its exports; surely this ability ought to be reflected in an enhanced value of gross domestic product. This statistical result will be achieved only by *not* including customs duties in the value of imports in the equation "Gross domestic product equals consumption plus investment plus exports minus imports" with all terms expressed at market prices paid by the purchasers *to the sellers*, *i.e.*, by *including* customs duties among the items that constitute the difference between domestic (or national) income at factor cost and domestic (or national) product at market prices.

And even if the full effect of the tariff increase were passed along to the final consumer, and the foreign exporter received the same amount of money as before, is it not clear that somebody's "gross domestic product at market prices" has risen? And is it not more logical, or at least more useful, to conceive of the product as having risen in the importing country, whose consumers are willing to pay a higher market price than before for their consumption, than in the producing country, which is not receiving a higher price for that part of its gross product sold to foreigners?

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¹ ECONOMIC JOURNAL, Vol. LXV, June 1955, p. 223.

IMPORT DUTIES AND THE GROSS DOMESTIC PRODUCT AT MARKET PRICES—A REPLY

MR. SAMMONS' criticisms of the method of dealing with import duties in the National Income Blue Books are on very similar lines to the criticisms previously advanced by Mr. Burton,¹ to which a reply has already been given.² He, too, relies on assertions unsupported by reasons. He does, however, introduce a new aspect when he writes "there is no satisfactory alternative to including indirect taxes in the value of the output of that sector which actually pays the taxes to the Government. Any attempt to use a more refined basis gets into the problem of tax incidence, clearly a matter for analytic investigation and not to be solved by statistical presentation." He does not explain why this aspect should affect one method and not the other, or how one method can be considered "more refined" than the other. The incidence of the tax burden in fact plays no part in my argument. Curiously enough, it is Mr. Sammons who, by concentrating on this aspect, lands himself in difficulties. He explains, as is well known, that the incidence of an indirect tax can be hypothetically apportioned between the producer and consumer (the analysis depending on the shapes of the supply and demand curves) and then says, "The question is not whether an indirect tax is in any real or final sense a part of the product of any domestic industry . . . , but whether in some logical or useful meaning of the term it is part of the gross product of the economy." An indirect tax which forms part of the gross product of the economy must, however, also form part of the product of a domestic industry (in the same, not in any more real or final sense which no one is invoking). Mr. Sammons indeed admits this when he writes, "The most useful meaning of the term gross product, it seems to me, is one that will permit adding up component parts and getting a total that equals the sum of all final expenditures on domestically produced goods and services whether purchased for use within the country or exported." It is just because customs duties are not part of final expenditure on domestically produced goods and services that they are excluded from the domestic product. His mere assertion, in the next sentence, that customs duties are part of the domestic product does not make them so—and contradicts his previous remark.

Mr. Sammons and Mr. Burton both insist that customs duties should be treated in the same way as other indirect taxes. As Mr. Burton puts it, "Since the incidence is the same, the treatment of them should be the same." In the official estimates, customs duties are included in the value of imports at market prices and are, therefore, treated in the same way as other indirect taxes. The effects on estimates of the domestic product are

different; but the incidence of a duty on wine imported from France is not the same as that of a purchase-tax on motor-cars produced in Coventry. Mr. Sammons' contention that, since the same treatment has different effects, the treatments are therefore different is not to be taken seriously.

When Mr. Sammons urges that a customs duty on imports is a tax on the industry which uses or distributes the imported goods, he is invoking the argument based on tax incidence which he himself says is irrelevant. Of course industries may suffer or benefit from excise duties; but no one would for this reason wish to alter the value of their output. Neither Mr. Sammons nor Mr. Burton considers the case of customs duties on goods (*e.g.*, watches, silk, tobacco) brought into the country by the final, private consumer. To what domestic industry can such taxes be attributed?

Mr. Sammons attempts to refute one of my arguments—that, unless customs duties are included in the value of imports, the domestic product would rise (fall) whenever we import more (less) of the goods that attract customs duties—by reversing it. He says, first, that this is not necessarily true because, if there were no corresponding change in incomes, either the higher tariffs would have to be absorbed by reductions in profits or expenditure in real terms would be reduced. He forgets that any increase or reduction in expenditure from the change in the duty would cause the income of the government to rise or fall by the same amount. "Suppose," he goes on to say, "that a customs duty were raised or imposed, and that the full effect fell on the foreign exporter, who made an exactly equivalent reduction in his price" (a most unlikely event). He argues that, because this reduction in price enables more imports to be obtained, it ought to be reflected in an enhanced value of the gross domestic product. He is here saying in effect that any rise or fall in the prices of imports should be deducted from or added to the gross domestic product, valued at current prices. To what strange lengths is he driven! Changes in the prices of imports cannot affect the value of the domestic product at current prices. Nor do they affect the value of the domestic product itself at constant prices. But, if it is desired to measure the income-equivalent of the domestic product, adjustments can, of course, be made specifically to allow for changes in the terms of trade.

Finally, he argues that, since consumers in the importing country are willing to pay the customs duties, the duties should form part of the domestic product of the importing country, rather than of the producing country. What about goods on which the excise duty in the exporting country is as high as the customs duty in the importing country? (Not an unlikely case, whisky being an example.) As consumers in both countries are willing to pay about the same, this criterion does not help. But since his argument could equally be applied to the non-tax component of the value of imports, its logical conclusion is that the domestic product should include all imports and exclude all exports. The term product would then lose

¹ ECONOMIC JOURNAL, December 1957, p. 644.

² *Ibid.*, June 1958, p. 393.

its meaning. Mr. Sammons has, I think, failed to notice that international consistency is achieved by the method suggested in my previous note (ECONOMIC JOURNAL, June 1958, footnote on p. 396).

Meanwhile Mr. Burton¹ has withdrawn one of his earlier criticisms and now admits that the internal inconsistencies resulted from his combining two different systems. But he blames his mistake on *National Income Statistics—Sources and Methods* for not including an explicit statement of the rule followed in the Blue Book. Page 347 of *Sources and Methods* includes the following sentence: "The view taken is that the market prices of imports, as of other goods, must include any customs and excise duties which they attract, since it is the price including duty which the purchaser has to pay, and which therefore affects substitution at the margin between different commodities, whether home-produced or imported." A footnote refers to my article in the ECONOMIC JOURNAL for a fuller explanation. Since this explicit statement and the article referred to were quoted by Mr. Burton, he cannot have overlooked them.

The only other reference to the subject in *Sources and Methods* is the general comment in the opening chapter that estimates of the national product at market prices, though not given in the Blue Book, can be obtained from the material which is provided. As this statement does not specify which taxes should be included, it cannot be construed as laying down how such estimates should be compiled. Estimates of the national product at market prices, which Mr. Burton seems to have some difficulty in calculating, are included in Table 11 of the 1958 Blue Book.

It is the prices including duties which affect economic decisions to import more or less of a given commodity. The fact that import duties are not paid to overseas suppliers does not directly influence these decisions. The exclusion of such duties from the income of others is not a sufficient reason for including them in the output of home producers.

Mr. Burton draws attention to the fact that a country's total domestic expenditure will now tend to exceed its national product and national income without mentioning that, in my previous note, I suggested a line of argument which would produce definitions which were consistent within and between different countries—viz., to treat import duties as part of the gross domestic products at market prices of foreign countries which is appropriated from them, so forming part of the national income but not part of the national product of the importing country. This is a perfectly tenable argument, and national expenditure would not tend to exceed national income thus defined. But to include import duties in the national product with the object of preserving the balance with income and expenditure is to let appearances dictate the method.

Central Statistical Office.

J. L. NICHOLSON

¹ ECONOMIC JOURNAL, September 1958, p. 585.

REJOINDER ON JAMAICAN LAND VALUATION AND LAND TAXATION

MR. D. F. MURRAY makes two groups of points in his "Further Note on Finance and Taxation in Jamaica".¹ He makes assertions about the upshot of the current process of revaluation. Personal inquiries during October 1958 of both officials concerned with general government policy and those concerned with valuation failed to find evidence to warrant those assertions. Re-valuation began in the summer of 1957. The parish of St. Catherine was selected. It took exactly twelve months to complete that parish. Valuers then moved to the parish of St. Ann. Valuations have now reached a peak of 8,000 per month. Providing valuations averaged that peak level month by month, the whole island would be completed in another three years. Meantime no report has been issued and no valuations divulged. The extremely slow progress hitherto—the parish of St. Catherine covers about one-tenth of the total area of Jamaica—is attributable to lack of adequate maps, difficulties in tracing ownership, periods of unusually heavy rain and the employment of a staff which, with the sole exception of the Commissioner for Valuations himself, was without experience in this kind of work. A possible offsetting advantage of the delay is that market data are becoming available all the time, and revisions of assessments can consequently be made before valuation rolls are issued. This should help to minimise the extent to which valuations are appealed against as inappropriate when they are issued. Even if the valuation roll for St. Catherine were available for inspection, Mr. Murray's assertions would still not be warranted. It would still not be possible to argue "the general appropriateness of unimproved value as the basis of taxation and rating" because the nature of the land tax is still unknown. It has still to be decided. Nor would it be possible to contend that "sales analyses made during the progress of the current revaluation of Jamaica have revealed that the prices paid for sites for hotels and luxury homes ensure that their unimproved values will constitute an equitable basis for taxation and rating." For, with rare exceptions, such luxury buildings are to be found along parts of the North Coast. St. Catherine was selected because it did not stretch to the North Coast and was mainly rural and was consequently relatively uncomplicated compared with some other parishes. Valuers have not yet reached the hotel areas. All that might rightly be asserted at present is that hotel land at perhaps £4,000 per acre and land for luxury homes at anything up to £16,000 per acre will undoubtedly head the list for each parish and, providing the proportions are right between these and other valuations, there is the possibility of equity as between landowners. Whether the owners of luxury buildings will pay in addition to the land tax a rate for local services

¹ ECONOMIC JOURNAL, December 1958, p. 835.

—as proposed by Dr. Murray—is still not decided. Whether they do or not clearly greatly affects the claim of equity made by Mr. Murray.

This brings us to the second group of points. Mr. Murray repeats, largely verbatim, both the critique of the proposals of Professor and Mrs. Hicks for land taxation and also the case for unimproved land value taxation that is to be found in Dr. J. F. N. Murray's *Report to the Government of Jamaica on Valuation, Land Taxation and Rating*. There has been no question of the need for a revaluation on some basis, for present valuations may be related back to the re-valuation of 1937 when apparently the 1929 values were simply transferred to the 1937 list unless there had in the meantime been re-appraisals resulting from change of ownership. The current re-valuation has apparently already shown the considerable extent to which re-assessments have been avoided in the past by the simple procedure of omitting to register property transfers. At the time that Professor and Mrs. Hicks visited Jamaica the alternatives that were apparently open if there was to be any reform in valuation procedure were: (a) a continuance of the full capital value system; (b) the site value rating favoured by Mr. Norman Manley and his friends, who were then in opposition and later formed the Government; and (c) any alternative which would as far as possible get the best of both bases. Professor and Mrs. Hicks recommended that the basis of valuation should be the full value, as determined by comparison with actual selling values, *i.e.*, it should be an improved-value basis; but in order to meet the case which had been advanced by supporters of Unimproved Value Rating, a temporary deduction from the full value should be allowed for certain improvements, based on the value of the improvements. The Hicks plan was put forward as a means whereby some of the economic advantages of de-rating buildings could be achieved without upsetting the equitable necessity for a contribution by the owners of large buildings to local services. Dr. Murray recommended—and Mr. Manley's Government accepted—that there should be a general land tax throughout the island, at a progressive rate, based upon unimproved value but to ensure that properties where the value of improvements greatly outweighed the land value were not virtually de-rated there should also be a special rate, either upon the basis of capital value or upon the excess value of improvements. The Murray plan is thus also a compromise which, if consistently applied, would achieve very similar economic ends to those that Professor and Mrs. Hicks had in mind. It was not proposed by them because it seemed to lay an unnecessary burden on the valuers who were not used to anything so complicated. The slow progress so far seems to have justified that fear, particularly when it is realised that St. Catherine was selected to begin the island-wide re-valuation because it was known to be a relatively uncomplicated parish. When valuers enter the corporate area of Kingston and St. Andrew or the North Coast resorts they will meet the full complexities of unimproved land valuation. In rural areas—the parish of St.

Catharine is mainly rural—"improvements" are necessarily less important than they are in urban areas. Land can be classified according to soil type and location without much difficulty, while in country parts local "services" are either nil or negligible. But in the few urban areas the position is quite different: the ratio of so-called "improvements" to unimproved value is high while local services are expensive to operate.

The question arises whether it was really necessary to embark on unimproved land valuation to obtain the advantages that are claimed. Dr. Murray agrees in his *Report* (p. 15, para. 107) that "capital values would be required for purposes of estate duty and in connection with acquisitions" as well as for the extra "services" rate that he proposes. This calculation would suffice for the Hicks plan. Dr. Murray says in the same paragraph that "the determination of the two sets of values is almost invariably a normal part of valuation procedure, and so there would be little additional work in ascertaining capital as well as unimproved values." But, on his own showing, capital values are required anyway, and unimproved values must therefore be the addition. Nor is it a trifling addition; it is an extremely tricky one to come by. The basis of unimproved land valuation is the value of vacant sites. In Kingston these are few and far between. So valuation will have to depend on deducing site values from remote rather than close evidence. In order, for example, to value the land on which the Myrtle Bank Hotel stands it would be necessary to think of the value of the site to a shipping company or an importer as it has a sea-frontage. How accurately can this be done? Furthermore, if a shipping company or importer bought the hotel in order to make different use of the site, they would necessarily be penalised. For Dr. Murray puts forward a rule for calculating unimproved value which does not take into account the possibility that a site with so-called "improvements" might be purchased in order to build something different on the site. Dr. Murray says in his *Report* (p. 28) that "the unimproved value shall in no case be less than the sum that would be obtained by deducting the value of improvements from the improved value." But this rule is valid only where the so-called "improvements" are intended to be used. If, as is not infrequently the case in the business quarter of Kingston, a site is bought in order to erect a new building the site value is in excess of the total value including "improvements" by the cost of clearing the site.¹ The complexity of the problem is clear. The need to tackle it is far from clear.

Three main arguments have been put forward by the Government of Jamaica for Dr. Murray's plan. In the first place it is argued that unimproved land valuation will provide an incentive to peasant agriculturalists to improve their holdings. Certainly improvements in peasant agriculture

¹ For this reason it would not be possible to use the method employed by Dr. H. W. Singer for estimating urban land rents. See "An Index of Urban Land Rents and House Rents in England and Wales, 1845-1913," *Econometrica*, Vol. 9, 1941, pp. 221-30.

in Jamaica are sorely needed and it is possible that unimproved land valuation might have some influence on some peasants even though it does not touch the roots of the problem. But on the Hicks plan it was possible to exempt small-holders from land tax. The complications of unimproved land valuation did not have to be faced in order to encourage the peasant. Secondly, unimproved land valuation is claimed to be the way to stop speculation in land. Again, unimproved land valuation seems unnecessarily complicated. A simpler alternative would have been a capital-value tax which would tax land on the basis of the selling value of strictly comparable plots and so take "community-created" appreciation into account. But it is the third argument that is crucial to the case for unimproved land valuation: its effect on the inducement to building development. Unimproved land valuation provides a general and permanent incentive to such development, while Professor and Mrs. Hicks suggested a selective and temporary incentive. The question arises whether in a poor country with meagre capital resources and low taxable capacity there should not be some order of priority when tax concessions are involved. This general point is borne out by experience in parts of East Africa, where unimproved land valuation is also popular at present. Thus in Dar-es-Salaam the derating of improvements gave a handsome present to luxury flats, hotels and banks and stimulated the construction of a whole lot more, so seriously hindering the Government's efforts to build very badly needed working-class houses.

These disadvantages can still be largely avoided. For the effect of the Murray plan will depend largely on the rate of the island land tax. Derating necessarily benefits the richer landowners with a high ratio of improvements against the poorer. Fixing a high level of land tax will hit both, but hit the poorer more because he has benefited less from derating. So from the point of view of incidence a low level of taxation is to be preferred to a higher rate. In order to obtain more revenue from property and to charge those enjoying local services for them, parishes should at the same time be encouraged to raise their rates (based on capital values). If this were done—low land tax and high parochial rates—no great harm would be done by the Government's doctrinaire attachment to unimproved values. Apart from the extra cost and bother of determining unimproved values, much the same economic ends would be gained as if the Hicks plan had been adopted.¹

D. J. MORGAN

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¹ I have already urged this arrangement on the Government of Jamaica. See my article "The Case for Low Land Taxation," *The Daily Gleaner* (Kingston, Jamaica, W.I.), January 20, 1958.

MR. LANCASTER'S WELFARE DEFINITIONS—A COMMENT

THIS note indicates in what respects Mr. Lancaster's welfare definitions are unsatisfactory and suggests definitions more appropriate to his declared purpose of constructing welfare theorems without the apparatus of indifference curves.

1. According to Mr. Lancaster, if the individual is offered his previous selection of goods and, in addition, at least one combination of goods not hitherto available, his welfare has unambiguously increased. This definition is unsatisfactory in all those cases where in fact he proceeds to reject the additional alternative(s) in favour of his previous selection of goods. Such behaviour warrants the inference that his welfare has remained unchanged.

Moreover, Mr. Lancaster's definition is not of a piece with his later argument (p. 466) explaining the nature of "expanded choice," in which he proposes that the loss of an area of choice which includes only previously rejected points does not make the individual worse off. In consequence, it is readily demonstrable that if we abide by his definition we can be led into self-contradictory statements.

2. The pursuit of this definition seems to have imposed unnecessary limitations on what might be legitimately inferred from a comparison of two situations. (i) In regard to a change which removes some of the choices previously available, but not the one actually taken, and adds no new choices, Mr. Lancaster says (p. 465): "We shall not make welfare judgments . . . although it would be possible to consider . . . welfare unchanged if one wished." But according to his later reasoning on expanded choice (p. 466) we have no option but to consider welfare unchanged. For his proposition that "no effective loss" arises from the removal of already rejected alternatives, if it means anything at all, means that the individual's welfare is unchanged.

(ii) As for the case in which the chosen combination of goods is removed but the individual is given, instead, alternatives not previously available, regard to consistency in choice may enable us to say something notwithstanding Mr. Lancaster's proposal that in the circumstances nothing may be said. For we may observe that the individual, on being subjected to this experiment, chooses none of the newly available alternatives; which is to say that, instead, he chooses a combination of goods he had previously rejected. In the event of such behaviour the clear inference is that the individual is now worse off as a result of the change.

3. The above criticisms prompt consideration of more discriminating definitions of welfare changes. I suggest the following should replace those advanced by Mr. Lancaster:

(a) An individual's welfare has increased unambiguously from I to II if, though the goods chosen in the I situation are still available to him

in the II situation, he chooses a combination from the new alternatives, hitherto unavailable to him.

(b) An individual's welfare has diminished unambiguously if, in moving from I to II, he chooses a combination of goods which he rejected in I.

(c) The individual's welfare remains unchanged if already rejected alternatives are removed, or alternatives which are added to his choice become rejected.

These definitions are consistent with Mr. Lancaster's main demonstration and are sufficiently comprehensive to enable us, where possible, to make welfare propositions in cases (i) and (ii) above.

E. J. MISHAN

London School of Economics.

A PYRRHIC VICTORY

Footnote 1 p. 701 of *Economic Journal* should read:—

$$(2) \quad M_t = T_t + M_i = Y_t + M_i(r_t)$$

$$(2a) \quad \frac{\partial M}{\partial R} \Delta R_t + \frac{\partial M}{\partial r} \Delta r_t = Y_t - Y_{t-1} + \frac{dM_i}{dr} \Delta r_t$$

Footnote 1 p. 703, line 9

$$M_1 = L_1 [C(Y) + I(r)]$$

H. NEISSER

New York.

CURRENT TOPICS

At the time of going to press it is uncertain whether a stoppage may occur in the printing industry before the final publication of this issue. The Editors wish to apologise in advance if in the event the distribution of the JOURNAL proves to be somewhat delayed.

THE Annual Meeting of the Royal Economic Society will be held at the London School of Economics on Thursday, July 9 at 5 p.m. After formal business, Sir Robert Hall will address the Society on "Reflections on the Practical Application of Economics."

WE regret to report that Professor A. C. Pigou, who succeeded Alfred Marshall in the Chair of Political Economy at Cambridge University in 1908 and held it until 1943, died in Cambridge on March 7. We shall hope to publish an Obituary Note in a subsequent issue.

THE British Association for the Advancement of Science will meet this year at York, September 2–September 9. The following are some of the papers which will be presented to the Economics Section (F) on that occasion: Professor J. Jewkes, Presidential Address, "How Much Science?" Professor S. R. Dennison and Professor A. Beacham, "The Restrictive Practices Court in Action." Mr. R. L. Sich, Registrar of Restrictive Trading Agreements, "Restrictive Trading Agreements." Professor B. R. Williams, "Conditions of Effective Competition." Sir Reginald Wilson, British Transport Commission, Dr. G. Charlesworth, D.S.I.R., and Mr. G. J. Ponsonby, London School of Economics, "Aspects of Transport Economics." A. A. Shenfield, Economic Director, Federation of British Industries, "The Theory of Social Balance." Professor T. Wilson, "The Affluent Society." Inquiries regarding accommodation and other arrangements for the meeting should be addressed to the British Association for the Advancement of Science, 18 Adam Street, Adelphi, London, W.C.2.

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Bruton, H. J.	Hayward, R. F.	Möller, Dr. H.
Fuz, Dr. J. K.	Horwood, Prof. O. P. F.	Somaiya, C. D.
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Egereonu, C. A.	Lam, Ho-Chuen	Perez, A. B.
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 No. 274.—VOL. LXIX.

RECENT PERIODICALS AND NEW BOOKS

Journal of the Royal Statistical Society

VOL. 121, PART 4, 1958. *The Development of the Universities in Great Britain*: SIR K. MURRAY. *Estimates of the Total Miles Run by Road Vehicles in Great Britain in 1952 and 1956*: K. N. CHANDLER and J. C. TANNER. *A Survey of Fellows: I*: K. GALES.

Economica

VOL. XXVI, No. 101, FEBRUARY 1959. *British Monetary Statistics*: H. G. JOHNSON. *Builders' Wage-rates, Prices and Population: Some Further Evidence*: E. H. PHELPS BROWN and S. V. HOPKINS. *The Physiocratic Concept of Profit*: R. L. MEEK. *Abraham Darby and the Coke-smelting Process*: M. W. FLINN. *Methodological Prescriptions in Economics*: K. KLAPP-HOLZ and J. AGASSI.

Oxford Economic Papers

VOL. 11, No. 1, FEBRUARY 1959. *An Approach to the Short-period Analysis of Primary-producing Economies*: D. SEERS. *The Bearing of Non-competitive Market Conditions on the Problem of Inflation*: J. M. FLEMING. *Mill and Cairnes on the Rate of Interest*: L. C. HUNTER. *The Theory of Public Utility Price: A Further Note*: J. WISEMAN. *Price and Quota Agreements: Their Effect on the Quality of Rubber-insulated Cable*: K. GROSSFIELD. *Is the National Debt a Burden?*: A Correction: J. E. MEADE. *Factor Intensities, Technological Progress and the Terms of Trade*: R. FINDLAY and H. GRUBERT.

The Manchester School of Economic and Social Studies

VOL. XXVII, No. 1, JANUARY 1959. *The Bank Rate Tribunal: A Symposium; An Economist's View of the Bank Rate Tribunal Evidence*: E. DEVONS; *A Political Scientist's View*: H. J. HANHAM; *The Social Background and Connections of "Top Decision Makers"*: T. LUPTON and C. S. WILSON. *Competition and the Law*: A. HUNTER. *The 1956 Restrictive Trade Practices Act: Price Agreements and the Public Interest*: J. B. HEATH. "Large" and "Small" Numbers in the Theory of the Firm: G. C. ARCHIBALD.

Transactions of the Manchester Statistical Society

125TH SESSION, 1957-58. *Free Trade in Steel?*: E. T. SARA. *Research as a Business*: D. W. HILL. *Some Aspects of European Integration*: P. URI. *Some Aspects of the Structure of British Industry, 1935 to 1951*: I. M. D. LITTLE and R. EVELY. *The Future of the Rate of Interest*: J. R. HICKS.

Scottish Journal of Political Economy

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- No. 6, 1958. *Some Problems of Our Planning*: I. VRANCHEV. *Some Methodological Problems on the Balance of Our Social Product*: G. ZHELEV. *On the Full Utilisation of the Scope for Producing Cheap Building Materials*: V. STALEV. *Conditions for a Choice Between Piece and Time Rates in Instrument and Repair Shops*: I. GELEV. *Changes in Production Structure and Scope for Expansion of Our Exports*: A. PEPELANOV. *Socialist International Lending and Its Role in the Development of Our Country*: N. TSAREVSKI. *On Economic Planning in Albania*: B. BARDOSHY. *The Technical and Organisational Plan of an Industrial Enterprise*: K. RACHEV. *The Economic Situation in Irak*: A. STOICHEV.

Finansi i Kredit (Bulgarian text)

- No. 6, 1958. *The Historic Decisions of the VII Congress of the Bulgarian Communist Party. For a Higher Level of Work in Establishing the Draft State Budget for 1959*: G. BOKOV. *Analysis of Annual Reports and Balance Sheets of Socialist Enterprises in 1957*: D. DIMITROV. *Some Problems in Credit on Goods Turnover*: K. KUZMANOV. *Some Problems of Planning Production Costs in Industrial Enterprises*: D. VAKARELSKI and M. RUSANOV. *Industrial Production Costs and Profitability in Industrial Enterprises*: V. RANKOV. *The Czechoslovak State Bank and Its Foreign Payments and Relations*: A. S.
- No. 7, 1958. *For the Protection of State Property. The Economic Efficiency of Loans for Minor Mechanisation*: R. CHUBRIEV. *The Bulgarian Unified Budget and the Creation of Special State Funds*: KH. SOTIROV. *Financial Planning by the Central Co-operative Union and Ways to Improve it*: G. SAMOKOVLIEV. *For a Wider Application of Planned Payments between Enterprises*: S. KITANOV. *For a Single Norm for Working Capital in Construction and Installation Organisations*: S. KARAGANEV. *Experience of Anti-cyclical Financial Policy in Capitalist Countries*: CH. BEYAZOV. *A Scientific Conference on Accelerating the Velocity of Circulation of Working Capital*: M. PETROV.
- No. 8, 1958. *Fourteen Years of People's Power. The Bases for Establishing Equilibrium between Resources and Outlay in the Credit Plan of the Bulgarian National Bank*: G. DIMOV. *Some Problems of the Correct Financing of Collective Farms*: G. SOTIROV. *Comparisons between Capitalist and Socialist Budgets in Bulgaria*: I. NIKOLOV. *Some Problems of Workers' Savings*: K. ANTOV. *Prices for Goods and Services Provided by Municipalities*: S. TODOROV. *The World Gold Market*: A. S. *A Meeting of Central Banks in Socialist Countries*: N. G.
- No. 9, 1958. *The Forty-first Anniversary of the Great October Socialist Revolution. The Growth of Welfare in the Second Five-year Plan and Present Tasks*: I. PALIISKI. *For Improved Financial*

Control over the Work of Draughting Organisations: I. POPOV. *The Financial Activity of Consumer Operatives in the Period of the Second Five-year Plan and Prospects for Its Development:* I. IVANCHEV. *Credits to Industrial Enterprises on Expenditure for Starting New Production Lines:* D. KHRISTOV. *Dwelling Construction and Saving for House Construction in Sofia:* D. S. *New Arrangements for Industrial and Building Finance in Czechoslovakia:* K. SUHAN. *Changes in the Revenue System of Bourgeois Budgets with Capitalist and Monopoly Profit:* M. RUSENOV.

No. 10, 1958. *For the Quickest Fulfilment of the Third Five-year Plan. Budget Estimates and Their Significance for Budget Finance:* V. GYUROV. *Exploitation Outlays in Engineering and Scope for Their Reduction:* YU. DONGHEV. *Unproductive Expenditures and Their Effect on Industrial Production Costs:* R. KOLEV and I. LAMBEV. *Insurance in the Service of Socialist Construction:* I. BOGOEV. *The Aggravation of International Financial Conflict:* V. ORNATSKI.

Ikonomicheska Misl (Bulgarian text, French and Russian summaries)

No. 8, 1958. *The XIIth Congress of the Bulgarian Communist Party and the Tasks of Economics. On Joint Investment Projects:* S. CHOLAKOV. *Correct Wage Performance for Industrial Trades:* A. DOBREV. *On the Organisation of Material and Technical Supply in Agriculture:* K. EZEKIEV. *The Advantages of Expanding Our Merchant Fleet:* B. BAEV. *The Wage System in Public Catering:* KH. KHADZHINIKOLOV. *Productive and Non-productive Labour:* V. NIKIFOROV.

Ekonomist (Serbo-Croat text, English summaries)

No. 3, 1958. *The Main Features of Post-war Industrial Location in Yugoslavia:* B. SREBRIĆ. *The National Wealth of Serbia:* I. VINSKI. *Prospects for Wheat Production and Consumption in Yugoslavia:* P. MARKOVIĆ. *Foreign Economic Difficulties of Western European Countries and Their Nature:* V. GLIŠIĆ. *On the Occasion of the 60th Birthday of Mijo Mirković:* G. GROTIĆ. *Quesnay's "Economic Table":* R. STAMENKOVIĆ. *The Market Mechanism and the Price Revision Problem for Industrial Goods:* M. TEŠIĆ. *The Location of Oil Refineries in Post-war Yugoslavia:* B. SREBRIĆ. *Discussion on Regional Statistics at the 19th Meeting of the Yugoslav Statistical Society:* G. PECELJ. *Enterprise Management and Economic Technical and Social Trends of the Contemporary World:* M. MACURA.

Ekonomiski Pregled (Serbo-Croat text, English summaries)

No. 6, 1958. *Some Aspects of the Structure of Yugoslav Foreign Trade:* I. KARLI. *The Condition and Problems of Handicraft Industry and Small-scale Production in Croatia:* V. LACKOVIĆ.

No. 7, 1958. *The Road-traffic Hinterland of the Port of Rijeka:* M. VARLANDY and B. VEGAR. *Is the Production and Consumption of Calcium Carbide in Proportion to Our Overall Industrial Growth:* I. PETRIĆ. *Operations Research:* R. BOSNER. *Characteristics and Problems of the Market for Maraška:* A. MELINČEVIĆ. *Specialisation of Teaching in Technical Secondary Schools with Special Reference to Commercial Schools:* A. ANDELIĆ.

Statistikai Szemle (Hungarian text, English and Russian summaries)

NOVEMBER 1958. *The Number of Engineers in Hungary:* GY. CUKOR and J. TIMÁR. *Communal Stock Breeding on Co-operative Farms:* I. PÁLFAI. *The Trend of Investments in 1957—I:* F. DEÁK. *The Competition of Production Volume and Productivity in the Iron and Steel Industry:* J. LACFALVI. *Results of Recent Changes in the Structure of the Output of Engineering:* M. KÁLDOR. *The Revaluation of Capital Assets in Light Industrial Enterprises in 1958:* B. POLGÁR. *Expenditures of the Hungarian National Social Insurance Fund (1949–1957):* GY. ILLÉS. *Bulgaria on the Road of Socialist Development:* GY. HAJDU.

DECEMBER 1958. *Analysis of Power Utilisation Based upon the Input-Output Table:* I. PAPP and G. RÉCZEY. *Some Questions of Foreign Trade Statistics:* I. PÁLÓS. *Findings of the First National Sample Survey on Public Road Traffic:* A. FEKETE. *The Trend of Investments in 1957—II:* F. DEÁK. *The Work of Machine-tractor Stations in 1957:* A. PELVA. *Characteristics of Production Cost Analysis in Coal-mining:* L. HÉJA. *The Position of the Co-operative Farm Movement and the Management of Co-operative Farms:* I. HAJDU. *Hungarian Kindergartens Are 130 Years Old:* D. KURUCZ. *The International Significance and Trends of Cement Manufacture:* GY. SEBESTYÉN. *Possibilities of Exchanging Electric Energy between the Countries of Central and South-East Europe:* T. KÁRMÁN. *Remarks on the Article "Current Problems of Statistics on Tourism" by Kolacsek and Markos:* E. PALLÓS. *Meeting of the Methodology Committee for Industry and Construction of the Central Statistical Office.*

Közgazdasági Szemle (Hungarian text, English and Russian summaries)

No. 12, 1958. *The Origin of Capitalist Land Rent in the Agricultural Development of Western Europe:* Zs. PACH. *Problems of Productivity in Co-operative Farms:* L. CSETE. *Prospects of Energy Production in the Three-year Plan (1958–1960):* Z. VADÁSZ. *Market Research and the Planned Economy:* E. KEMENES. *The Marxian Reproduction Scheme and the Input-Output Method:* I. ACZÉL. *Georgism:* E. SZÁNTÓ and R. HOCH. *Discussion of Some Dissertations of Candidates:* Zs. E. and J. Sz.

No. 1, 1959. *Categories of Value in a Socialist Planned Economy:* P. ERDŐS. *The Significance of the Seven-year Plan in the Peaceful Competition of the Soviet Union and the U.S.A.:* F. MOLNÁR. *Methods of Forming New Producer Prices:* I. VINCZE. *On the System of Distributing Income in Co-operative Farms. Improving Living Standards in the German Democratic Republic:* G. MANZ. *The Results of the Italian Agrarian Reform:* GY. ENYEDI. *Scientific Session of the Karl Marx University of Economics:* J. E.

No. 2, 1959. *Calculations on the Economics of Foreign Trade. The "Family Farm" in Contemporary Capitalist Agriculture:* F. ERDEI. *The Improvement of the Material Situation of the Soviet People in the Period of the Seven-year Plan:* A. BALASSA. *The Yugoslav Revisionist Views as Displayed in the Theory of the State:* R. HOCH. *Main Trends in the Development of Our Chemical Industry:* L. BONTÓ. *Discussion on the Economic Efficiency of Investments and the Establishment of New Technique in the Soviet Union:* I. FARKAS. *International Conference of the Economists of Socialist States on Scientific Co-operation. Discussion of Some Dissertations of Candidates:* Zs. E. and J. Sz.

Probleme Economice (Rumanian text, English and French summaries)

No. 10, 1958. *The Socialist System of World Economy on the Rise. A New Attempt of the Bourgeois and Revisionist Economists to Deny the Superiority of the Socialist System:* M. DULEA. *Current Problems of the Development of Soviet Agriculture on the Way to Communism:* S. NICA and D. DUMITRIU. *Some Problems of Wage Control:* I. BATI. *The Role of Natural Gas in the Supply of Energy in Rumania:* V. BOICO and I. SAVU. *Concerning the Rate of Profit in Monopolistic Capitalism:* E. DOBRESCU. *Chinese Trade with Socialist Countries:* TI KE TSIEN. *Competition Between the Chinese People's Republic and Great Britain in the Field of Industry:* V. ALDEA. *Living Standards in Czechoslovakia:* J. ADAMEC. *Data on Bulgarian Agriculture:* N. BELLI. *The Recession in Capitalist Countries.*

No. 11, 1958. *A Decisive Stage in Building the Material and Technical Base of Communism in the U.S.S.R. Democratic Centralism—the Main Principle of Socialist Economic Management:* R. MOLDOVAN. *Some Aspects of the Distribution of Urban Population in our Country:* N. HAIDUC. *Aspects of the Improvement of Wage and Rate-setting Systems in the Coal Industry:* N. SCHWARTZ. *Railway Freight Policy:* P. LUPŞE. *Metal Saving in Engineering:* V. GHIOCEL and L. APA.

No. 12, 1958. *The System of Popular Democracy—Guarantee of Continuous Economic Development. Some Problems of Current Economic Policy:* R. MOLDOVAN and V. RAUSSER. *The Importance of Soviet Aid in Economic Relations between Socialist Countries:* G. RĂDULESCU and I. BURSTEIN. *Development Prospects of the Chemical Industry in the U.S.S.R.:* M. GHEORGHIU and S. GIOGA. *New Measures for Improving Workers' Incentives:* S. ZEIGHER. *Scope for Increasing Labour Productivity in the Coal Industry:* E. BATI and M. PIRJOL. *The Subordination of the Rumanian Industry to Interests of German Capital During the Second World War:* S. MIHAI.

Ekonomia Popullore (Albanian text)

No. 1, 1959. *An Immense Programme for the Mass Construction of a Communist Society. On the Basic Methodological Regulations for Establishing an Economic Plan:* F. KOTOV and P. KRYLOV. *More Attention to Saving Material Outlays:* M. GJONI. *The Production of Building Materials on Schedule and Without Waste Is an Important Condition for Executing the Construction Plan:* S. GAMBETA. *Stocks and the Rate of Their Turnover:* H. BARDHI. *The Experience of Soviet Banking as an Aid to Our State Bank:* K. HOTI. *The Activity of Local Industry in Tirana in 1958:* TH. LIÇO. *The Innovation Movement at the Misto Mame Woodworking Plant:* TH. DENEKO. *The Bulgarian Third Five-year Plan:* A. ANGELOV. *On Planning Stock-breeding:* L. LEPURI.

NEW BOOKS

British

D'ANDRADE (J. F.). *Freedom Chooses Slavery*. London: Allen & Unwin, 1959. 9". Pp. 139. 15s.

[The gist of this not very lucid essay on world ills appears to be that it is essential to "return to the concept that work is the only way to earn a living and for a nation to gain the right to make its voice heard in world life." This concept has, in the author's view, been lost sight of by America no less than by other countries, as manifested by the stress laid on inter-governmental programmes of assistance.]

ARON (R.). *War and Industrial Society*. Auguste Comte Memorial Trust Lecture, No. 3. London: Oxford University Press, 1958. 9". Pp. 63. 7s. 6d.

[In this wide-ranging lecture M. Aron considers the causes of past wars and their relation to industrial development, giving special attention to the ideas of Comte and Veblen. He does not share Comte's optimism, but he suggests that if all countries can escape from the vicious circle of poverty and can learn to tolerate one another's ideologies, there is at least a possibility that the insanity of full-scale conflict will be effectively recognised. M. Aron's ideas are stimulating, even if the structure of his argument is at times a little elusive.]

BALL (M. M.). *NATO and the European Union Movement*. London: Stevens for the London Institute of World Affairs, 1959. 9". Pp. xi + 486. 42s.

[This is an organisational study of the steps that have been taken towards the unification of Europe in the economic and political spheres and of the relation between the European movement, on the one hand, and the wider association embodied in NATO, on the other. The author is Professor of Political Science at Wellesley College.]

BOULDING (K. E.). *Principles of Economic Policy*. London: Staples Press (New York: Prentice Hall), 1959. 9". Pp. viii + 440. 30s.

[To be reviewed.]

BROWN (A. J.). *Introduction to the World Economy*. London: Allen & Unwin, 1959. 9". Pp. 212. 18s.

[To be reviewed.]

CONDLIFFE (J. B.). *The Welfare State in New Zealand*. London: Allen & Unwin, 1959. 9". Pp. 396. 35s.

[To be reviewed.]

DORE (R. P.). *Land Reform in Japan*. London: Oxford University Press for the Royal Institute of International Affairs, 1959. 9". Pp. xvii + 510. 55s.

[To be reviewed.]

The Economics of "Under-developed" Areas. An annotated reading list of books, articles and official publications compiled by A. HAZLEWOOD. Second enlarged edition. London: Oxford University Press for the Institute of Commonwealth Studies, 1959. 8½". Pp. xii + 156. 8s. 6d.

[The first edition of this useful annotated list of books and articles was published in 1954. The new edition contains a supplement with some 400 further items that have appeared since then.]

EDEY (H. C.). *Business Budgets and Accounts*. London: Hutchinson, 1959. 8½". Pp. 174. 18s.

[To be reviewed.]

FITTON (R. S.) and WADSWORTH (A. P.). *The Strutts and the Arkwrights, 1758-1830. A study of the early factory system*. Manchester: Manchester University Press, 1959. 9". Pp. xii + 361. 35s.

[To be reviewed.]

GUILLEBAUD (C. W.). *An Economic Survey of the Sisal Industry of Tanganyika*. Welwyn: Nisbet and The Tanganyika Sisal Growers Association, 1959. 9". Pp. xii + 119. 15s.

[To be reviewed.]

HANCE (W. A.). *African Economic Development*. London: Oxford University Press for the Council on Foreign Relations. 8½". Pp. viii + 307. 30s.

[To be reviewed.]

HANSON (A. H.). *Public Enterprise and Economic Development*. London: Routledge & Kegan Paul, 1959. 9". Pp. xiv + 485. 42s.

[To be reviewed.]

HENDERSON (W. O.). *The State and the Industrial Revolution in Prussia, 1740-1870*. Liverpool: Liverpool University Press, 1958. 9". Pp. xxiii + 232. 37s. 6d.

[To be reviewed.]

HOFFMANN (W. G.). *The Growth of Industrial Economies*. Translated from the German by W. O. HENDERSON and W. H. CHALONER. Manchester: Manchester University Press, 1959. 9". Pp. xiii + 183. 25s.

[To be reviewed.]

Housing. *A Co-operative approach*. London: Co-operative Union (54 Victoria Street, S.W.1), 1959. 9½". Pp. 29. 1s.

[The most original feature of this general survey of Britain's housing problem is the suggestions made as to the possible application of the co-operative principle in housing. An appendix describes the experience of other countries in this.]

JONES (J. O.). *Agricultural Economics in Yugoslavia*. The report of a short study tour. Oxford: University of Oxford Institute of Agrarian Affairs, 1958. 8½". Pp. 24. 3s. 6d.

[This account of impressions gathered during a month's travel in Yugoslavia is concerned partly with the general state of agriculture there and partly with the work being done in Yugoslavian universities on agricultural economics.]

JUCKER-FLEETWOOD (E. E.). *Economic Theory and Policy in Finland, 1914-1925*. Basle Centre for Economic and Financial Research, Series B, No. 1. Oxford: Basil Blackwell, 1958. 9". Pp. 109. 15s.

[This contains two separate studies. The first is concerned with the question why Finland managed to avoid the economic crisis that was experienced by most countries in 1921. The analysis is conducted in terms of the four variables in the equation of exchange, M, V, P and T . Among the main reasons that emerge are the abundant supply of money, which was partly a legacy of the preceding inflation and partly the result of current monetary policy, and the absence of any wide disparity between the movements of wages and prices such as occurred in most other countries. For these and other reasons, the pattern of events in Finland after the First World War resembled developments in the world as a whole after the Second World War more than it resembled developments in other countries at the time. Mrs. Jucker-Fleetwood's second study is of the policy pursued in Finland between 1922 and 1925. This policy was a carefully planned and prepared return to the gold standard at a new parity. It was in general a success, though the author expresses some reservations about the acquiescence in the depreciation of the currency's value that was implied by the particular new parity chosen.]

KLYUCHEVSKY (V.). *Peter the Great*. Translated by Liliana Archibald. London: Macmillan, 1959. 9". Pp. xii + 282. 36s.

[This is a new translation of the fourth volume in Klyuchevsky's famous *History of Russia*, which first appeared in 1910. The translator has added some explanatory notes and references to more recent books and articles in English and French.]

Know Your Ministry. A description of government departments whose operations affect the conduct of business, revised and reprinted from *The Midland Bank Review*. London: Europa Publications, 1959. 9". Pp. 248. 25s.

[These articles were prepared in the Midland Bank's Intelligence Department, under the direction of Mr. W. F. Crick. They describe the structure and responsibilities of government departments which have dealings with industry, and they are designed primarily for the use of business-men.]

KURIHARA (K. K.). *The Keynesian Theory of Economic Development*. London: Allen & Unwin, 1959. 9". Pp. 219. 21s.

[Reviewed in this issue.]

MARSHALL (J. D.). *Furness and the Industrial Revolution. An economic history of Furness (1711-1900) and the town of Barrow (1757-1897) with an epilogue.* Barrow-in-Furness: Barrow-in-Furness Library and Museum Committee, 1958. 8½". Pp. xxii + 438. 23s.

[To be reviewed.]

MAXCY (G.) and SILBERSTON (A.). *The Motor Industry.* Cambridge Studies in Industry. London: Allen & Unwin, 1959. 8½". Pp. 245. 25s.

[To be reviewed.]

VON MISES (L.). *Theory and History.* London: Jonathan Cape, 1958. 9". Pp. ix + 384. 30s.

[To be reviewed.]

MORRIS (J. H.) and WILLIAMS (L. J.). *The South Wales Coal Industry 1841-1875.* Cardiff: University of Wales Press, 1958. 9". Pp. xii + 289. 25s.

[To be reviewed.]

NATIONAL INSTITUTE OF ECONOMIC AND SOCIAL RESEARCH. *16th Annual Report 1958.* London: National Institute of Economic and Social Research (2 Dean Trench Street, Smith Square, S.W.1), 1959. 10". Pp. 34.

[This informative report describes the progress of the Institute's current research projects. Particulars are also given of a new inquiry into investment behaviour to be carried out by Mr. T. Barna and Mr. L. Needleman.]

PILKINGTON (SIR H.). *Facing the Future in Industry.* The fifth Fawley Foundation Lecture. Southampton: University of Southampton, 1958. 8½". Pp. 26. 2s. 6d.

[This is a discursive survey of some of the economic, technical, administrative and social problems that are likely to face industry in the future. In an attempt to cover so large a field, the treatment of some subjects is more or less bound to remain at the level of the commonplace, but Sir Harry makes many points that are interesting and relatively unfamiliar.]

ROBERTSON (A. H.). *European Institutions. Co-operation, integration, unification.* London: Stevens for the London Institute of World Affairs, 1959. 10". Pp. xix + 372. 42s.

[This contains a description of the structure and functions of seven major European institutions: OEEC, the Council of Europe, NATO, Western European Union, ECSC, the Economic Community and Euratom. Briefer treatment is also given to Benelux and a number of minor organisations. The author describes his book as "an essay in that branch of international law which is coming to be known as 'the law of international institutions.'"]

SAVAGE (C. I.). *An Economic History of Transport.* London: Hutchinson, 1959. 9". Pp. 216. 18s.

[To be reviewed.]

SIGSWORTH (E. M.). *Black Dyke Mills. A history, with introductory chapters on the development of the worsted industry in the nineteenth century.* Liverpool: Liverpool University Press, 1958. 9½". Pp. xvii + 385. 35s.

[To be reviewed.]

SZCZEPANIK (E.). *The Economic Growth of Hong Kong.* London: Oxford University Press for the Royal Institute of International Affairs, 1958. 9". Pp. xiv + 186. 25s.

[To be reviewed.]

WATSON (W.). *Tribal Cohesion in a Money Economy. A study of the Mambwe people of Northern Rhodesia.* Manchester: Manchester University Press for the Rhodes-Livingstone Institute, Northern Rhodesia, 1958. 9". Pp. xxiii + 246. 30s.

[This anthropological field-study is mainly concerned with the effect on the economic and social organisation of the Mambwe people of Northern Rhodesia of the temporary absence of the younger men as wage-labourers in the mines and elsewhere. Dr. Watson's findings differ somewhat from those of writers who have studied the effects of labour migration on other tribes in that he does not find that tribal life has been disrupted by it. On the contrary, it has benefited in a number of ways.

The contrast is explained with reference to differing patterns of agriculture and also differences in kinship systems. The attachment of the Mambwe to their land and their tribalism is very strong, and no move towards permanent urbanisation is apparent. In addition to discussion of the effects of wage-labour, Dr. Watson's study also contains a comprehensive general survey of the Mambwe village and of the usual topics of interest to anthropologists.]

WHETHAM (E. H.) and CURRIE (J. I.). *A Record of Agricultural Policy 1956-1958.* Occasional Paper No. 5 of the Farm Economics Branch of the Cambridge University School of Agriculture. Cambridge: Farm Economics Branch, School of Agriculture, 1958. 9½". Pp. 44. 4s.

[This useful publication continues for a further two years the record of British agricultural policy contained in the earlier papers in the series. Legislation and price policy are described against a background of information on recent trends in agricultural incomes. Taking 1946-48 as base, agricultural incomes are shown to have risen more rapidly than national income up till 1953/54, after which the reverse was the case. The relative position of farmers was, however, improved in 1957/58, and the overall average conceals important differences between types of farm.]

WILLIAMS (GERTRUDE). *Training for Skill.* Fabian Research Series, No. 205. London: The Fabian Society (11 Dartmouth Street, S.W.1), 1959. 8½". Pp. 28. 2s.

[This pamphlet is largely based on Lady Williams' recent book, *Recruitment to Skilled Trades*. In it she considers the reasons for the present shortage of skilled labour. The apprenticeship system is in her view unsatisfactory in many respects. It is expensive and unattractive to employers, it does not ensure a good training, and it is unduly rigid in its age requirements. Lady Williams suggests that radical rethinking of the whole question is needed, and she concludes by enumerating various possible types of scheme which have been tried here and abroad and appear worth experimenting with further.]

WORTLEY (B. A.). *Expropriation in Public International Law.* Cambridge Studies in International and Comparative Law. London: Cambridge University Press, 1959. 9". Pp. xviii + 169. 30s.

[The subject of this monograph is one to which Professor Wortley has devoted many years' study. Both confiscation and nationalisation with compensation are treated, since the author maintains that the distinction between them is essentially one of degree.]

YOUNGSON (A. J.). *Possibilities of Economic Progress.* London: Cambridge University Press, 1959. 9". Pp. x + 325. 32s. 6d.

[Reviewed in this issue.]

Australian

Payment by Results. Addresses to the Third Labour-Management Conference at the University of Western Australia, October 1957, edited by D. W. OXNAM. Nedlands: Western Australia University Press, 1958. 9". Pp. v + 115. 5s.

[The first group of addresses in this symposium deals with the situation regarding payment by results in Australia to-day, the second with the problems raised by payment by results. Most of the contributions are by employees and trade-union leaders. One paper is contributed by an economist and one by a psychologist.]

Canadian

MACPHERSON (R. B.). *Tariffs, Markets and Economic Progress.* A re-appraisal of trade policies in the light of Canada's productive capabilities and long-term opportunities. Toronto: Copp Clark, 1959. 9". Pp. xi + 91. \$3.50 Canadian.

[This is a fairly sophisticated statement of the case for protectionism, as applied to Canada. The author is an economist employed in business who has gradually over a period of years moved away from the orthodox free-trade position he originally held. Among the defects of classical international-trade theory which in his view make it misleading as a guide to practical policies are its neglect of increasing returns, its unduly static character, its over-emphasis on the significance of differences in climate and factor endowments between countries and its failure to appreciate that the category of "inferior goods" may with increases in product complexity become something more than an unimportant exception.]

VINER (J.). *Canada and its Giant Neighbour*. Ottawa: Carleton University, 1958. 8". Pp. 60.

[These lectures are the first to be delivered on a new foundation. In them Professor Viner surveys the cultural, political and economic relations between Canada and the United States. He treats his subject with the light touch appropriate to a public lecture, but with much underlying seriousness. He discusses first the often-expressed fear that the distinctive features of the Canadian way of life and Canadian culture will be submerged beneath the tide of American influence. He does not altogether share this fear. He suggests, however, that any danger there may be of cultural contamination can best be met by strengthening Canadian educational institutions and avoiding the faults unfortunately present in American education to-day. On the economic side, Professor Viner sympathises with many of the complaints made by Canadians about American commercial policy. He does so, however, from the stand-point of a free trader, and he does not share the attitude of those who condemn American illiberality while advocating a policy of protection for Canada herself. Finally, he suggests that the present inflow of American capital into Canada is a desirable and indeed essential feature of the present stage of Canada's economic development, but may not necessarily continue to be so in the future, as Canadian incomes rise and growth in Canada assumes a less capital-intensive form.]

American

AVRAMOVIC (D.) assisted by R. GULHATI. *Debt Servicing Capacity and Postwar Growth in International Indebtedness*. Baltimore, Maryland: The Johns Hopkins Press, 1958 (London: Oxford University Press, 1959). 9". Pp. xvi + 228. 40s.

[This contains a statistical analysis of the growth of international indebtedness and debt service payments since the War, together with data on the growth of income, savings and exports in the borrowing countries, studied from the point of view of the capacity of these countries to service their debts and the benefits they have gained from incurring them. The authors are on the Economic Staff of the World Bank, and the book is published under the Bank's auspices.]

BANCROFT (GERTRUDE). *The American Labor Force: Its Growth and Changing Composition*. In the Census Monograph Series. New York: John Wiley for the Social Science Research Council and the Bureau of the Census of the U.S. Department of Commerce (London: Chapman & Hall), 1959. 9½". Pp. xiv + 256. 60s.

[This latest publication in the Census Monograph Series contains, like its predecessors, a very large assembly of well-digested data, accompanied by discussion. Topics treated include trends in the labour force since 1890, changing patterns of labour force participation 1940-50, trends in the part-time labour force, family employment patterns and projections of the labour force to 1975. The long-run increase in employment of married women is one of the dominant themes.]

BARCLAY (G. W.). *Techniques of Population Analysis*. New York: John Wiley (London: Chapman & Hall), 1958. 9½". Pp. xiii + 311. 38s.

[This useful text-book is designed for students interested in demographic data from the social and economic point of view, rather than for actuaries. It therefore deals not only with formal techniques, but also with the problems involved in using imperfect data of the type normally available (especially in backward countries) in order to cast light on the sort of questions that economists and sociologists are concerned with.]

BIDWELL (P. W.). *Raw Materials*. A study of American policy. New York: Harper for the Council on Foreign Relations, 1958 (London: Oxford University Press, 1959). 8½". Pp. xvi + 403. 48s.

[In successive chapters the author surveys the policies adopted by the United States with respect to each of the major raw materials which are imported in substantial quantities: lead and zinc, copper, nickel, raw materials for the iron and steel industry, wool, rubber and oil. In his concluding chapter he argues the need for a properly thought-out raw-material policy which will be consistent with other branches of American economic and diplomatic policy, instead of the series of *ad hoc* expedients which have been chiefly used to date.]

BLAUG (M.). *Ricardian Economics*. A historical study. New Haven, Connecticut: Yale University Press (London: Oxford University Press), 1958. 9½". Pp. x + 269. 40s.

[To be reviewed.]

DERNBURG (T. F.), ROSETT (R. N.) and WATTS (H. W.). *Studies in Household Economic Behavior*. New Haven, Connecticut: Yale University Press, 1958 (London: Oxford University Press, 1959). 9½". Pp. 144. 30s.

[This consists of three separate econometric studies. They are *Consumer Response to Innovation in Television*, by Thomas F. Dornburg; *Working Wives: An Econometric Study*, by Richard N. Rosett; and *Long-run Income Expectations and Consumer Saving*, by Harold W. Watts.]

DOWD (D. F.) (Ed.). *Thorstein Veblen: A Critical Reappraisal*. Lectures and essays commemorating the hundredth anniversary of Veblen's birth. Ithaca, New York: Cornell University Press, 1958 (London: Oxford University Press, 1959). 9". Pp. xii + 328. 40s.

[This contains seventeen papers on different aspects of Veblen's work, originally delivered at Cornell University and at the 1957 meeting of the American Economic Association.]

FINNEY (KATHERINE). *Interbank Deposits*. The purpose and effects of domestic balances, 1934-54. New York: Columbia University Press, 1958 (London: Oxford University Press, 1959). 9". Pp. xiv + 129.

[The holding by banks of deposits with other banks is a major factor of the system of correspondent banking, and correspondent banking is a necessary concomitant of the unitary banking system typical of the United States. The first three chapters of this monograph are devoted to an analysis of the purposes, uses, volume, distribution and movements of inter-bank deposits in the United States between 1934 and 1954. The existence of inter-bank deposits, which are regarded by their holders as a form of reserve, raises some difficult and interesting questions about legal reserve requirements. The present system by which legal minimum reserve ratios differ between different classes of city was designed to meet these difficulties, by requiring higher reserves in centres where correspondent banks are situated. But this system has some serious drawbacks, and in the author's view stands in need of reform. She believes, however, that special reserve ratios against inter-bank balances are needed in some form or other.]

HARROD (R. F.). *The Pound Sterling, 1951-1958*. Essays in International Finance, No. 30. Princeton, New Jersey: International Finance Section, Princeton University, 1958. 9". Pp. 49.

[To be reviewed.]

HENDERSON (J. M.) and QUANDT (R. E.). *Micro-economic Theory*. A mathematical approach. Economic Handbook Series. New York and London: McGraw-Hill, 1958. 9". Pp. xii + 291. 58s.

[To be reviewed.]

HIGGINS (B.). *Indonesia's Economic Stabilization and Development*. New York: Institute of Pacific Relations (333 Sixth Avenue, New York 14), 1957 (London: Allen & Unwin, 1959). 8". Pp. xxii + 179. \$4.00.

[To be reviewed.]

HIRSCHMAN (A. O.). *The Strategy of Economic Development*. Yale Studies in Economics, No. 10. New Haven, Connecticut: Yale University Press, 1958. 9½". Pp. xiii + 217. \$4.50.

[To be reviewed.]

HOBBS (S. H.). *North Carolina*. An economic and social profile. Chapel Hill, North Carolina: University of North Carolina Press (London: Oxford University Press), 1958. 9½". Pp. xvii + 380. 48s.

[This is a comprehensive factual survey of the resources, industry, commerce and government of the State. It is a sequel to a survey of the same nature published by the author in 1930.]

HOLLISTER (W. W.). *China's Gross National Product and Social Accounts 1950-1957*. Glencoe, Illinois: The Free Press for the Center for International Studies, Massachusetts Institute of Technology, 1959. 8½". Pp. xxv + 161. \$4.00.

[To be reviewed.]

IMLAH (A. H.). *Economic Elements in the Pax Britannica*. Studies in British foreign trade in the nineteenth century. Cambridge, Mass.: Harvard University Press, 1958 (London: Oxford University Press, 1959). 9½". Pp. xiv + 224. 48s.

[To be reviewed.]

KIMMEL (L. H.). *Federal Budget and Fiscal Policy, 1789-1958*. Washington, D.C.: The Brookings Institution, 1959. 9". Pp. x + 337. \$5.00.

[After a briefer introductory treatment on fiscal policy and practice in the United States before 1929, the author devotes the greater part of his book to discussion of the controversies that have taken place during and since the great depression about the proper role of government finance and the desirability of a balanced budget. The author admits that the balanced-budget doctrine still has some influence (comparison may be made with Professor Harris' article "The 1957 Investigation of the Financial Condition of the United States" in the March 1958 issue of this JOURNAL); but he suggests that no one in a position of authority still holds it as an article of faith in the way that it was held in 1931-33.]

LESIEUR (F. G.) (Ed.). *The Scanlon Plan. A frontier in labor-management cooperation*. New York: The Technology Press of the Massachusetts Institute of Technology and John Wiley, 1958 (London: Chapman & Hall, 1959). 9½". Pp. ix + 173. 36s.

[The Scanlon Plan is a form of labour-management co-operation associated with a particular kind of bonus system. The prime objective of the Plan is to provide an incentive to efficient teamwork, instead of individual incentives, which frequently lead to bad results by creating friction and by encouraging the worker to maximise his own earnings in ways that have unfavourable repercussions elsewhere in the plant. To this end bonuses under the Plan are related to overall measures of the firm's performance, such as the ratio of labour cost to sales value (profits are not favoured as a yardstick, since they depend so largely on factors outside labour's control). In certain plants where it has been adopted, the Plan has been found to have spectacular effects on morale and efficiency. The objection that general adoption of the Plan would over a long period lead to wide and arbitrary divergences between earnings in industries with differing scope for productivity increase is partly but not entirely met by the provision that norms are recalculated when new capital is installed. This volume consists of a series of papers by different writers on the Plan and its aims and achievements in practice. These are preceded by two papers on Scanlon himself, who, after a varied career as cost accountant, steelworker, professional boxer and union leader, worked on the staff of the Industrial Relations Section at M.I.T. from 1946 till his death in 1956.]

MEIER (R. L.). *Science and Economic Development: New Patterns for Living*. New York: Technology Press of the Massachusetts Institute of Technology and John Wiley (London: Chapman & Hall), 1956. 9". Pp. ix + 266. 48s.

[This is a thoughtful and comprehensive study from the scientist's point of view of long-term problems of human needs and resources. The author surveys the present state of knowledge about man's physical needs and the prospects of satisfying these needs for a world population growing at the rate which now seems likely. Attention is given not only to food and fuel requirements but also to questions of space utilisation and urbanisation and to social implications. Very broadly the author's conclusion is that as far as physical possibility is concerned there is no reason why application of present scientific knowledge and research along roughly foreseeable lines should not enable the world to support a very large population for a long time. For this to be achieved, however, considerable changes in consumption habits and attitudes will be needed, and also some changes in the emphasis of applied scientific research.]

MILLS (L. A.). *Malaya: A Political and Economic Appraisal*. Minneapolis, Minnesota: Minnesota University Press (London: Oxford University Press), 1958. 9½". Pp. xi + 234. 30s.

[To be reviewed.]

SCHOTT (F. H.). *The Evolution of Latin American Exchange-rate Policies since World War II*. Essays in International Finance No. 32. Princeton, New Jersey: International Finance Section, Princeton University, 1959. 9". Pp. 28.

[The author describes and analyses, with much interesting technical detail, the way in which Latin American countries have tended in recent years to move towards systems under which exchange rates are permitted to fluctuate for part or all of their international transactions. This tendency has been accompanied by some simplification of multiple exchange-rate practices, though multiple exchange rates in one form or another, of course, remain widely prevalent in the region. Mr. Schott suggests that the comparatively tolerant attitude displayed by the IMF towards some of the fluctuating-rate systems adopted in Latin America may conceivably presage introduction of similar systems in other under-developed countries, which, like the countries of Latin America, suffer from inflation and from unstable export markets.]

SHERWIN (S. F.). *Monetary Policy in Continental Western Europe, 1944-1952*. Wisconsin Commerce Studies, Vol. II, No. 2. Madison, Wisconsin: School of Commerce, University of Wisconsin, 1956. 9". Pp. 311. \$1.15.

[This is a study of the post-war monetary policies of six western European countries—Belgium, Denmark, France, Germany, Italy and Holland. The author's primary objective is to show the

causes of changes in price levels. His approach is historical. The book is divided into four sections covering different phases of policy: first the aftermath of the War and the conduct of currency reforms; second, the period of reconstruction ending in 1948; third, the period of recession which afflicted a number of these countries in 1948/49; lastly, the Korean boom and its collapse. In each section the aims and operations of the monetary authorities of each country are described separately in considerable detail, and an attempt is then made to draw the lessons of the period. The author recognises that many factors besides monetary policy were at work during the years under review, and the quantity theory, which is the theoretical framework of the book, is applied with an air of realism.]

SMITH (L.). *Money, Credit, and Public Policy*. Boston, Mass.: Houghton Mifflin, 1959. 9½". Pp. x + 757. \$6.75.

[This is an introductory text-book on money and banking designed for use in American universities. It maintains the traditional approach to the subject as a topic of its own, but an attempt is made to link it with other branches of economic theory at the appropriate points throughout.]

TANG (A. M.). *Economic Development in the Southern Piedmont, 1860-1950*. Its impact on agriculture. Chapel Hill, North Carolina: University of North Carolina Press (London: Oxford University Press), 1958. 9". Pp. xv + 256. 48s.

[This is a contribution to the study of the agricultural problems of the American South. The author's method is to compare the state of affairs in different counties in the region studied, in order to cast light on the causes of agricultural poverty. In 1900 the different counties in the region were all much on a par, but since then substantial divergences have appeared. The dominant factor is found to be the development of local non-farm employment. In counties where industry has developed, labour has been drawn off the land, and the incomes and productivity of those remaining have benefited. In the other counties the overall outward migration of labour has been greater, but this has not been sufficient to offset the lack of non-farm employment within the county. Much interesting detail is given on the effects of these developments on capital formation, production patterns, etc., in agriculture. One of the conclusions drawn is that in a backward region the optimum utilisation of labour in agriculture appears to be fostered by wide geographical dispersion of industry.]

WARNER (A. W.) and FUCHS (V. R.). *Concepts and Cases in Economic Analysis*. New York: Harcourt, Brace, 1958. 8½". Pp. 288. \$2.75.

[This lively collection of material is designed primarily as an adjunct to the usual type of elementary text-book. Each chapter starts with a brief digest of the relevant portion of theory, and this is followed by "cases" and questions on them. The cases consist mainly of brief quotations concerned with specific problems to which the theoretical concepts can be applied. The bulk of the material is in the field of microeconomics, and the book's main usefulness is as a collection of exercises on supply and demand, requiring intelligence and a sense of relevance rather than advanced theoretical techniques.]

WSZELAKI (J.). *Communist Economic Strategy: The Role of East-Central Europe*. The economics of competitive coexistence. Washington, D.C.: National Planning Association, 1959. 9". \$3.00.

[This is an informative and objective study of the role of the Soviet satellite countries in international trade. The author shows how complementarity has gradually developed between the economies of these countries and of the Soviet Union itself, so that each stands in need of the other for trading purposes. The author argues that any dissolution of the Communist bloc in Eastern Europe is likely on this account to be strongly resisted for economic reasons, quite apart from political considerations. Mr. Wszelaki also analyses the part played by the satellite countries, especially Czechoslovakia, in the recent Communist trade drive directed towards the uncommitted countries.]

German

HIRSCH (H.). *Mengenplanung und Preisplanung in der Sowjetunion*. Basel: Kyklosverlag; Tübingen: J. C. B. Mohr, 1957. 9½". Pp. x + 195. DM. 21.50.

[The Russian Communist technique of planning output and determining prices is discussed in a theoretical and scholarly fashion. There is a great deal of interpretation and discussion of the Communist methods in connection with their avowed ends and functions, which constantly interrupts the description of actual practices; as a result, the book is a treatise on centralisation and decentralisation, the organisation of the market process and the like, rather than an introduction to the structure of the Russian economic system.]

KNAPP (F. G.) and BENDIXEN (F.). *Zur staatlichen Theorie des Geldes*. Basle: Kyklosverlag; Tübingen: J. C. B. Mohr, 1958. 9½". Pp. vii + 268. DM. 19.80.

[The edited correspondence between Knapp, the author of the State theory of money, and Bendixen, a Hamburg banker, in the years from 1905 to 1920.]

KRUMHOFF (J.). *Gemeinsame Wege der europäischen Agrarwirtschaft*. Kiel: Institut für Weltwirtschaft, 1957. 9". Pp. 133.

[A factual and statistical survey of the post-war conditions in agriculture for the O.E.E.C. countries is followed by an extensive report of the conference on European agricultural problems of 1953. The discussion of the possibilities of the future, which completes the book, is beset by uncertainties concerning the Free Trade Area that have since been settled.]

VON MANGOLDT-REIBOLDT (H. K.). *Währungspolitische Probleme der europäischen Wirtschaftsintegration*. Kiel: Institut für Weltwirtschaft, 1957. 9". Pp. 18. DM. 1.50.

[A brief pamphlet on balance of payments problems of the Common Market and Free Trade Area.]

Swiss

KNESCHAUREK (F.). *Die Nationale Buchhaltung*. Zürich: Polygraphischer, 1958. 9". Pp. 212. Sfr. 27.45.

[The author first gives a competent introduction to national accounting and then goes on to demonstrate its use. This part consists of a large number of simple econometric examples, mainly based on United States figures; rather unexpectedly it also contains a description of a complete policy model, presumably because its definitions and several identities have been taken from the national accounting framework. The major original contribution is in the last chapter, which gives a set of preliminary national accounting estimates for Switzerland for the years 1951-54.]

LUDWIG (M.). *Internationale Rohstoffpolitik*. Zürich: Polygraphischer, 1957. 9". Pp. 127. Sfr. 18.80.

[A largely descriptive book on commodity control: the main contents are a survey of all international organisations that are (however remotely) concerned with the problem, and a series of brief descriptions of actual control schemes for various commodities.]

WERNER (J.). *Die Wirtschaftsverbände in der Marktwirtschaft*. Zürich: Polygraphischer, 1957. 9". Pp. xii + 230. Sfr. 22.80.

[Trade associations, ranging from largely social organisations to virtual cartels, have become an important element of industrial structure. Dr. Werner sets out to discuss their role in the economic system. Apart from a historical review and a comparison of Swiss and American conditions, however, the major part of his book consists of extensive summaries of bargaining theories. It is suggested that such theories would apply (more or less by analogy) to the behaviour of trade associations, but there is little information about what the latter actually do.]

WÜRGLER (H.). *Malthus als Kritiker der Klassik*. Winterthur: P. G. Keller, 1957. 8". Pp. xxiv + 200.

[In Dr. Würgler's view, the characteristic tenets of classical economics are an objective theory of price and Say's law. The first was destroyed by the advent of marginal utility, and the second by Keynes; this book sets out to demonstrate that Malthus forestalled both. As a rehabilitation of Malthus as an economist it is confined to the two points at issue. Mainly for specialists, but easy to read, it is a lively contribution to the history of economic thought.]

Dutch

HEICHELHEIM (F. M.). *An Ancient Economic History, from the palaeolithic age to the migrations of the Germanic, Slavic and Arabic nations*. Revised and complete English edition, translated by J. Stevens. Leiden: Sijthoff, 1958. 10". Pp. xi + 542. Dfl. 42.50.

[To be reviewed.]

IGGERS (G. G.). *The Cult of Authority. The political philosophy of the Saint-Simonians. A chapter in the intellectual history of totalitarianism*. The Hague: Martinus Nijhoff, 1958. 9½". Pp. 210. Guilders 14.25.

[The author's central contention in this study is that previous writers on Saint-Simon have insufficiently emphasised the rejection of democratic ideas involved by Saint-Simonian doctrines. The Saint-Simonians favoured equality of opportunity and economic planning for the public good, but they contemplated rule by an aristocracy of talent and an all-powerful state not subject to the requirement of popular consent. The author bases his interpretation on an extensive study of Saint-Simonian periodical and pamphlet literature.]

LEFEBER (L.). *Allocation in Space. Production, transport and industrial location. Contributions to Economic Analysis, No. XIV*. Amsterdam: North-Holland Publishing Company, 1958. 9". Pp. xv + 151. 34s.

[To be reviewed.]

OORT (C. J.). *Decreasing Costs as a Problem of Welfare Economics*. Amsterdam: Drukkerij Holland, 1958. 9½". Pp. xii + 187. \$1.50.

[To be reviewed.]

Danish

BERKE (K.) and USSING (N.). *Studier over Danmarks nationalprodukt, 1870-1950*. Copenhagen: G. E. C. Gads, 1958. 10". Pp. 175.

[This book provides estimates of the national product of Denmark and its composition by industry of origin, of the shares of consumption and investment, and of the composition of the balance of trade, for five-year periods from 1870 to 1952. A full account is given of the methods of calculation, and several of the underlying series are reproduced in full.]

FESTSKRIFT TIL FREDERIK ZEUTHEN. Copenhagen: Nationaløkonomisk Forening, 1958. 9". Pp. 410.

[A collection of thirty-three essays in honour of Professor Zeuthen, the majority by economists from the Scandinavian countries, of whom Brems, Frisch, Haavelmo, Lindahl and Myrdal are probably best known abroad. Although many papers are concerned with economic policy, the individual contributions are not restricted to a common theme, and a wide range of subjects is covered.]

Israel

ALEXANDER-KATZ (E.) (Ed.). *Public Subsidies in Israel. A symposium of the Haifa Branch of the Israel Political Sciences Association*. Haifa: Israel Political Sciences Association (copies obtainable from W. P. van Stockum & Zoon, 36 Buitenhof, The Hague), 1958. 9½". Pp. 52.

[The contributors to this symposium are mostly engaged in the public service in Israel. The subsidies discussed include subsidies to exports, to shipping, to agriculture, to consumers, in electricity rate policy, etc. The full text of the papers is given in Hebrew. The English version contains full translations of a few and a summary of the remainder.]

Japanese

TSURU (S.). *Essays on Japanese Economy*. Tokyo: Kinokuniya Bookstore, 1958. 8½". Pp. iv + 241.

[To be reviewed.]

Official

BRITISH

MINISTRY OF LABOUR AND NATIONAL SERVICE. *The Length of Working Life of Males in Great Britain. Studies in Official Statistics, No. 4*. London: H.M. Stationery Office, 1959. 11". Pp. 24. 3s. 6d.

[This contains an interesting collection of material in the form of life tables on the expectation of working life, the proportion of the population at work, and the numbers withdrawing from the

No. 274.—VOL. LXIX.

FF

labour force, due to death and retirement, at each year of age. These data are given in comparable form for 1931 and for 1955. Between these two dates the expectation of working life rose from 40.6 to 45.6 years, the cause being the decline in mortality. The figures of withdrawal from the labour force show the expected bulge at age 65; the percentage occupied at age 65 is 87.5, while at age 66 it is 60.0.]

INTERNATIONAL

GENERAL AGREEMENT ON TARIFFS and TRADE. Trends in International Trade. A report by a Panel of Experts. Geneva: G.A.T.T., 1958. 9½". Pp. 138. \$2.00.

[To be reviewed.]

INTERNATIONAL MONETARY FUND. Enlargement of Fund Resources through Increases in Quotas. A report by the Executive Directors to the Board of Governors. Washington, D.C.: International Monetary Fund, 1958. 11". Pp. 28.

[This report describes the policy adopted by the Fund in the administration of its resources and states the case for a 50% enlargement of quotas within the general framework of its present policy. Procedure for the proposed enlargement of quotas is also discussed.]

INTERNATIONAL MONETARY FUND. Summary Proceedings of the Thirteenth Annual Meeting of the Board of Governors, September 1958. Washington, D.C.: International Monetary Fund, 1959. 9½". Pp. ix + 189.

[This includes the address given by Mr. P. Jacobsson in presenting the Fund's 13th Annual Report. In it he describes the activities of the Fund during the past year and discusses certain issues concerning the proper role of the Fund in a recession period such as that recently experienced.]

UNITED NATIONS. Coffee in Latin America. Productivity problems and future prospects. I: Colombia and El Salvador. A report prepared under the joint programme of the Economic Commission for Latin America and the Food and Agriculture Organisation of the United Nations. New York: United Nations, 1958. 11". Pp. xi + 144. 12s. 6d.

[These two separate studies on coffee production in Colombia and El Salvador respectively are to be followed by a third on coffee production in Brazil. This third study will contain also a comparative survey of the results of the research project as a whole. Much information is given in the present volume on inputs, yields, etc., in different regions of the two countries, as well as on the part played by coffee in their national economies.]

UNITED NATIONS. The Development of Manufacturing Industry in Egypt, Israel and Turkey. New York: United Nations, 1958. 11". Pp. xiv + 131. 11s.

[This provides a useful and comprehensive survey of statistical and other data on the subject. Attention is given not only to the place of manufacturing in the economies of the three countries, but also to such questions as size of establishment, returns to capital and labour, ownership, location, cost and efficiency.]

UNITED NATIONS. World Economic Survey 1957. New York: United Nations, 1958. 11". Pp. xv + 227. 18s.

[This is divided into two parts. The first is a general survey of the course and causes of inflation during the 1950s in industrial countries, in primary producing countries and in centrally planned economies. This is a lucid and interesting piece of work, and may be recommended as one of the best general treatments of the subject now available. Part II is concerned with current economic developments, and its main theme is the causes of recent deflationary tendencies.]

UNITED NATIONS. Economic Developments in Africa, 1956-57. Supplement to World Economic Survey, 1957. New York: United Nations, 1958. 11". Pp. viii + 84. 5s. 6d.

UNITED NATIONS. Economic Developments in the Middle East, 1956-57. Supplement to World Economic Survey, 1957. New York: United Nations, 1958. 11". Pp. xi + 163. 12s. 6d.

[These two supplements to the World Economic Survey consist mainly of country-by-country surveys. Economic development in Africa during the period studied was much affected by the weakening in the markets for primary products. In the Middle East, on the other hand, the central topics are the progress of each country's internal development programme and, in the case of some of the countries, the state of the oil industry.]

THE ECONOMIC JOURNAL

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ACCUMULATION AND THE PRODUCTION FUNCTION

THE present-day interest in capital accumulation and economic development has given a new lease of life to the neo-classical problem of accumulation in an economy where the labour force and the state of knowledge are constant, with the rate of profit falling and the real wage-rate rising as time goes by.

Various recent attempts to synthesise the concept of a neo-classical production function with a Keynesian treatment of saving and investment have not been very successful. My own version, though not, I believe, erroneous, is clumsy and unconvincing.¹ Mr. Kaldor² throws away the neo-classical half of the problem. In his model there is a mysterious force that keeps the capital/output ratio constant, but if it did ever alter, on his assumptions, it would alter through a bias in technical progress, not through a movement along a production function. Mr. Little³ throws away the Keynesian half and makes saving govern investment. The brilliance of Professor Solow⁴ dazzles more than it enlightens. Professor Champernowne⁵ asks us to ascend a towering structure of argument without adequate assurance that all the rungs are firmly fixed. Mr. Ara⁶ takes refuge in "K is a quantity of capital" and Mr. Swan in sets of Meccano.⁷

The difficulty of the problem arises, I think, from attempting to rig up assumptions to make it seem plausible that a private-enterprise economy would continuously accumulate, under long-period equilibrium conditions, with continuous full employment of a constant labour force, without any cyclical disturbances, in face of a continuously falling rate of profit. But why try to make it seem plausible, when we know that in real life nothing like it ever happens? Let us take it simply as an exercise, and postulate that

¹ *The Accumulation of Capital*, Chapter 14.

² "A Model of Economic Growth," *ECONOMIC JOURNAL*, December 1957.

³ "Classical Growth," *Oxford Economic Papers*, June 1957. The elegance of this article is marred by a supply curve of labour which is left flapping about unattached to the rest of the argument.

⁴ "A Contribution to the Theory of Economic Growth," *Quarterly Journal of Economics*, February 1956, and "The Production Function and the Theory of Capital," *Review of Economic Studies*, 1955-6, Vol. XXIII, 2, No. 61.

⁵ "Capital Accumulation and the Maintenance of Full Employment," *ECONOMIC JOURNAL*, June 1958.

⁶ "Capital Theory and Economic Growth," *ECONOMIC JOURNAL*, September 1958.

⁷ "Economic Growth and Capital Accumulation," *Economic Record*, November 1956. No. 275.—VOL. LXIX.

accumulation does take place in this way for no other reason than that that is what we choose to postulate.

I

The conditions which have to be fulfilled are:

- (1) There is constant employment, without excess or deficient demand for labour at the ruling wage rate.
- (2) The ratio of income to investment satisfies the propensity to consume, without undesigned saving or dis-saving. Prices and supply and demand for individual commodities are in competitive equilibrium.
- (3) All possible technical relations between labour, capital goods and output are known and do not change through time. They are subject to conditions of diminishing returns in the sense that a rise in the value of capital per man, capital goods being reckoned at constant prices, leads to a smaller proportionate rise in output.
- (4) Those in charge of the investment plans of firms believe with perfect confidence that they know all relevant future prices and wage rates, and, in the light of these and the technical relations, always choose the physical form of investment in such a way as to maximise their profits. (This is excessively fanciful, for they have to operate with a complex of profit rates, applying to each investment the rate appropriate to the length of life of the capital goods concerned, but it is part of the neo-classical equilibrium conditions that we accept for the sake of the exercise.)

We take it that it is possible to measure a quantity of consumption goods in an unambiguous manner. All values are reckoned in terms of money of a constant purchasing power over consumption goods.

In one set of technical relations and conditions governing the propensity to consume (thriftiness conditions for short) there is one path that the process of accumulation must follow. With any given value of the stock of capital in existence (the fruit of past accumulation) the rate of investment (in conjunction with the thriftiness conditions) determines the current rate of profit and influences expected rates, in a Keynesian manner. The complex of expected profit rates (in conjunction with technical relations) determines the form in which capital is invested (the choice of technique) and the amount of employment it offers, in a neo-classical manner. There is one set of expectations that will be compatible with continuing equilibrium. At any particular moment the past course of the rate of accumulation (the ratio of net investment to the stock of capital) has been such as to make the course of profits such that (being foreseen) they have caused the capital to be embodied in forms that offer employment to just the amount of labour that is available. And they must continue to be such as to fulfil this requirement.

The Keynesian freedom of entrepreneurs to invest as they please has not been sacrificed to the neo-classical conditions, but to the postulate that equilibrium is never ruptured. If, at any moment, the rate of investment rose above the level required by the prescribed path of accumulation, there would be a rise in the share of profits and a failure of real wages to reach the prescribed level. There would then be an excess demand for labour and an inflationary situation would develop. If the rate of investment fell below the prescribed level, a slump would set in. Nothing prevents this from happening except the postulate that equilibrium is maintained. The Keynesian forces are being asked, for the sake of the exercise, to dance to a neo-classical tune.

Granted that at any moment the *rate* of gross investment is such as to give full employment and full-capacity working of the existing stock of capital, it must be of a *form* which is such that full-capacity working will be compatible with full employment when the equipment now being built is in use. The allocation between investment and consumption, and the techniques chosen, must be such as to keep the supply of goods in equilibrium with demand, and demand for labour in equilibrium with supply. If, say, too much were being put into enlarging capacity in the investment sector, then a scarcity of plant and inflationary profits would develop in the consumption-good markets. Or if too capital-using a technique were being installed, current gross investment would be replacing old machines by a quantity of new machines which required less labour to operate, and full employment would become impossible.

There is one pattern of physical investment that will preserve equilibrium, and one pattern of expectations that will induce investment to take that physical form; it is just these expectations that are confidently held by the firms carrying out investment. The expectations being compatible with equilibrium, the plans based on them will maintain equilibrium and cause the expectations to be realised.

Since this is true at every moment, the history of accumulation is prescribed and the evolution of the stock of capital through time is determined by it. At any moment there is a particular assemblage of concrete capital goods, including stocks and work in progress, that can be listed in a who's who, specifying their technical properties, age and prospective life. And there is a particular pattern of prices, such that the value of the stock of capital, of the rate of investment and real income, and the division of income between wages and profits, are compatible both with the technical relations and the thriftiness conditions at that moment. The who's who and the values for all past and future dates are implicit in the situation at that moment, and the whole history, backwards and forwards, can be seen at any moment in it. Looking back, the value of the stock of capital dwindles till it is vanishingly small, and the rate of accumulation indefinitely great. Looking forward, the value of the stock of capital is growing and the rate of

accumulation sinking towards zero. The stationary state appropriate to the given conditions is the asymptote which the system is approaching at a predestinate pace along its predestinate path.

We let ourselves in for this when we accepted the neo-classical postulate of continuous equilibrium with full employment.

II

The thriftiness conditions are stated in terms of the ratio of net saving to net income. The provision which capitalists make for amortising investments over the life of the capital goods concerned are not counted as net saving. Our second equilibrium condition, therefore, must be stated in terms of net investment. It is net investment that net saving has to be equal to. What exactly do we mean by the value of a year's net investment, and how is the net investment related to the growth in the value of the capital stock over that year?

We have no need to worry about any pseudo-Wicksell effects¹—revaluations of existing capital goods when changes occur in the rates of interest. The complex of interest rates for different terms correctly reflects the complex of prospective rates of profit. Our capitalists never make a mistake and are never surprised by the fall in interest rates that occurs as time goes by. The difference in value of a capital good made by expected changes in interest rates over its life is already in its price to-day, and no revaluation will be called for when the expected happens.

On the other hand, we do have to take account of real Wicksell effects. As time goes by the real-wage rate and the complex of interest rates are altering. The reproduction cost (in terms of money of a constant purchasing power over consumption goods) of any fairly long-lived capital good is equal to its historic cost only by a fluke. It may be greater or smaller, according as the effects of higher wage rates more or less than offset the effects of lower interest rates. Generally a piece of equipment will not be replaced by a replica of itself, for during its life-time different techniques have become eligible, but the issue can be stated without supposing that it is.

Let us suppose that we calculate what the latest type of equipment would cost on the basis of the wage rate and the relevant interest rates that were ruling when the equipment now being replaced was new. A physical quantity of the new plant that, at the old prices, would have a cost equal to the historic cost of the old plant may be regarded as the "equivalent" of the old plant. In terms of consumption goods to-day, a sum equal to the initial cost of the old plant may purchase more or less than its "equivalent," according as the effect on the cost of plant of the new type of lower interest more or less than outweighs the effect of higher wages. By this means we can detect whether money of a given purchasing power over commodities

¹ Cf. Joan Robinson, "The Real Wicksell Effect," *ECONOMIC JOURNAL*, September 1958.

now has a higher or lower purchasing power over capital goods, and so separate out the part of the change in value of capital per head which is due to changes in factor prices from that which is due to the change in the physical quantity of capital.¹

In the case where an amortisation fund equal to the historic cost of a plant will purchase more than its "equivalent" when the old one comes to be replaced, the additional physical capital is not counted as part of net investment. It is just thrown in. It means that the physical and technical counterpart of a given value of capital has become bigger and better without any net saving having been required. On the other hand, when the cost of its "equivalent" has risen over the lifetime of the old plant the finance which has to be added to its amortisation fund in order to re-create an "equivalent" amount of plant is included in net investment.

Our capitalists need not be conscious of all this. It is only necessary that they (and the economists observing them) follow the rule of calculating net profit after providing for the maintenance of the value of any finance invested in the past. So long as this rule has always been followed (without mistakes in foresight), the total value of the capital in existence to-day is the sum of all the net investments that have been made in the past. But it means that there is no simple relation between a sum of value of capital and any kind of index of physical capital. The relation between output per unit of labour and the value of capital per head is not a purely technical one. It combines the effects (in various proportions in different situations) of technical relations with the effects of differences in factor prices.²

The neo-classicals hanker after a simple one-dimensional index of physical capital. Formerly they tried (as Wicksell with his grove of trees) to reduce physical capital to terms of a simple time-dimension. Nowadays the fashion is rather to try to reduce the time dimension to technical terms by postulating that capital goods are one-hoss shays that work with perfect efficiency for a technically determined time and then fall to pieces. Bigger and better one-hoss shays are somehow to be expressed in units of a standard shay. But even if a reasonably satisfactory physical unit can be devised, it is no help.

At any particular moment, with given wage rates and prices and a given complex of interest rates, the technical choices open to investors can be set out in terms of value of capital per unit of labour, and a physical unit is not necessary. If we had one, we could reckon the quantity of gross investment in terms of it, but it would not help us to detect how much of the gross investment was net, for depreciation allowances are designed to maintain the value of capital, not its quantity in terms of any particular physical unit.

Thus the interplay between the thriftiness conditions (which run in net value terms) and the technical relations (which run in gross physical terms)

¹ Cf. T. Swan, *loc. cit.*

² I have been much complained of for this, like the messenger in an old play who brings bad news.

is extremely complicated. All the same, under the rules governing our exercise, it is perfectly determinate. At each point on the prescribed path there is a certain value of capital, a certain concrete stock of capital goods, a certain flow of physical outputs of consumption and capital goods, and certain levels of wages and prices. The technical and the value relationships are always fitted together to fulfil the equilibrium conditions in terms of which the exercise is conducted.

III

As accumulation goes on, the value of capital per unit of labour is continuously rising. How does this affect the relative share of wages and profits in total net income?

To discuss this question we must specify the thriftiness conditions. Let us make them simple. We postulate a two-class economy in which the workers' consumption is equal to their wages, while capitalists spend a lump sum, say b per annum, which is independent of income, plus a proportion $(1-s)$ of what they properly regard as net profit. Then, when designed savings are equal to investment, net profits per annum are $(b+I)/s$, where I is a year's net investment. The constant, b , is not of any importance in the present context (it will come in handy later); we may assume it to be negligibly small in relation to I . The share of profit in income then rises or falls in proportion to the ratio of investment to income.

Now, as time goes by, the ratios of income and of investment to the overall value of the stock of capital are changing (in general, both are falling). There is no presumption as to how the ratio of investment to income will change.

Its behaviour at each phase of accumulation depends upon the relation between real wages and output per head. Generally speaking, as capital accumulates real-wage rates rise, and are expected to rise further; the degree of mechanisation of each new lot of capital goods installed (exception made for cranky cases) is greater than that of the last, and output per head is tending to rise. When the rise in real wages, over a certain period, has been accompanied by a more than proportionate increase in the value of output per head, gross profits per man employed have risen, and there must have been an increase in the ratio of investment to income to absorb the increase in savings per unit of value of output that this entails. Otherwise a deficiency of demand would have developed and the increase in mechanisation would have thrown men out of work. Conversely, when a rise in real wages has been accompanied by a smaller increase in output per head, the ratio of investment to income must have fallen sufficiently to permit the consequent increase in consumption to take place without an inflationary excess of demand.

When the rise in output per head happens to be exactly proportional to the rise in wages associated with it, the ratio of investment to income is

constant and consumption is increasing at the same rate as total net income. This is the case of constant relative shares.

How does this agree with the neo-classical doctrine that the behaviour of relative shares depends upon the "elasticity of substitution" between labour and "capital"? It is usually far from clear what this elasticity is intended to mean. The neo-classicals are in a dilemma. If they state their doctrine in terms of an index of physical capital they find themselves discussing net profit per unit of physical capital, when what they need is the rate of profit on capital, which governs the choice of technique. On the other hand, if they state it in terms of the value of capital, the definition of the elasticity of substitution becomes circular—the elasticity is greater or less than unity according as the relative share of profit in income rises or falls when the value of capital per man increases.

But however confusedly the doctrine is stated, it contains a substantial point. A situation in which a small rise in real wages causes a large switch to more mechanised technique is one of high substitutability of capital for labour, and where a great rise in real wages can occur with only a small rise in output per head substitution is weak.¹ The level of wages which affects the choice of technique is not only the level ruling when a choice is made. The future levels expected over the lifetime of any equipment set up to-day affect its prospective profitability and play a part in the choice.² Thus the relationship between wages and the choice of technique is immensely complicated at any moment and is liable to shift as time goes by. It is not adequately expressed by a two-dimensional elasticity. But taking it broadly, when the technical relations are favourable to substitution, the ratio of investment to income has to be rising to prevent unemployment. Thus, broadly speaking, easy substitutability causes the share of profits to rise as capital accumulates, and sticky technical relations cause the share of wages to rise.

When it comes to specifying precisely the condition for relative shares to be constant, however, we can only say that it is such that the rise in the value of output per head (over the economy as a whole) which accumulation is bringing about is exactly proportionate to the rise in real wages that is consonant with the physical form that gross investment is taking.

In short, when equilibrium conditions require constant relative shares, constant relative shares is what equilibrium conditions require.

IV

In spite of the stringent requirements of equilibrium, the course of the prescribed path may be very varied.

¹ In my *Accumulation of Capital*, Chapter 14, I rigged up assumptions to make substitutability zero and infinite in alternating phases.

² In the analysis referred to in the above note I assumed very short-lived capital goods to evade this problem.

For instance, it is in general true that, as the value of capital grows, the ratio of income to capital falls, but this need not be the case all the time. When the price of "equivalent" capital goods has recently fallen sharply the equipment for a more mechanised technique may cost less now, per unit of output, than that for a less-mechanised technique did earlier (of course, with the prices and interest rates prevailing at any one time, the more mechanised technique has the higher capital cost per unit of output). Moreover, the capital/output ratio may vary very much between one sector of the economy and another, so that the change in the overall ratio depends upon the allocation of investment between sectors.

Again, the rate of accumulation (the ratio of net investment to value of capital) is in general decelerating, but it is not impossible that it might speed up for a time. At a particular stage it might become appropriate to invest in productive capacity for investment to such an extent that the productive capacity for consumption goods actually declines—re-investment of amortisation funds syphons capital from one sector to the other. The share of profit must then rise so much that real wages actually fall. The current rate of profit on capital rises.

For the moment less-mechanised techniques become eligible, but this applies only to short-lived capital goods, for the very fact that the current rate of profit is exceptionally high means that the longer-term rates fan out more steeply. The very fact that the rate of accumulation has risen means that the economy is moving faster than before towards a period of lower profits.

The prescribed path may contain many such sinuosities, but diminishing returns will prevail in the end. Over the long sweep, income per unit of capital, the rate of accumulation and the current rate of profit must all be falling. The system is moving towards the stationary state which is the asymptote of its prescribed path.

Each set of thriftiness conditions has its appropriate stationary state. In the conditions that we have postulated, when net investment is zero net profit per annum is equal to b/s , that is, to the annual value of capitalists' consumption. The value of the stock of capital and the rate of profit (which in a stationary state closes the fan and becomes a single, constant rate) have to be such as to yield that net profit, while keeping the capital intact, and this requirement, with the technical relations, determines the physical composition of the stock of capital and the level of wages.

In the stationary state the neo-classical conditions have to dance to the Keynesian tune.

V

The requirement that there is in existence, whenever we break into the story, an equilibrium stock of capital (because there have been correct

expectations in the past) deprives our exercise of application and reduces it to a mere pastime.

What has led us astray is the bad habit (which Marshall tried to resist, but Wicksell freely indulged) of conducting an analysis of movements through time as though they occurred in stationary equilibrium.

The neo-classical argument starts at the point where an economy is already in a stationary state. Compare two economies of which one, Alpha, relatively to the other, Beta, is more thrifty or is performing more "waiting," or what you will. Alpha in any case, has a lower rate of interest than Beta when there is zero net saving in each. The stationary state implies that any past aberrations due to mistakes of foresight have long ago been digested and the future is expected to be simply a continuation of the present. In each economy the who's who of capital goods, and all prices and wage-rates are determined. The rate of profit on capital is equal to the zero-saving rate of interest. The degree of mechanisation of technique is higher in Alpha, where the rate of profit is lower. If we had some reasonable physical measure of capital, physical capital per head could be said to be higher in Alpha. It would be only in a rather peculiar case that the value of capital would not also be higher. Thus it seems fairly safe to say that Alpha has "more capital" without bothering about how capital is measured.

Now, having established that the economy with "more capital" has a lower rate of profit, the neo-classicals jump into motion and argue that an *increase* in the stock of capital *lowers* the rate of profit. But that jump shatters the equilibrium and deprives them of the right to apply what they have deduced from equilibrium relations to what will happen next.

IV

To escape from the requirement of an equilibrium past, let us descend into a short-period situation, and postulate that there is a specific stock of capital goods, that just happens to be whatever it is.

We can rig up full-employment assumptions as follows. There is a more or less definite capacity output of the equipment in the consumption sector (given habits about shift-working, etc.) and the price policy of firms is such that they work to capacity. A higher or lower level of demand is then reflected in higher or lower gross profit margins, not in more or less output and employment. The number of men employed on the consumption-good equipment being given, skilfully operated financial controls work in such a way as to induce enough investment to employ the rest of the labour (we may suppose either that there happens to be enough equipment to offer the requisite employment, or that there are some navvying jobs, where the ratio of labour to physical equipment is very high, which can take up any slack).

We now have full employment guaranteed whatever the past and future

state of expectations may be. Profits yielded by the various capital goods are all over the place, for the various stocks in existence are quite arbitrary relatively to the pattern of demand. The techniques in use are not in equilibrium with the level of wages and profits.

The amount of investment is regulated, but its form is left to profit-seeking firms to decide. The capital goods being produced are designed to increase capacity in what appear to promise to be the most paying lines, and the techniques being chosen are influenced by expectations of current and future wages and prices. A pale ghost of a tendency towards equilibrium is floating around the planning of investment. But expectations in the past have not proved correct (for the stock of capital we started from was not in equilibrium) and there is no warrant for postulating that the expectations to-day guiding investment plans will turn out to be correct in their turn. Indeed, there is no reason why all the various expectations being held independently should be consistent with each other. It is not possible for each firm to know what all the others have planned to do, and to deduce from this knowledge how its own position will be affected. Since the expectations being held are not self-consistent, they are not capable of being justified by the event. The investment now being done is going to create another out-of-equilibrium stock of capital goods. So the system will lurch from one short-period position to another.

Nevertheless, if the full-employment level of investment is maintained and capacity for producing consumer goods is being enlarged, real wages are rising, for the full-capacity level of prices is falling relatively to money-wage rates as capacity grows. Consequently (in the absence of technical progress) the techniques being chosen at each moment are influenced by expectations of a higher cost of labour in terms of product than were those chosen earlier. Therefore the degree of mechanisation of technique is, in general, rising as time goes by and the current rate of profit on capital falling. This is all we can offer to appease the neo-classical ghost.

In all this technical progress has been ruled out. When technical progress is going on, the poor ghost may have very little to play with, for then there will often be one obviously best technique to choose at each round of investment, which though it requires a higher value of capital per man employed than others, also offers a higher output per unit of capital. Only when accumulation is so rapid that it cannot find an outlet in installing superior techniques (or giving innovations a capital-using bias) does it spill over into raising the degree of mechanisation on a given production function.

Very rapid accumulation is characteristic, nowadays, of socialist economies; it is there, if they would have it, that the neo-classical ghost could find a congenial home.

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INCOME ORIGINATING IN THE MODELS OF HARROD AND DOMAR¹

THE growth models of Harrod and Domar are widely believed to be very similar, and this belief is expressed in the frequent reference to these models as the Harrod-Domar models.² In large part, this view stems from the fact that Domar's required rate of growth of investment works out to be the same as Harrod's warranted rate of income growth. (See below for proof of this.³) The similarity of results has always been a puzzle to this writer. In Domar's basic equation, $\Delta I (1/\alpha) = I\sigma$, the multiplier is explicitly included, while in Harrod's warranted rate the acceleration principle is explicitly included. Since the mechanisms and the workings of the multiplier and the accelerator are different, it is strange that they should produce the same results. That is to say, assume an economy that has grown at a rate equal to the marginal propensity to save times the capacity effect (in Domar). This rate will be formally the same as Harrod's warranted rate equal to the average propensity to save divided by the required capital coefficient.⁴ Assume in the first instance that this economy has grown at the required rate via the accelerator. Does this mean that Domar's model does not hold for this economy? Assume in the second instance that the economy has grown at this rate via the multiplier. Does this mean that Harrod's warranted-growth model does not hold? Assume, thirdly, that the economy grew via both the accelerator and the multiplier (as in the Samuelson model). Does this mean that neither models apply? Or should not the assertion that these models are similar imply that the *same* mechanism of growth (whether accelerator or multiplier) is viewed from different standpoints?

This problem may be stated more specifically by means of a simple numerical example (taken from Domar's article in the *AER*, March 1947,

¹ The writer is indebted to Miyohai Shinohara of Hitotsubashi University, Albert Rees of the University of Chicago, Kenneth K. Kurihara of Rutgers University and Philip Cartwright and Donald Gordon of the University of Washington for discussion on various aspects of the paper. This paper does not deal with issues such as the self-sustaining nature of the warranted rate. It is solely concerned with the validity of the warranted rate and the required rate from a social-accounting point of view. The problem may be specified as: when the economy is growing at the required rate, is there any need for the multiplier effect to keep capacity fully operating? The answer given in this paper is no. It is claimed that the income originating in the operation of the new capacity is sufficient for full operation. (T. Kondo of Japan's Economic Planning Agency has independently arrived at similar conclusions.)

² See, for example, Domar in the *American Economic Review*, March 1947, p. 42. As far as the writer can make out, Harrod has nowhere committed himself to this view. Also T. C. Schelling, *American Economic Review*, December 1947, p. 864.

³ See T. C. Schelling, *American Economic Review*, December 1947, p. 864.

⁴ Both writers assume an equality of marginal and average propensity to save. The capacity effect is the reciprocal of the capital coefficient. Both writers take these to be constant, so that the required rate of growth of investment (Domar) is equal to the warranted rate of income (Harrod).

pp. 41–2.) Assume an economy where S/Y is 12% and is equal to $\Delta S/\Delta Y$, $\Delta K/\Delta Y$ is 4 or $\Delta Y/\Delta K$ is 25%, Y (income) is 150, I (investment equal to saving) is 18, C (consumption) is 132. (All these refer to the same period following the periodising preferred by both writers.¹ The absolute units are in billions of dollars.) Using the formulas of both, the rates of investment increase and income increase are 3%. (ΔY is 4.50, which works out to be 3% of 150 and ΔI is 0.54, which is also 3% of 18.) And yet the mechanisms by which these identical results are obtained are different. In Domar when the economy is moving at the required rate the incremental portion (or as he puts it the “very top”) of investment becomes a multiplicand and via the multiplier sequence generates ΔC to the extent of 3.96 and ΔS to the extent of 0.54, this amount comprising the saving induced by and matching the new money injected into the system (or the multiplicand). No part of ΔI or for that matter the rest of I is induced by ΔC and ΔS , which together comprise ΔY of 4.50. Rather it is the other way around. ΔI may be regarded as inducing ΔC and ΔS .

This is completely different from Harrod's warranted-growth equation. ΔK in the required capital coefficient including the “very top” is induced by the ΔY of 4.50 or the implicit ΔC of 3.96 and ΔS ($\equiv \Delta I$) of 0.54. Since, corresponding to ΔS there is already 0.54 of investment goods, ΔI is not a multiplicand. Thus in Domar the ΔY generated by the required growth rate of investment is the result of the multiplier, while in Harrod the ΔY in his warranted-growth rate is the result of the accelerator. These are *not* two ways of looking at the same movement (as they should be if they are the same) but are two different movements.²

How, then, is one to account for the same solution in the Domar model and in the warranted model? It will be argued that the similarity of results is accidental, arising from a serious social-accounting error in Domar's multiplier model. This error is due to Domar's failure to include an important demand flow in his theory, namely the income originating in the productive operation of the I shown on the right side of his basic equation, $\Delta I (1/\alpha) = I\sigma$, where α is the marginal propensity to save and σ the capacity effect. When the economy is growing at Domar's required rate, the incre-

¹ See Domar, *Econometrica*, April 1946, and Harrod, *Economic Journal*, June 1951, pp. 268–9. The argument may be stated with the use of two or more periods. Since the individuals mentioned in the first footnote disagree with the criticism outlined in this paper and appear to rest their case on period analysis, my point is restated briefly at the end of this paper using two periods.

² The foregoing discussion is confined to Harrod's warranted rate of growth. His actual or “trial and error” rate of growth can be interpreted as including not only the accelerator but also the multiplier, since the actual capital coefficients do not represent capital requirements. In discussing S. Alexander's attempts to liken Harrod's system of ideas to Samuelson's multiplier–accelerator model, Harrod remarked that there is no dynamic equilibrium (*i.e.*, the warranted-growth rate) in Samuelson. *Economic Journal*, June 1951, p. 271. If our discussion in the text is valid, the departures of the actual from the warranted rate in Harrod may be due largely to a positive multiplicand when $G > Gw$ (stocks fall below normal) and to a negative multiplicand when $G < Gw$ (stocks rise above normal). For the purpose of this paper, the relevant comparison is Domar's required rate and Harrod's warranted rate.

ment of capacity represented by I is put into operation; to the output of goods represented by $I\sigma$ there always corresponds an equal amount of wages, salaries, rent, profits, etc., paid out for producing $I\sigma$ goods. (Such income flows are designated as income originating in the productive capacity of I in social accounting.) This *income originating* is not included on the left-hand side of his equation, which deals entirely with the *multiplier incomes*, to designate them separately from income originating. In other words, there are not just two aspects to the investment process as Domar maintains, the capacity effect and the multiplier aspect of the increment of I , but a third, the income-originating effect of the total I , under conditions in which the economy is operating at the required rate.

To show that the income-originating flow is a separate and distinct flow from the multiplier income flow, we may revert to the numerical example given above. If, as Domar maintains, the ΔI of 0.54 is a multiplicand—although it will be shown below that this cannot be—then the construction of ΔI worth of capital goods financed by new money will start a multiplier sequence. The factors of production participating in the construction of ΔI will receive wages, salaries, rent, interest, profits, etc., to the extent of 0.54 at the outset. These factors of production will spend (to the extent of the assumed marginal propensity to consume) part of their income on consumption goods (or 0.48) in the second round. The factors producing these consumption goods (food, clothing, services) will receive 0.48 of income and will, in turn, spend (to the extent of $\Delta C/\Delta Y$) part of their income on consumption goods in the third round; and so on in the familiar multiplier sequence.¹ Note that the income recipients in this sequence are the factors in the construction and consumption goods trade.

In contrast, the income-originating flows are received by the factors of production who are hired as operatives, etc., in the factories represented by the total I of 18. To bring out the contrast most sharply, assume that the total I represents factories producing automobiles and other consumer durables. The factors of production hired by these factories receive wages, salaries, etc., as these factories produce consumer durables throughout the year. The income originating is the monthly wage bill, etc., regularly paid out to the operatives, etc., of these factories only and no one else. These are not multiplier flows, since no multiplicand is involved.² (It will be shown below that these income-originating flows are identical with the incomes which induce ΔK in Harrod's warranted-growth equation.)

If the economy is growing at Domar's required rate—and assuming that his σ and s are equal—the income-originating flow is an *additional* stream of income and exactly equal in magnitude to the multiplier flow of income.

¹ To be technically correct, these sequences should be worked through changes in stock, but it will take us too far afield to restate the multiplier sequence through changes in stock.

² In the real world some merging, and thereby duplication, may be involved between the two type of flows, but this is negligible and by chance.

Accordingly, it must be added to the multiplier side of his equation and to the extent of this addition, Domar's theory (as distinct from the solution of his equation) gives inflationary results.¹

But the question arises: if the foregoing point is valid, why and how is it that Domar's required rate of investment growth comes to the same solution as Harrod's warranted rate of income growth? The answer to this puzzle requires a separation of Domar's theory from the solution of his equation obtained by solving for $\Delta I/I$. Domar's theory runs in terms of the multiplier. It is the demand generated by the multiplier mechanism which creates the demand necessary to put into operation the factories represented by ΔK or I . But since he has failed to include the income-originating flows, only the multiplier income flows appear on the left-hand or demand side of his equation. As noted previously, the income-originating flows are exactly equal to the multiplier-income flows if σ is equal to s . This may be demonstrated as follows. The income-originating effect may be defined as follows:

$$(1) \quad \Delta K/Y \cdot \Delta Y/\Delta K = \Delta Y/Y$$

The left-hand side may be regarded as denoting the income-originating effect of ΔK or total I . ΔY on the right is the additional income produced or originating in the operation of ΔK , while $\Delta Y/\Delta K$ is the income originating per unit of ΔK . The latter will be recognised as identical with Domar's capacity coefficient. The right-hand of Domar's basic equation, $\Delta I (1/\alpha) = I\sigma$, may be written as $I \cdot \Delta Y/\Delta K = \Delta Y$. Dividing both sides of the equation by Y , we get:

$$(2) \quad \frac{I \cdot \Delta Y/\Delta K}{Y} = I/Y \cdot \Delta Y/\Delta K = \Delta Y/Y$$

This equation is the same as equation (1) since $I = \Delta K$. Domar's multiplier side is made to be equal the capacity created on the right-hand side of his basic equation, so that the multiplier effect is always equal to income originating, when $\sigma = s$.

Harrod's warranted growth equation, $G_w C_r = S$, may be written as $\Delta Y/Y \cdot \Delta K/\Delta Y = S/Y$, where the left-hand side is taken to represent the acceleration effect. If this equation is solved for G_w , it becomes $\Delta Y/Y = \Delta K/Y \cdot \Delta Y/\Delta K$, which is equation (1), the income-originating equation. This implies that the *magnitude* of the acceleration effect on income is equal to the income-originating effect. But, in contrast to the multiplier effect on income in Domar's theory, the income flows in the acceleration effect are

¹ To the extent that $\sigma < s$, income originating in the operation of ΔK is less than the multiplier effect of ΔI . Under normal conditions the departure of σ from s is probably small, since only the increment in the rate of obsolescence causes discrepancy between σ and s in the long run, the non-incremental portion being included in depreciation. The differences between s and σ cited in the *American Economic Review*, March 1947, pp. 44-5, are not relevant in the context of the above discussion. Accordingly, for our purpose it is convenient from an expository point of view to regard $\sigma = s$. The argument can be stated without this equality but not as briefly as in the paper.

identical with those in the income-originating effect and are, therefore, not additive. And they are identical because the ΔK required to produce ΔY is the capacity from which ΔY originates. The difference of the two effects is in the fact that, viewed as acceleration effect, the relation of $\Delta Y/Y$ and $\Delta K/\Delta Y$ expresses a theoretical (or causal) relationship while viewed as income originating effect it is a non-causal, accounting relationship.

In sum, Harrod's warranted growth model does incorporate the income-originating effect in the form of the acceleration effect. Domar's basic equation does not do this, but the solution derived from the equation is equal by definition to Harrod's warranted rate because the multiplier effect of ΔI is made to equal the income originating in I . But since the income-originating effect (or income) is always present when the economy is operating at the required rate or the warranted rate, the failure to include it destroys the value of Domar's theory as distinct from the solution of his equation.

The foregoing analysis leads to the conclusion that it is not necessary for the economy (operating under the assumption of a given $\Delta S/\Delta Y$, capital coefficient, etc.) to generate a multiplicand as required in Domar's theory. This conclusion seems plausible even from the point of view of general reasoning entirely apart from the severe deductions based on the algebraic manipulation of rigorously defined quantities, given coefficients and assumed relationships. Intuitively, it seems strange that there should be a need for the injection of new money for the purpose of establishing the multiplicand in Domar's system where presumably the tendency is toward excessive saving. To assume that there is a need for injecting new money to set up a multiplicand implies that current investment is absorbing current savings.¹ This condition in turn implies that past investment was not excessive, so that there is no tendency toward capacity to exceed aggregate demand. The need to inject new money to set up a multiplicand seems inconsistent with Domar's thesis of excess capacity.

Domar's reasoning on this point is as follows:² since an increase in I is needed for the requirements of adequate growth, additional money must be injected into the system. This injection of new money is mistakenly taken to constitute a multiplicand. Not all new money injected to transact a larger output induces a multiplicand. For new money injected into the system to be a multiplicand, it must constitute additional purchasing power for which there is no consumer-goods counterpart elsewhere in the economy (so that, as the saying goes, "money chases after goods"). But the need to inject new money to finance ΔI implies that savings are equal to $I - \Delta I$ in Domar's system; and this equality signifies that without the ΔI the system is already operating at required rates, taking into account demand from income

¹ This last point is brought out more clearly in Harrod's approach, which is built directly on the balance of demand and supply of savings. It shows also that there can be no multiplicand (at least in the net sense, positive minus negative multiplicand) in Harrod's warranted-growth model.

² See *American Economic Review*, March 1947, pp. 40, 42, 47.

originating in the operation of $I - \Delta I$ capacity.¹ There is actually no need to inject new money to finance Domar's ΔI . This can be done from that part of income originating in the operation of $I - \Delta I$ capacity which is saved. It is, of course, true that new money is needed (assuming velocity of money constant) to finance the transaction of the enlarged rate of production (and income). But this injection comes in at the point where production operations begin for I (or 18 worth of capacity). In Domar's model $I\sigma - \Delta I$ (or $4.50 - 0.54$) must be consumer's goods. The new money (short-term loans) financing the production of these consumer's goods does not constitute a multiplicand, since to the extent of such new money there are consumer-goods counterpart. And since $\Delta Y - \Delta C$ (or $4.50 - 3.96$) is equal to 0.54 , the latter is the saving for financing ΔI , mentioned above. Thus, in Domar's theory there are two errors, the omission of the income-originating flows and the inclusion of multiplier flows. These errors offset to give a solution identical with the solution from Harrod's theory.

CONCLUSION

Domar's model does not need nor should have a multiplicand. In reality, the left-hand side constitutes the income-originating effect, and it is this which gives a solution in his model identical with Harrod's warranted rate. Since the income-originating effect is equal to that of Harrod's acceleration effect, the Domar equation (as distinct from his theory) taken as an income-originating equation rather than a multiplier equation amounts to Harrod's warranted-rate equation.

The upshot of this discussion is that there is no need (on the assumptions of the models) for the economy to generate a net multiplicand in order for it to advance steadily. Indeed, on the assumptions of Domar's model the generation of a multiplicand is inflationary. This view is consistent with the record of the post-war Western economies, which have shown strong tendencies toward inflation rather than toward deflation, as one may expect from Domar's theory.² If the tendency of capacity to outrun aggregate demand is not as important as shown by Domar's theory, the problem of instability may lie in the net savings ratio and its variability.

Note to Footnote 1, p. 444.

This note attempts to state the argument by dropping the assumption of simultaneity and using several periods. While this mode of exposition is not preferred by Harrod and Domar, logically the argument should hold also for period analysis. The assumptions regarding the saving ratio and the capital

¹ In terms of Harrod's system, ΔI financed by new money with the multiplier constitutes the (minimum absolute) extent of the upward deviation from the warranted rate.

² See his article in the *American Economic Review*, March 1957, and December 1948, p. 784.

coefficient, and the numerical examples are those of Domar as noted on p. 443.

Period 0: $Y_0 = 150.00$, $C_0 = 132.00$, $S_0 = I_0 = 18.00$

The saving ratio being 12%, a national income of 150.00 generates saving on the income side of 18.00, which is exactly equal to a total of 18.00 investment goods on the expenditure side of the national account. For this periodising it is assumed that the investment of each period *can only become actual capacity or exercise its influence as capacity* in the next period and that its productive use and operation can only be made in the next period.

Period 1: $Y_1 = 154.50$, $C_1 = 135.96$, $S_1 = I_1 = 18.54$

Income increases by 4.50, since the productive capacity (or sigma) is 25%. I_0 in Period 0 (now transformed into actual capacity and put into production operation) yields 4.50 amount of income (this is our income-originating affect). The saving ratio, assumed to be constant, applied to 4.50 yields 0.54 of saving, over and above 18.00, which is yielded by the 150.00 of income generated by the productive capacity already existing in Period 0. (The same result is obtained, of course, by multiplying 154.50 by 12%.) The additional consumption good of 3.96 is obtained as a residual, deducting the additional saving from 4.50. Savings on the income side are equal to investment goods on the expenditure side. But since investment in this period can only become actual capacity (or exercise its influence as capacity) in the next period, the economy ends the period in equilibrium. Note that there is no multiplier.

Period 2: $Y_2 = 159.14$, $C_2 = 140.04$, $S_2 = I_2 = 19.10$

As in Period 1, the increase in income is the result of putting into operation I_1 , assuming 25% as the productive capacity. This increase is divided on the expenditure side into 0.56 of investment increase and 4.08 of consumption increase, given the saving ratio. Thus, as long as the economy is operating at a rate of increase in income of G_w (3% in the example) there is no need for a multiplicand with its multiplier effect on income.

It may be argued that in Period 1, 0.54 of investment represents unintended inventory excesses. But whether 0.54 (or for that matter the entire 18.54) is unintended investment is not known until the next period, since there is no definition of intended inventory additions with increased income. As far as Period 1 is concerned, the investment goods of 18.54 has been produced. In Period 2 it may become evident that part or all of 18.54 is unintended (past) investment (in the form of excess capacity or excess inventory). But the essential point is, as long as the economy grows at the rate G_w in Period 2, no part of the *past* investment of 18.54 becomes unintended accumulation.

For the sake of the argument, let us assume that since 0.54 represents the

excess of I_1 over I_0 , it may be taken to constitute unintended inventory additions in Period 1. Even in this case, there is no need for 0.54 to constitute a multiplicand as argued by Domar. All that is needed is an injection of additional purchasing power to absorb the excess inventory. And whether this additional income is financed by government borrowing or by consumer borrowing, there is no multiplier effect attaching to such spending, since the goods purchased are already in existence (see p. 447 above). (I am indebted to Albert Rees for the foregoing method of illustration.)

Nor is it pertinent to argue that since there is no *prior* or initiating demand for output of the productive capacity of ΔI_1 in Period 2, excess capacity will be the result. There is no prior demand in Period 2 for not only the capacity of ΔI_1 but also for I_1 and also for any part of the older productive capacity.

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DOMAR AND DYNAMIC ECONOMICS

1. I HAVE had reasons for believing that perplexities of the kind set forth by Mr. Oshima in the foregoing article have been shared by other workers in the field, and his expression of them may be taken as a suitable opportunity for me to attempt a further elucidation of these and kindred matters.

2. I should like to take advantage of it to extend my fraternal greetings to Professor Domar. I have for long felt a lively appreciation of his work, and for my unseemly delay in expressing that on paper I can only plead that there was no occasion that struck me as suitable. He paid a generous tribute to my work more than seven years ago.¹ From the beginning he realised that the correct analysis of a growing economy required far-reaching changes in some of our traditional habits of thinking; and, independently of me,² he provided an equation, formally identical (see below) with one that I had previously furnished, which, I believe, must remain central to growth theory. Since the date A.D. of a formulation is of no importance, our names have rightly been jointly associated with this equation. I would claim that my first contribution to growth theory, as we both understand it, and as it is now, albeit slowly, coming to be understood by others, appeared in *Economica* in August 1934.³ In the years before that it seemed to be becoming clear that the traditional concepts of static economies would not serve to demonstrate decisively which of the rival monetary theories of Keynes and von Hayek was correct; yet from the practical point of view of the maxims of central banking it was vitally important to do so. The view was voiced that the matter might really be indeterminate—depending on “who got to the telephone first.” The position, so it seemed to me, was entirely transformed, if one assessed the rival theories by their application to a growing economy, and this I sought to explain in the afore-mentioned article. Two years later I published my *Trade Cycle*, which showed that the effects of the accelerator and the multiplier are intimately interlocked.⁴ The attempt to reduce the doctrines of that book to better order and to extricate what was most essential resulted in the formulation of a theory of which the afore-mentioned equation was a central feature.⁵

¹ *American Economic Review, Papers and Proceedings*, Vol. 42, No. 2 (May 1952), p. 481.

² Cf. his statement, *Essays in the Theory of Economic Growth*, p. 92, n. 11a.

³ “Expansion of Credit in an Advancing Community”. Reprinted as Essay 11 in *Economic Essays* (Macmillan, 1952).

⁴ For an early attempt to define dynamic theory, see also the last two pages of a paper which I read at a meeting of the Econometric Society in Oxford in 1936 (*Econometrica*, January 1937; Republished in *Economic Essays* as Essay 12).

⁵ “An Essay in Dynamic Theory,” *ECONOMIC JOURNAL*, March 1939. Essay 13 in *Economic Essays*.

3. This equation must be considered primarily as the expression of certain necessary relations between elements in a steadily growing economy. The validity of the equation is consistent with rival causal theories of growth, although not with all theories that have been suggested. It is to be stressed that, subject to two relatively minor reservations to be mentioned, Domar's equation is identical with mine. Confusion has been caused by the fact that he introduced it by considering the increase of production rendered possible by new investment; he designates the potential increase of output per unit of new investment by the symbol σ . I, on the other hand, make no explicit reference to this increased productivity, but, looking at the same fact from the opposite point of view, considered how many units of new investment are required, on the assumption that the new investment is properly utilized, to produce an extra unit of output; this I designated C_r .¹ My approach was quite reasonably regarded as giving expression to the acceleration principle, about which Domar appeared to say nothing. If we carefully note the fact that Domar's σ is valued on the basis that the new investment is properly utilised—it is in Domar's words the measure of a "potential"—and the fact that my C_r is valued on the basis that the new investment is no more nor less than that required to produce a growth of output, *i.e.*, that it is properly utilised, it is evident that

$$\sigma = \frac{1}{C_r}$$

As, subject to the two reservations to be mentioned, there are no other terms in the Domar equation that are not in mine, nor terms in my equation that are not in Domar's, it is clear that the equations are identical.

4. One of the reservations referred to in the last paragraph is as follows. Domar states his equation in its most familiar form as

$$\frac{\Delta I}{I} = \alpha \sigma$$

where σ is as stated above, α is the fraction of income saved and I investment. My equation makes no reference to ΔI or I , but runs

$$G_w = \left(\frac{\Delta Y}{Y} \right)_w = \frac{s}{C_r}$$

where Y stands for income, the suffix w indicates that the equation defines conditions for a steady rate of growth, or a moving equilibrium, and s is the same as Domar's α .

5. The formal identity of the two equations is preserved, owing to Domar's assumption that

$$\frac{\Delta I}{I} = \frac{\Delta Y}{Y}$$

I make no such assumption, and my equation is accordingly more general

¹ In *Towards a Dynamic Economics*; in the *Essay* it was designated simply C .

than his. In my equilibrium equation there is no reference, explicit or implicit, to ΔI or I . In my equation expressing certain necessary relations

in an actual rate of growth, *viz.*, $G = \frac{s}{C}$, there is an implicit reference to I ,

since C is defined as $\frac{I}{\Delta Y}$; but there is still no reference, explicit or implicit,

to ΔI . I may add here, in relation to Mr. Rose's interesting article,¹ of which I shall have something to say presently, where, in an endeavour to restate my doctrines, he refers to the amount of outstanding capital (and to its rates of increase), that I have never made any reference, explicit or implicit, to the amount of outstanding capital. These omissions were not accidental, but deliberate and the result of careful thought before I composed my *Essay*. These various values, of which I took no cognisance, will, of course, have to be brought in and play their part in a complete dynamic theory; the omissions were due to the desire to achieve great *generality* as befits a very fundamental proposition. Failure to notice that I do not refer at all to these values must betoken some lack of understanding of my thought. To extend the list of omissions yet further, be it noted that, although I sometimes referred loosely to capital goods in the *Essay*, the theory expounded in it draws no distinction between capital goods and consumer goods. (The first time that I brought the distinction in is on the last page of my *Supplement on Dynamic Theory*.²) This is very carefully stressed in the *Essay*. "Circulating and fixed capital are lumped together" (p. 18).³ "It may be well to emphasise at this point that no distinction is drawn in this theory between capital goods and consumption goods" (p. 18).⁴ In *Towards a Dynamic Economics* I also said explicitly: " C is the addition to capital, but need not consist exclusively, or even mostly, of capital goods. It is merely the accretion during the period of all goods . . ." (p. 80). Consequently the distinction that Mr. Rose draws between two variant interpretations of my theory is not tenable.⁵ He writes, "the primary source of instability [*viz.*, according to one interpretation] is in the effect of excess demand or supply on production decisions, not [as according to the other interpretation] in the effect of growing capital shortage or redundancy on investment decisions." As in my theory capital goods and consumer goods are lumped together, an "excess demand" is one and the same thing as "capital shortage," since "capital" includes stocks of consumer goods, and no distinction can be drawn between "production decisions" and "investment decisions." I revert to this matter below.

¹ "The Possibility of Warranted Growth," *Economic Journal*, June 1959.

² *Economic Essays*, Essay 14.

³ *Economic Essays*, p. 258.

⁴ *Op. cit.*, p. 259.

⁵ *Economic Journal*, June 1959, p. 317.

6. To return to Domar, I believe that he worked in terms of the increment of investment, because he was anxious to show that investment must itself grow if the economy is to grow. In order to bring this out clearly in his formulation, he made the restrictive, but not otherwise objectionable, assumption that the rate of growth of investment is the same as the rate of growth of income.

7. Mr. Oshima affirms that Professor Domar's equation explicitly includes the multiplier and implies that mine does not, or, if not that, at least that the multiplier does not play so large a role in it as in Domar's equation. I cannot agree to this, nor perceive any difference in this regard between myself and Domar. s in my equation, as stated above, is the reciprocal of the multiplier, and the magnitude of ΔY varies, *ceteris paribus*, directly with that of s . I do not see how one could make the multiplier play a greater role!

8. Mr. Oshima states that we both assume that the marginal propensity to save is equal to the average propensity to save. There can be no such assumption in my equation, as there is no reference, explicit or implicit, to the marginal propensity to save in it. (Another deliberate omission.) Domar tells us that he assumes equality of the marginal and average propensities, and this is presumably tied up with his assumption that $\Delta I/I = \Delta Y/Y$.

9. It may occasion surprise that the marginal propensity to save plays no part in my equation. I would say that such a surprise would be an *ignoratio elenchi*. I lay great stress on the importance of this, because from the beginning it has seemed to me that one of the great services that the formal definition of a dynamic branch of economics could render would be to prevent mistakes about dimensions.

Theorists of static economics, including Keynes, proceeded for long without perceiving (or anyhow without thinking it practically important) that a theory of static equilibrium is self-contradictory unless it assumes that savings = investment = zero. If we wish to postulate that saving is, and continues to be, positive, that entails that the economy is growing; we can no longer work in terms of a static equilibrium, but instead must use the concept of a steady rate of growth. If the fraction of income saved is constant, then, *ceteris paribus*, the steady rate of growth remains constant. My equation purported to specify what the steady rate of growth at a given point of time would be, given the values of the dynamic determinants.¹ I did not dare venture beyond that. It seemed prudent to tackle one thing at a time.

The fraction of income saved (average propensity to save) may rise temporarily in consequence of the trade cycle or owing to other disturbances; such changes are not relevant to the determination of the value of a steady growth rate. But it may be desirable to postulate a steady continuing rise in the fraction of income saved. Some time ago it would have been held

¹ Cf. "The analysis relates to a single point of time" "Essay in Dynamic Theory", p. 24; *Essays*, p. 267).

confidently that such a rise would be normal in a growing economy, but doubt has been cast on that. If a rising average propensity to save has to be postulated, this, *ceteris paribus*, entails an *accelerating* growth of income. In order to maintain growth, it would not merely be necessary, as Domar so strongly urges, for ΔI to be positive, but it would be necessary that $\Delta I_t > \Delta I_{t-1}$. I did not venture, nor, I believe, did Domar—at least in his central articles—on the economics of accelerating (or decelerating) growth.

10. So much for the identity of our equation, still subject to one further reservation shortly to be discussed. But its formulation is by no means the full tale of my thinking, which utilised it, nor indeed of that of Domar. When we go further in comparing our theories we find further similarities, as well as differences; I by no means wish to follow a precedent that may occur to the minds of some readers and to stress the difference between us. I will first point out a further similarity, to which I do not think that attention has been drawn. Those who have studied my theory know that the distinction between the "warranted" rate of growth, as expressed in my equation, and the "natural" rate of growth is central to it. I see a kinship between my distinction between these two rates and Domar's distinction between his concepts, σ and s .

Of my two growth rates it is the natural rate that is to be regarded as the welfare optimum. The warranted rate is the path on which the supply and demand for goods and services will remain in equilibrium, given the propensity to save; thus the economy could move along the warranted rate with great and growing "involuntary" unemployment in Keynes' sense. It is the natural rate that implies full employment. The actual rate of growth may be below both.

The natural rate is also the maximum sustainable rate, although the actual rate may go above it in phases of the trade cycle, when unemployed are being brought back into work, or when high marginal earnings, only worth paying in periods of high inflationary pressure, attract people to paid occupations who are not normally available for them. According to me, the difference between the gradients of the natural and warranted rates has to be considered in close relation to the instability principle, *i.e.*, the presence of centrifugal prices on either side of the warranted rate. (Mr. Rose's remarks on this are discussed below.) If the warranted rate is above the natural rate, the actual rate must be below the warranted rate for most of the time, and the centrifugal forces pull it further down, causing frequent periods of unemployment. (This is the dynamised version of the stagnation thesis.) If the natural rate is above the warranted rate, full employment may be achieved more frequently owing to the upward pull of the centrifugal forces, but only at the cost of inflation. The target of policy should be to bring the warranted rate as near as possible to the natural rate.¹

¹ It is to be noted that Mr. Kaldor holds that in the long run these will be brought together by a shift of income distribution.

In certain passages, however, Domar makes it plain that his rate of growth

$$r = \alpha\sigma$$

is for him a target rate and would sustain full implement. This caused me to look very closely at the concept σ . I believe that the rate specified by Domar's equation is really my "natural" rate; what is peculiar is that he should have defined it by what are (apparently) the same terms that I used to define my "warranted" rate, the very essence of which is that it may give trouble by being different from the natural rate. My warranted rate is simply the dynamised version of Keynes' excess or deficiency of aggregate effective demand in relation to what is required for full employment. The key to this puzzle is, I believe, to be found in Domar's concept of σ . Domar uses the symbol s for the ratio of the net value added by new projects of production to the capital invested in the projects. "But the productive capacity of the whole economy may increase by a smaller amount, because the operations of these new projects may involve a transfer of labour (and other factors) from other plants, whose productive capacity is therefore reduced."¹ σ makes allowance for this fact and is the net potential *social* average productivity of the new capital. σ is taken to be usually less than s . We need not bother with Domar's point that this may be so owing to a mis-direction of investment. Apart from that, the shortfall of σ compared with s is due to the fact that there is a "lack of balance between the propensity to save on the one hand, and the growth of labour, discovery of natural resources and technological progress on the other."² If only there was enough labour, etc., the economy could grow at $r = \alpha s$, which is really my "warranted" rate; but the lack of labour, etc., compresses potential growth to $r = \alpha\sigma$, my "natural" rate.

The result of this shortage of labour, etc., according to Domar, is that, fixed capital having been made to match existing savings, some pre-existing equipment has to be "junked"; too many stands having been created, some can just not be used; this junking process has a crucial and sinister role in Domar's scheme of things. It may be useful to relate the junking process to my equation.

Let us first suppose that the extinction of the amount of pre-existing equipment, as required by Domar, is due, not to competitive pressures, as postulated by him, but to periodic enemy action; this continuing enemy action would raise the amount of new capital required to sustain a given increase of output in the economy; it would raise the value of my C_r .

Thus, if, in the absence of enemy action, the warranted rate $G_w = \frac{s}{C_r}$ were above the natural rate, enemy action of the correct intensity would, by

¹ *Essays in the Theory of Economic Growth*, p. 73.

² *Op. cit.*, p. 79.

raising C_r , reduce G_w to the natural rate. Then the warranted rate would be equal to the natural rate and all could go forward smoothly.

But in fact there is no enemy action. Domar supposes that: (i) for a time enough investment is undertaken to absorb all saving at a full employment level, and that (ii) in consequence of more stands being created than there is labour to fill them, some of the pre-existing stands are junked. But the matter does not rest there; the junking process may well have a discouraging effect on the propensity to invest. If previously growth had pro-

ceeded in accordance with $\frac{\Delta I}{I} = \frac{\Delta Y}{Y} = \sigma\alpha$, the reduction in investment will

cause growth to fall below this level.

In my terminology what has happened in this case is that the fact of the warranted rate (that required to absorb saving) being above the natural rate (that required, having regard to technological progress, to maintain full employment) has caused the actual rate to fall below either. In Domar's terminology it is the excess of s over σ that, by leading to junking, causes the actual rate to fall below $s\alpha$ and $\sigma\alpha$. It is clear that the theories are very near together. The excess of s over σ is really another way of expressing what I have expressed by the excess of the warranted over the natural rate.

By parenthesis I should like to say that, in my judgment, Domar deals in rather too cavalier a way with junking. He recognises that it may be resisted by monopolists; he recognises that an excessive rate of junking may be socially undesirable (presumably by requiring too much mobility of labour); and he recognises the important point that it may reduce the future propensity to invest and thus cause a lapse from the path of steady growth. But otherwise he seems to look favourably, in a rather loose way, on junking; he seems to imply that, apart from the afore-mentioned drawbacks, the more junking the better. He does not seem to appreciate that a high rate of junking might be bad from a purely productivity point of view.

In fact, from the productivity point of view, there is an optimum rate of junking. Let us suppose, to simplify the argument, that the labour force is stationary and that no new natural resources are discovered. Progress would then consist: (i) in providing new-type stands, by investment, on which more old-type goods could be produced per unit of labour than before, and (ii) in the event of the demand for the old-type goods not being sufficiently elastic to absorb all the labour previously employed on them, in providing stands, by investment, for the released labour to produce new-type goods. New-type stands under (i) should, from a welfare point of view, and, if capitalist expectations and calculations are correct, will, be produced only if the cost of making old-type goods on the new-type stands, including interest and amortisation on them, is less than the cost of making those goods on the old stands free of interest and amortisation. The amount of investment required to produce all the new-type stands under (i), as justified by this

computation, depends on technical progress. The amount of investment required to produce stands for the production of new-type goods depends on technical progress and on the amount of labour released under (i). There is no *a priori* reason why the amount of investment required under (i) and (ii) together should be sufficient to absorb saving at full employment. I characterise such a state of affairs as one in which the warranted rate is above the natural rate. According to me this state of affairs causes the actual rate to fall below both natural and warranted rates; so it does according to Domar, but by a slightly different mechanism. If the labour force were increasing sufficiently, such a situation would not arise, since the surplus saving would be mopped up in providing extra stands for the new hands. The actual and warranted rates can stand above the natural rate for a period in the trade cycle when unemployed labour is being re-absorbed. But this must in due course come to an end.

Domar, on the other hand, appears to envisage that for a period new stands will be created (presumably in a spirit of gay optimism) to the full number permitted by the saving available, regardless of the question of their profitability; then, if there are too many stands altogether, some old ones will be junked. But suppose that, in order to mop up all savings by creating new stands, one makes some that are not more profitable than the old, on the basis of the computation mentioned above; then the old will not necessarily be junked; all stands may be under-employed. This will have an even more depressing effect than a high rate of junking on the future propensity to invest.

Domar writes:

"I am not prepared to say whether we already are or shall be soon faced with a serious difference between s and σ , though I doubt that was an important problem in the past, except perhaps for the short boom years. My own guess is that we shall be more concerned with the disparity between $\alpha\sigma$ and r [the actual rate of growth] that is with the failure of income to grow at the required rate."¹

(We are dealing here, of course, with an excessive propensity to save, not with the dilemma of the under-developed countries.) In my opinion there are not two separate phenomena here, but one phenomenon. In my view it is the excess of s over σ that causes r to fall below $\alpha\sigma$.

11. The tendency of an excess of the warranted rate over the natural rate to cause chronic depression depends on the instability principle; otherwise a community tending to excess saving might continue to bump along the ceiling of full employment. (I judge that Domar broadly endorses the instability principle.) Accordingly, this may be a suitable point at which to comment on Mr. Rose's criticism of that principle.² He takes me to task for

denying that my theory implies lags—and Domar makes a similar denial—holding that it does imply a lag in the adjustment of the amount of capital to the need for it. I do not deny that. Language is apt to be influenced by time and circumstance. What I was seeking to stress was that my trade-cycle theory—if it can be dignified with that name—was not of the same genus as those theories which econometricians were developing with great vigour at that time, and which, I doubt not, must be a part of the full and final theory of growth; these essentially depended on systematic lags between variables in a model which generated an oscillation, explosive or damped. There was danger that my instability principle might become confused with such theories, to which it really has no direct relation.

But I do deny that investment on any given day can be identical with the amount of investment that people want to have on that day, *unless* they are on the line of warranted growth. Only if people are on that line will *ex-post* investment in a given period be equal to the investment that is needed to bring the total amount of capital, fixed and circulating, up to no more nor less than the level that people deem they require to have in that period. If people find that on a given day, or, to be more realistic, in a given month, they have less capital, whether fixed or circulating, than they require in order most conveniently to execute turnover at its current level, there is nothing they can do to rectify that within the month. What they can do is to place extra orders, or, to step up their own production. The essence of the instability principle is that this will make capital still more short in the following period. The idea that they can have whatever capital, fixed and circulating, they require in a given short period, by taking some action in that period, is unacceptable. I conclude that Rose's criticism of the instability principle can be safely rejected.

12. Rose's thought appears to rest on a distinction between fixed and circulating capital, and it may be expedient to consider that. Let us suppose that, after a period of progress on the line of warranted growth, there is a divergence from it in an upward direction. This may be due to a chance aberration. Such an eventuality must be allowed as possible, since the economy consists of a great concourse of entrepreneurs, all working in conditions of uncertainty, and in particular industries, each of which may be growing at a different rate. It is essential to the idea of a static equilibrium, that, subject to the possibility of cobweb action, etc., it is stable, in the sense that chance divergences from it will be corrected in due course and a return to the previous positions secured. In the case of the dynamic equilibrium it is important to remember that a divergence may be due, not to a change in the behaviour of any entrepreneur, or of a group of entrepreneurs, but to a change in the values of the dynamic determinants themselves. These are, of course, always changing. This is analogous to the point, which Wicksell made so often, that the divergence of the market rate of interest from the natural rate, as defined by him, was less often due to a change of attitude on

¹ *Essays in the Theory of Economic Growth*, p. 81.

² *Economic Journal*, June 1959, pp. 319-21.

the part of the bankers in extending credit and more often to a change in the forces that determine the natural rate.

Let us suppose that, on the occasion of such a chance divergence, fixed capital is found to be short, taking the overall average of all entrepreneurs, but stocks of consumer goods not so. Let us suppose that extra orders for fixed capital are placed in consequence, but no extra orders for consumer goods. Rose is not wrong in thinking that this may redress the balance between capital goods and consumer goods on hand; it is possible that the orders for extra capital goods might be so extensive as to redress the shortage of capital goods; but, if this happens, it can only be by creating a shortage of circulating capital (consumer goods), which will be greater in value than the previous shortage of fixed capital. It is this fact that justified my lumping capital goods and consumer goods together for the purpose of formulating the dynamic theorem in its most general possible form.

13. While I hold that the instability theorem is safe, in the sense that the warranted rate of growth is surrounded by centrifugal forces and that a chance divergence from the warranted rate will be accentuated, I do not claim to have made any thorough-going analysis of the regions lying farther afield from the warranted rate.

There is no doubt that an explosive expansion will eventually be checked by the ceiling of full employment, at which growth has to be slowed down to the natural rate. Professor Hicks has brought that fact into play in his study. (Incidentally it is to be noted that the British boom of 1954/55 broke through the full-employment ceiling by temporarily bringing into employment people not normally seeking it.) It has been observed that booms sometimes terminate before the full-employment ceiling is reached. Domar has drawn attention to terminations of expansion in the United States in 1907, 1929 and 1937 considerably below the ceiling.¹ He draws a moral from them which I do not find acceptable.

He raises the question whether it is possible for the American economy to sustain an advance on the line of growth indicated by his equation. I would put that question in the form—is the warranted rate there above the natural rate? If it is, then, according to Domar's thinking, one would expect an actual advance to be checked by labour shortage, and, if it was not so checked on the three occasions cited, that suggests to his mind that the warranted rate was not unachievable on account of labour shortage and that the lapse from it was due to *other* reasons than its being impossibly high in relation to labour availability. In what follows I seek to examine that argument closely.

14. The possibility of a boom terminating before the full-employment ceiling is reached has naturally been present to my mind from the beginning, and I have given two reasons why that might happen. I propose to add a third reason, which may be even more important.

¹ *Op. cit.*, p. 117.

15. The first reason I gave was that a boom might in due course temporarily raise the propensity to save considerably above its normal level. It will be evident that a rise in the rate of growth above its warranted level (G_w) cannot in a moderate period of time be accompanied by an equiproportional rise in the fraction of income saved (s), even if, for all values of Y above normal, the whole of ΔY was saved. Accordingly, capital would remain in short supply. But after a time a strong rise in s (due to profit inflation) could overtake the rise in G ; this could bring the boom to an end before the ceiling of full employment was reached. I have suggested that this is the reason why a boom accompanied by an unrestrained price inflation might peter out more quickly than one in which excess demand was reflected mainly in delayed delivery. In the former kind the profit inflation and the temporary rise in s are greater. I incline to think, however, that variation in s may be more relevant in explaining why a slump may come to an end— s may have a great decline in a severe slump—than why a boom may do so.

16. My second, and probably more important, reason was that the actual rate of growth begins to decline well before the ceiling of full employment is reached, owing to decreasing mobility of labour, as unemployment declines, and the appearance of bottlenecks that are due to shortages of specific types of capital. Growth, having moved freely forward in the early stages of boom at a rate well above the warranted rate, may begin to decline and thus fall below the warranted rate, even when there is still a sizeable reserve of unemployed labour; thus the boom is brought to an end and a recession rendered inevitable.

17. In order to explain the third possible reason for this phenomenon it is needful to take a wider view. In my original *Essay* I distinguished between those orders governed by the acceleration principle, those that are dependent rather on the current level of income (designated by me as k , where k is a fraction of income) and those unrelated to either (designated K). Professor Hicks has developed the distinction between the first and last mentioned, calling the former "induced" and the latter "autonomous" investment. For the instability principle to be valid, all that is required is that *some* current orders should be dependent on the acceleration principle. In the more refined account of the instability principle given in my *Supplement*, I have stated the assumption required as follows: "The more general assumption is simply that the entrepreneur will order more, the lower the ratio of his stocks and equipment to current put-through, and this I take to be safe."¹ This does not imply that there are no other forces governing his current level of orders.

I have suggested that we must not suppose a hard-and-fast line between induced and autonomous investment. Orders that, in relation to a short and mild slump, should be regarded as belonging to the autonomous (K) category, may come up for review, if the slump is severe and prolonged. This is an

¹ *Economic Essays*, p. 283.

aspect of the vicious-circle process of a depression; the more the types of investment are that are reconsidered for downward revision in consequence of a recession, the stronger the forces of recession will be; and the stronger the recession, the more the types of investment are that will be considered for downward revision. The opposite process may occur in the boom phase; a prolonged period of expansion may make people take a buoyant view and set more long-range projects going.

But in the boom there is another aspect. People may become more buoyant, but they may also become more sceptical. In the early phase of a boom one may, and indeed must, suppose that their long-range views are unaffected by it, so that the felt shortage of capital, fixed and/or circulating, which is caused by the boom, induces them to increase orders. This increase of orders cannot mitigate, but must, by itself, increase the felt shortage of capital, fixed and/or circulating. But as the boom proceeds, their view of the longer future may come in as a counteracting influence. They may begin to ask themselves whether demand can continue to increase at the rate at which it has recently been doing. Even if the experienced shortage of capital continues unabated, they may begin to hesitate about placing increased orders, anyhow for fixed capital, on the ground that calls upon its utilisation are not likely to continue to increase at their existing and recent rate. In the case of fixed capital at least, they must have regard to its utilisation over its expected length of life. These hesitations caused by attention to the longer future may bring the boom to an end before full employment is reached.

18. I will first deal with the relevance of this point to Domar's doubts. Consider the hypothesis that lack of full employment in the United States before the War was due to the warranted rate being above the natural rate, or in other words, that the rate of growth required by Domar's formula just *could* not be achieved permanently because labour (along with technical progress) was not growing fast enough to render that possible. Domar rules out this explanation on the ground that three booms broke before any labour shortage was apparent. But one must also look at the prospective demand side. I take the following figures merely to illustrate the argument. Let us suppose that in the United States α s was for a longish period 4% and that this rate of growth, or a higher one, was achieved in boom periods. Let us further suppose (*my* hypothesis, on which the premature termination of booms, in Domar's view, throws doubts) that the maximum sustainable rate of progress in the United States over this longish period, having regard to technical progress and population increase, was, say, 3%. During the boom period we suppose that fixed capital was increased at 4% or more with confidence, since the revival, accompanied by re-employment of labour, was bringing demand up at that rate. This rate of 4% or more is, of course, an average; entrepreneurs in expanding industries may have based themselves on a much higher rate of prospective increase. Let us call the boom-period

rate of increase x , where x has a different value in each industry. After a period of expansion canny entrepreneurs may have said to themselves that they could not believe that the x rate of expansion could continue indefinitely; and these would accordingly slow down their rate of increase of fixed capital, although there was still a shortage of it, and thus bring the boom to an end. And why should the entrepreneurs think that the x rate of expansion could not continue? Because the 4% (or higher) rate of expansion in the economy as a whole could not continue. Population increase and technical progress were just not sufficient to allow aggregate output to expand at that rate indefinitely, and therefore aggregate effective demand could not expand at that rate indefinitely. Thus the canny entrepreneurs would have been proved right in their scepticism. But if the increase of output allowed by technical progress and population increase had been 4% p.a., then the entrepreneurs would have been wrong in their scepticism. Thus the fundamental reason why the entrepreneurs were right to be sceptical was that the maximum increase in demand that could possibly be sustained indefinitely was 3% in the aggregate, viz., below the x % recently current in each industry, and below the aggregate 4% (recently achieved or exceeded), as represented by Domar's α s. This perfectly correct attitude of scepticism could serve to bring the boom to an end before full employment was reached. But if the natural rate had been as high as 4%, viz., not less than the warranted rate, the entrepreneurs' scepticism would have been groundless and the expansion could have continued. The natural rate being below the warranted rate (taken to be 4%) would thus have served to push the actual rate below either, *before* any labour shortage had begun to be felt; the low level of the natural rate operated through the fact that the insufficient rate of increase of demand sustainable indefinitely cast its shadow backwards, and reasonably and rightly deterred entrepreneurs from ordering as much as would have been needed to keep the increase going forward at its sustainable rate. Accordingly, I hold that the termination of booms before labour shortage began to be felt is not a reason for denying that it was the warranted rate being above the natural rate that prevented either being achieved for more than ephemeral boom periods. The insufficient expansion of the labour force (or of technical progress) checked expansion because it meant that a correct prognosis of the maximum possible increase of demand was not sufficient to justify sufficient ordering in the present. Be it noted that this check would have operated, even if the entrepreneurs had been thoroughly optimistic, not assuming that any recession was coming, but assuming only that demand would not continue indefinitely to grow at more than the maximum possible sustainable rate.

It is fair to observe that Domar anticipates this point. "It is possible to argue that . . . such a situation . . . was foreseen by the entrepreneurs who reduced their commitments in advance." He adds, "but without sufficient evidence, almost anything could just as well be said." It is always

desirable to have factual evidence; but it does not seem reasonable to dismiss in so cavalier a way the hypothesis that the entrepreneurs did what they ought to have done on a correct prognosis.

19. This may be the appropriate point to refer to a criticism by Professor Baumol.¹ He suggests that the instability principle would not operate, if only entrepreneurs kept a firm grasp of what was likely to happen in the long period and based their policy on that. "It is even possible that entrepreneurs will generally believe in the 'normalcy' of the economy and so will regard any case of overproduction as a temporary phenomenon soon likely to disappear." It is evident that my preceding paragraph makes some concession to Professor Baumol's point of view (which presumably applies to underproduction also). I have suggested that, despite a continuing experienced shortage of capital, fixed and/or circulating, entrepreneurs may abate their rate of ordering, because they regard the current tempo of advance of demand for their products as abnormal and not capable of being sustained indefinitely. But I suggest that it would be pushing this argument much too far to regard it as obviating the instability principle. For it to do that, one would have to suppose entrepreneurs having fixed and rigid views about the future, on which they based their current action, and not being influenced at all by current experience. This is rendered the more unlikely when we consider that each industry, and each sector of an industry, has its own problems and uncertainties, its own current rate of expansion and its own projected future rate of expansion, which may be different. In view of this, it is, I would even say, absurd to suppose that each firm will go forward on the basis of its own view about the future and disregard current experience. We know that an upward deviation in the rate of growth will cause a shortage of fixed and/or circulating capital in relation to current requirements on the overall average. The incidence of the shortage will probably be uneven as between industries and firms. I submit that we must suppose that, on the overall average, firms, faced by a novel shortage, will place more orders than they otherwise would. And this will cause a further deviation from the path of warranted growth. In fine, the instability principle will operate.

It would be most inappropriate to make any comments on the far-reaching views set out by Mrs. Robinson² and Mr. Kaldor³ at the end of what is primarily a tidying-up article; the omission to do so must therefore not be taken to imply lack of regard for them. If my views come to constitute a limb of those "Principles of Dynamic Economics," which we still await, then it is important that their scope and limitations should be understood with precision.

Christ Church,
Oxford.

R. F. HARROD

¹ *Economic Dynamics*, pp. 53-4.

² *The Accumulation of Capital* (Macmillan, 1957).

³ "A Model of Economic Growth," *ECONOMIC JOURNAL*, December 1957.

SOME ECONOMETRICS OF THE DETERMINATION OF ABSOLUTE PRICES AND WAGES

INTRODUCTION

MUCH of the discussion of the current problem of inflation in the Western Capitalist world is necessarily strongly coloured by awareness of the course of the leading statistical series on wages, prices, productivity, profits and unemployment during the last decade, when the general inflationary situation has caused many series to move together. It is rather difficult, but of the utmost importance, to try to unravel the chains of relationship among the several variables in order to delineate *structural* relations that have a high degree of *autonomy*.

The annual figures on wage-rates over the past ten years are correlated with those on productivity, but they are as highly correlated with employment, unemployment or the price level. Many of the apparent correlations are derived relations obtained by combining more basic structural relations. Many such derived relations can be obtained from a given structure, and one must take great care in the conclusions drawn from empirical correlations. The problem is especially serious in this decade of annual observations when reversals of the broad trend give only delicate statistical movements that are not easily discerned with the use of blunt tools.

The present paper arises out of work being done by the authors on the construction of an econometric model of the United Kingdom economy as a whole.¹ This work involves the fitting together of autonomous structural components, from which a partial sub-system of interest on the inter-relationship between wages and prices emerges. The findings we have obtained on this part of the model seem to be relevant in assisting an understanding and interpretation of the post-war inflation. We are thus moved to put forward some of our results at this stage. The entire investigation is based on quarterly series covering a wide variety of economic activities in the United Kingdom. It is hoped that by refining the sample data to this unit of observation we have a more sensitive and adequate body of information for discriminating among alternative explanations of the phenomena at hand. We cannot claim, however, to have found *the single* explanation of the inflationary process. That is hardly ever possible in our subject, and statistical analysis alone on the very best samples of economic data have never been able to prove that a particular relationship or explanation dominates the field to the exclusion of all others. We do, however, feel that our results are reasonable and carefully backed up by *a priori* considerations of autonomous

¹ Our colleagues, A. Hazlewood and P. Vandome, of the Institute of Statistics, Oxford, have collaborated on the larger study, of which this paper is a part.

patterns of behaviour. We can also bring evidence to bear against *some* alternative hypotheses.

In the discussion of post-war inflation, a number of economists have taken strong sides on the question of whether we have *demand* or *cost* inflation. Unfortunately, the great lesson of Walras has been forgotten in this debate. We have tried to keep a picture of the inter-related network of equations in the economy as a whole in front of us constantly, in building each component of the United Kingdom model, and feel that this may be advantageous in the interpretation of wage and price movements. In our system, and by our method of approach, it is possible for both demand and cost elements to show their respective strengths.

Some observers would base their characterisation of the type of inflation we have been having on the movements in such statistical series as the spread between wage-rates and earnings, others on the share of profits in national income, others on the lead-lag relationship between price and wage indexes, and others on the relationship between productivity and earnings. We select no key series for our study of this problem but try to inter-relate several series as part of a comprehensive model.

A MODEL OF THE WAGE-PRICE MECHANISM

The central equation of the model dealing with the determination of wages is based on the purely conventional assumption that market prices move in order to wipe out excess demand or supply. Economic theory has given us no reason to say whether dynamic movements of *relative* or *absolute* prices are associated with market clearing. We assume that money wage-rates move over time in response to excess supply or demand in the labour market. This latter concept is given a statistical representation in the figures for unemployment, but such measures as unfilled vacancies may also be used.¹

Actual bargaining over wage-rates takes place in terms of monetary units, but this is not to say that a "money illusion" is involved. Both sides are only too aware of the "real" nature of economic affairs. Movements in the cost of living are prominent facts at the bargaining table. Instead of saying that dynamic movements in real wage-rates are functionally related to unemployment, we take the somewhat more general and more realistic view that the time rate of change in money wage-rates is a function of the level of unemployment and the time rate of change of the price level. A further reason for expressing the relationship in this way is that a time lag may exist between price and wage movements. The nature, extent and magnitude of this time lag is of key significance in the interpretation of inflation as being of either the demand or cost variety. With ten annual observations it is difficult to say anything very definite about the lag, but

¹ Vacancy statistics are used by J. C. R. Dow and L. A. Dicks-Mireaux, "The Excess Demand for Labour," *Oxford Economic Papers*, Vol. 10, 1958, pp. 1-33.

with quarterly data we may eventually hope to make a stronger inference, although not necessarily from the samples at hand.

Two other factors are frequently mentioned in connection with wage bargaining—profits and productivity. To some extent price movements and the level of unemployment already indicate the profit position, but in one of our experimental computations we explicitly introduce a profit variable into the relationship.

Productivity, we believe, is more properly entered at a different stage of the process, but we do not rule out the possibility that it can be a variable in its own right in the bargaining equation and similarly include it in an experimental computation.

If workers try to keep pace with the cost of living and find their bargaining power sensitive to the state of excess supply or demand in the labour market, we would be inclined to call the resulting movement in wages an expression of demand inflation. They simply follow the ordinary laws of the market. If, however, workers are able to push wages ahead of price movements or the level dictated by the state of the labour market, we have evidence of cost inflation spurred by deliberate trade-union action. It is often said that trade unions co-operated with the post-war Labour Government and moderated their demands in accordance with a tacit understanding about general economic policy. It is further claimed that the trade unions became more aggressive or militant in pressing their claims after 1952 under the Conservative Government. To the extent to which trade unions can turn off or turn on steam in forcing the outcome of wage bargains in predetermined fashion we have cost inflation. In our equation we shall introduce a constructed statistical variable (sometimes called a "dummy" variable) which assumes the value of zero before 1952 and unity thereafter, to take into account this dual character of the period under review. In this respect our model has some flexibility in describing events during the period 1948-57, but otherwise our system has the same structural coefficients at all times. We treat the whole group of quarterly periods since 1948 as homogeneous as far as the mechanism of inflation is concerned.

Another way of expressing the power of trade unions over and above their market-induced strength is to note that they force wage movements on an irreversible path upwards. Either a steady trend growth or some type of "ratchet" variable could bring out this aspect of union strength on the side of cost-induced inflation. Our results do not have this irreversibility, and perhaps they are not typical of all business-cycle movements in this respect. In the framework of the post-war economy there are no adequate data for testing this hypothesis. The pre-war data are unsatisfactory because many basic series are not available for that period. The power of trade unions is believed to have changed greatly since the War, and the structure of the economy as a whole has similarly changed. For these reasons, we are not pursuing the aspect of irreversibility in the present model.

We formulate a wage-rate equation, in which changes in wage-rates appear as a function of previous price changes, and of the level of excess demand for labour as indicated by our proxy measure, the level of unemployment.

The particular form that we have given to this equation originates from a suggestion made to us by L. A. Dicks-Mireaux, who has pointed out the implications of discontinuity in wage bargaining. Broadly speaking, workers in any particular sector of the economy bargain for changes in wage-rates once a year. The extent of the change will depend on the current level of excess demand, but also on the change in the price level that has occurred since the workers in that particular sector last had an increase in wage-rates, *i.e.*, the price change in the previous twelve months, assuming that workers receive wage increases at roughly the same time in each year. This assumption is, of course, not strictly true, since in some years the roles of leader and follower in wage demands may have been altered. Nevertheless, it is close to the now familiar concept of the annual round of wage-rate increases. It follows from this argument that wage-rate increases in any one quarter for workers having a change in that quarter will depend on the level of excess demand, and the price change over the previous twelve months.

For workers not subject to change in wage-rates during that quarter, their wage-rates will be related to excess demand and prices of previous quarters, *i.e.*, the time of their last negotiations. If we follow through this scheme of discrete adjustment we find that the *average* wage change for all workers of any quarter over the same quarter of the preceding year is a function of a weighted average of past levels of excess demand and past changes in the price level. The weights in the average will depend upon the number of workers coming up for negotiation in each quarter, but we assume an even distribution of numbers among the quarters.

The equation to be estimated takes the form,

$$(1) \quad (w_r)_t - (w_r)_{t-4} = \alpha_1 \left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right) + \\ \alpha_2 \left(\frac{p_t - p_{t-4} + p_{t-1} - p_{t-5} + p_{t-2} - p_{t-6} + p_{t-3} - p_{t-7}}{4} \right) + \\ \alpha_3 F_t + \alpha + \alpha_{s1} Q_{1t} + \alpha_{s2} Q_{2t} + \alpha_{s3} Q_{3t} + u_{1t}$$

w_r = quarterly average of index of weekly wage-rates;

U = index of unemployment;

p = quarterly average of index of consumers price level;

F = political factor, zero in quarters before 1952 and unity thereafter;

Q_i = seasonal factor, unity in the i th quarter and zero in any other quarter;

u_1 = disturbance.

It may be observed at once, from the form of (1), that the institutional character of wage bargaining has led to the formulation of an equation, in

which there is, on the average, a six-month lag between wage and price changes. But this lag does not carry any implications about what most people have in mind on the existence of demand or cost inflation. Nevertheless, it does introduce a lag of rates behind prices.

The political factor, F_t , needs no explanation beyond that already given. For every equation of the entire United Kingdom model the data used are completely unadjusted for seasonal variation. In each equation, however, we include explicit seasonal variables. Inspection of graphs has led us to posit a linear additive seasonal component throughout. In this equation with four quarter changes and moving averages there is little, if any, scope for seasonal influence. Indeed, the empirical results indicate this below, but every equation was treated for seasonal variation on the same basis, and we include the Q_{it} variables here for the sake of uniformity.

Equation (1) is a key equation in the United Kingdom model because it displaces, in a sense, the money balance equation for the absolute determination of prices and wages. It is difficult, again following the Walrasian idea, to say what determines what in a truly inter-related system, but the main function of equation (1) is to complete the system in respect of the determination of absolute wages or prices, while the cash-balance equation has the main function of completing the system in respect of the determination of the interest rate.¹ The state of the labour market displaces the state of the money market in determining the course of absolute prices or wages. "It is hardly an exaggeration to say that instead of being on a Gold Standard we are now on a Labour Standard."²

To be as realistic as possible we have carefully formulated the bargaining equation of our model in terms of a wage-rate variable. Earnings, however, not rates, are the important cost variables to an entrepreneur. We therefore seek to establish a relation between the two concepts of wage payment. The spread between earnings and rates has, as noted above, become an important statistical indicator in the view of many people who are trying to diagnose the inflationary process. Our measure of wage-rates used in this model is an index of weekly wages paid for a full-time standard work period. It is averaged to quarters for our purposes. The earnings variable is similarly an index of total wages paid for a week's work regardless whether that week's hours consist of short time, full time or overtime. It includes bonus or other premium payments as well. In some analyses an attempt is made to measure hourly earnings, in which case weekly earnings would have to be adjusted to a uniform number of hours worked, but we make no such adjustment.

¹ These ideas are developed in connection with similar American models in L. R. Klein, "The Empirical Foundations of Keynesian Economics," *Post Keynesian Economics*, ed. by K. Kurihara (New Brunswick: Rutgers Univ. Press, 1954), and L. R. Klein and A. S. Goldberger, *An Econometric Model of the United States: 1929-1952* (Amsterdam: North Holland Publishing Co., 1955).

² J. R. Hicks, "Economic Foundations of Wage Policy," *ECONOMIC JOURNAL*, Vol. LXV, September 1955, p. 391.

If workers are paid, wholly or partially, on a piece basis, their earnings will rise with rising productivity; therefore the excess of the earnings index over the rate index is made to depend on the level of productivity. For those workers geared to a time rate of pay, their earnings will vary with hours worked. As they work more or fewer hours they will earn more or less per week. In addition, overtime work will provide a premium rate of pay. We make the spread between the two wage indexes a function of hours worked as well as of productivity. Hours worked are one of the most sensitive indicators of economic activity; therefore it is not necessary to introduce another variable into the relationship to indicate the level of demand pressure. The hours contribution to the earnings-rate spread seems to be a measure of demand pull, while the productivity contribution is perhaps more mixed in its cost and demand elements. Some writers, however, regard any positive movement in the spread as demand inflation. Our second equation takes the form

$$(2) \quad w_t - w_{rt} = \beta_1 h_t + \beta_2 (P/hE_p)_t + \beta + \beta_{s1} Q_{1t} + \beta_{s2} Q_{2t} + \beta_{s3} Q_{3t} + u_{2t}$$

w_e = quarterly average of index of weekly earnings;

h = quarterly average of index of hours worked per week;

(P/hE_p) = quarterly average of index of productivity (industrial production index divided by index of weekly man hours in corresponding production industries).

In contrast with the previous equation, there is a significant seasonal factor in the excess of earnings over rates, and the seasonal variables introduced in this equation are more important than in the previous one.

Hours are introduced as an explanatory variable in equation (2); hence we include an equation in our sub-system to show how hours worked are related to some of the other variables included in the same sub-system. Short of a presentation of our entire model consisting of more than a score of equations, we cannot show how industrial production is related to other variables of the system. For the purposes of the present paper we leave the system open at the production end. It is a partial and not a complete analysis.

When output is high hours worked are high. Hours are fairly closely tied to the general business cycle. They are also related to the pressure on facilities. When activity is slack, short time is introduced, and when labour is very scarce overtime comes back into operation. Our third equation thus relates the hours index to indexes of industrial production and of unemployment.

$$(3) \quad h_t = \gamma_1 P_t + \gamma_2 \left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right) + \gamma + \gamma_{1s} Q_{1t} + \gamma_{2s} Q_{2t} + \gamma_{3s} Q_{3t} + u_{3t}$$

This equation shows that the hours variable in (2), accounting partially for

the earnings-rate spread, is, in turn, related to the level of activity measured both by P_t and the average of unemployment.

The completion of our sub-system involves the construction of an equation giving a direct link between wages (earnings) and prices. It is, in effect, an equation of mark-up over prime costs. In the debates on the theory of the firm the adherents of full cost pricing as a pattern of behaviour would suggest an equation roughly similar to that which we propose below. An alternative approach, which we have not followed in this particular model, would be to construct equations proposed by the adherents of marginal analysis, *i.e.*, marginal-cost or marginal-productivity equations. Such equations, transformed into equations of *labour's share* on the assumption of an exponential production function, have been estimated in the econometric models of the American economy.¹ The equation of entrepreneurial behaviour introduced here is similar to that used in the model of the Dutch economy.² In our mark-up equation the index of price of final output is made a function of the earnings index, the import price index, and an index of indirect tax rates.

$$(4) \quad p_t = \delta_1 (w_e)_t + \delta_2 (p_i)_{t-2} + \delta_3 T_t + \delta + \delta_{1s} Q_{1t} + \delta_{2s} Q_{2t} + \delta_{3s} Q_{3t} + u_{4t}$$

p_i = index of import prices;

T = index of ratio of indirect taxes less subsidies to consumer expenditures.

It is assumed that import prices affect final output prices with a time lag of 2 quarters, but a somewhat shorter lag, a somewhat larger lag or even no lag at all would not have greatly altered our statistical results or the conclusions based on them.

It may be thought that the mark-up over costs varies cyclically, and we have looked into this possibility by adding a productivity variable to the equation in an experimental calculation. Increasing productivity enables producers to moderate mark-ups over costs and yet retain levels of profits. We should expect to find a negative coefficient of a linear productivity variable if one is added to an equation like (4). Other cost variables may be considered as well, but we have restricted the analysis to prime costs covering wages, imports and taxes. To the extent to which fluctuations in import prices contribute to final price fluctuations we have a form of cost inflation (or deflation) but not one that is attributable in any sense to trade-union power. In this equation and in our entire United Kingdom model import prices are treated as exogenous. While we attempt to close the system at many points in the trading accounts, we make no attempt to do so with respect to import prices.

¹ L. R. Klein and A. S. Goldberger, *op. cit.*

² See *Scope and Methods of the Central Planning Bureau* (The Hague: Central Planning Bureau, 1956), pp. 70-3.

STATISTICAL ESTIMATES OF THE MODEL

The four equations outlined in the preceding section have all been estimated from quarterly data of the period since 1948. The method of estimation used is known, in technical terms, as limited information maximum likelihood, a variant of the well-known method of maximum likelihood. This method has the desirable property of taking into account the structure of an interdependent model when estimating any particular part of it. For this reason it avoids the problems of bias (large sample theory) prevalent in application of what might be called single-equation methods such as the familiar methods of multiple-correlation theory applied to any particular equation without reference to the fact that it is imbedded in a larger inter-related system. Our method also has desirable properties in forecasting for the economy as a whole and is being used to estimate the entire United Kingdom model. The present estimates are a by-product of this effort.¹

The estimated set of four structural equations in our partial model with sampling errors noted in parentheses is

$$\begin{aligned}
 (1e) \quad (w_r)_t - (w_r)_{t-4} &= \frac{-0.091}{(0.013)} \left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right) + \\
 &\quad \frac{0.854}{(0.092)} \left(\frac{p_t - p_{t-4} + p_{t-1} - p_{t-5} + p_{t-2} - p_{t-6} + p_{t-3} - p_{t-7}}{4} \right) + \\
 &\quad \frac{2.90}{(0.40)} F_t + \frac{10.26}{(1.41)} + \frac{0.10}{(0.57)} Q_{1t} + \frac{0.30}{(0.57)} Q_{2t} + \frac{0.19}{(0.57)} Q_{3t} \\
 (2e) \quad (w_e)_t - (w_r)_t &= \frac{2.06}{(0.54)} h_t + \frac{0.625}{(0.150)} (P/hE_p)_t - \frac{272.72}{(45.80)} + \\
 &\quad \frac{0.001}{(0.96)} Q_{1t} + \frac{1.58}{(0.98)} Q_{2t} + \frac{5.54}{(1.36)} Q_{3t} \\
 (3e) \quad h_t &= \frac{0.089}{(0.007)} P_t + \frac{0.0068}{(0.005)} \left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right) + \\
 &\quad \frac{90.20}{(1.13)} - \frac{0.11}{(0.20)} Q_{1t} + \frac{0.15}{(0.20)} Q_{2t} + \frac{0.83}{(0.22)} Q_{3t} \\
 (4e) \quad p_t &= \frac{0.421}{(0.013)} (w_e)_t + \frac{0.216}{(0.030)} (p_t)_{t-2} + \frac{0.013}{(0.161)} T_t + \frac{35.65}{(13.03)} + \\
 &\quad \frac{0.94}{(0.50)} Q_{1t} + \frac{2.06}{(0.50)} Q_{2t} + \frac{1.18}{(0.50)} Q_{3t}
 \end{aligned}$$

All variables are measured as index numbers which average to 100 in the base period calendar year, 1948, except Q_{it} and F_t . These are

¹ Substantive economic content and not econometric method occupies our main interest in this paper; therefore we shall not discuss the details of the estimation procedure. The method is explained and described in L. R. Klein, *A Textbook of Econometrics* (Evanston: Row Peterson and Co., 1953). An example of its application to similar problems is given in L. R. Klein and A. S. Goldberger, *op. cit.*

constructed variables which take on the values of zero or unity in specified quarters.

The estimate of equation (1) gives a close correspondence between actual and computed four-quarter rates of change in wage-rates. The degree of correlation, adjusted for degrees of freedom used up in estimation is 0.93 in our sample of 36 quarterly observations over the period 1948-56.¹ The residual variation appears to be slightly auto-correlated, thus indicating that we have not isolated all the non-random factors. We measure auto-correlation in residuals by the statistic $\frac{\delta^2}{s^2}$, known as the ratio of the mean square successive difference to the variance.² For values above 1.4 in samples of our size we accept the hypothesis of non-auto-correlation at the 5% level of significance. In the present case our value is estimated at 1.02.

By another test using a statistic which is smaller than $\frac{\delta^2}{s^2}$ by the factor $\frac{N-1}{N}$ (N = sample size), we find an indecisive answer to the question whether or not there is significant auto-correlation.³

Our estimated equation suggests three positive conclusions: (1) Excess demand for labour as represented by a moving average of unemployment has been an important factor over the sample period. (2) Wage-rate adjustment to price changes has in effect roughly compensated for the effects of price increases between the times at which increases in wage-rates have been obtained. (3) The behaviour of wage-rate changes before 1952 is markedly different from that after 1952. The latter point is illustrated by the magnitude and statistical significance of the coefficient of F_t in (1e). On the average, wage-rates moved up over a four-quarter interval by 2.90 more index points after 1952 than before, *ceteris paribus*. This might be taken as evidence of the strength of cost inflation.

Since the change in wage-rates is measured over a period of four quarters, seasonal factors are effectively repressed. This is indicated by the small sizes and comparatively large sampling errors of the coefficients of the Q_{it} in (1e).

The main influence of demand inflation should be shown through the coefficient of $\left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right)$, the index of unemployment averaged over the past four quarters. As would be expected from *a priori*

¹ Although this is not an ordinary multiple-correlation estimate, we can compute an overall correlation coefficient defined as $(1 - S_u^2/S_v^2)^{1/2}$, where S_u^2 is the estimated variance of disturbances (determined from the variance of residuals) and S_v^2 is the variance of the normalised variable in the equation (the variable with a unit coefficient).

² B. I. Hart and J. von Neumann, "Tabulation of the Probabilities for the Ratio of the Mean Square Successive Difference to the Variance," *Annals of Mathematical Statistics*, Vol. XIII, 1942, pp. 207-14.

³ J. Durbin and G. S. Watson, "Testing for Serial Correlation in Least Squares Regression, II," *Biometrika*, Vol. 38, 1951, pp. 159-78.

considerations, this coefficient is negative and has a relatively small sampling error. In earlier work we estimated (1e) in the form.

$$(1e') \quad (w_r)_t - (w_r)_{t-4} = \frac{-0.021}{(0.017)} \left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right) + \\ \frac{0.975}{(0.115)} (p_t - p_{t-4}) + \frac{3.88}{(0.48)} F_t + \frac{2.40}{(2.05)} - \frac{0.31}{(0.67)} Q_{1t} - \frac{0.25}{(0.67)} Q_{2t} - \frac{0.030}{(0.67)} Q_{3t}$$

It will be noted that the principal difference between (1e') and (1e) is the size and significance of the coefficient of the moving average of unemployment. There seems a pronounced tendency for this coefficient to increase in absolute size and significance as we increase the lag in the price change. The estimate of (1e') was obtained using the method of limited information. We experimented, however, with an alternative form of (1e') using $(p_{t-1} - p_{t-5})$ as our price variable, and making use of the method of least squares. As we have observed, least-squares estimates of an equation of the form (1e) or (1e') will be biased estimates, but though they are biased, they probably give a good estimate of the change to be expected from introducing a new variable.¹ Using $(p_{t-1} - p_{t-5})$ as our price variable, the coefficient of $\left(\frac{U_t + U_{t-1} + U_{t-2} + U_{t-3}}{4} \right)$ jumped to 0.06, and was significant as opposed to the estimate in (1e'), which is only just greater than its sampling error. Least-squares estimates of (1e') gave us a coefficient of 0.039, which suggests that 0.06 represents an upward biased estimate; nevertheless, the added importance given to the influence of demand as a result of the introduction of a price lag appears to be firmly established.

This result indicates that the magnitude of the coefficient of the moving average of unemployment in (1e) is not unexpected, since we have, in effect, an average lag of price changes of six months. The actual size of the coefficient in (1e) indicates that if the average price change were zero, and $F_t = 1$ for all t , the index of the moving average of unemployment would have to be about 150 before the change in wage-rates would become negative. The rise in the coefficient of $\left(\frac{U_{t-1} + U_{t-2} + U_{t-3} + U_{t-4}}{4} \right)$ in (1e) as compared to (1e') has been partially offset by the increase in the constant term from 2.40 to 10.26, which represents the trend increase in wage-rates.

This raises the question to what extent equation (1e') can be considered reversible. It would undoubtedly be felt by some that even if the price level could be stabilised over the requisite period, a rise in the index of the moving average of unemployment to 150, representing an absolute level of unemployment of about 350,000, would not be sufficient to result in an

¹ It may be observed that the least-squares estimates of the coefficients in (1e) are hardly distinguishable from those obtained using a consistent method of estimation. The coefficient of the moving average of unemployment is 0.089 as against 0.091, and that of the distributed lag in prices, 0.812, as against 0.854, with the sampling errors being virtually unchanged.

actual decline in the wage-rate index. In the case of equation (1e'), unemployment would have to be about 520,000 before this eventually would take place, with perfectly stable prices. Even in this case, many would doubt this conclusion.

We prefer to reserve judgment on this point. The ever-continuing upward movement in money wage-rates is only an assertion by some economists, who can read basic trends from something like a decade or two of experience. We believe that logically equation (1e) is more satisfactory than equation (1e'), since it reflects more closely the institutional character of wage bargaining, although it increases the possibility of a fall in the wage-rate index when the price level is stable. On the basis of our sample we are unable to reach any positive conclusion on this matter. Other economists have emphasised that the relation between changes in wage-rates and excess demand for labour is non-linear, so that a considerable rise in unemployment (say) may take place with the wage-rate index being unaffected.¹ We have seen no empirical evidence that justifies the fitting of a non-linear rather than a linear function to the sample data, though to some this procedure is highly plausible on *a priori* grounds.

In assessing the responsibility for inflation it is important to be able to specify the lags involved, if any. As already pointed out, the particular form of the wage-rate equation (1e) has a built-in lag of six months between wage-rate and price changes. However, our sample does not permit a statistical discrimination of any other lag.

We have already mentioned the possibility that either the level of profits, changes in productivity or both should be included as variables in our wage-rate equation. Utilising the method of least squares, we have experimented with their introduction into equation (1e').

Productivity change over four quarters when introduced into (1e') leaves the estimates of the other coefficients nearly the same and itself carries a negative coefficient. As a result of this finding we concluded that the inclusion of productivity change would not improve our basic model.

The influence of profits was assessed by a similar least-squares computation, using as a variable the average of our index of company profits over the last four quarters. The result was that a positive coefficient larger than its sampling error was obtained on the profit variable. The coefficients of the other variables were, however, reduced, and strong auto-correlation was introduced into the residuals. This seems to lend weight to the idea put forward above that the influence of profits is largely reflected already in the

¹ In this connection we may mention the work of J. C. R. Dow and L. A. Dicks-Mireaux "The Determinants of Wage Inflation: United Kingdom, 1946-56," *J.R.S.S.* forthcoming and a recent paper by A. W. Phillips "The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861-1957," *Economica*, N.S. Vol. XXV, 1958, pp. 283-300. Phillips explains wage-rate changes in terms of unemployment alone, except in period of rapidly rising import prices. He estimates a non-linear (*i.e.*, logarithmic) relation between wage-rate changes and unemployment.

other variables. Accordingly, we have not included the profit variable in our unbiased computations. We have conducted these experiments in an equation of the form (1e'). We believe, however, that the results obtained may be extended to an equation of the form (1e), and have therefore not included either the change of productivity or our profits variable in our basic equation.

Equation (2e) above represents the spread between earnings and wage-rates. As it stands, this is in terms of the levels. The degree of correlation is estimated at 0.93, which indicates a fairly close agreement between the actual and computed value of the spread. Unlike the previous equation, however, its residual variation is decidedly not free of serial correlation. For this equation the measure $\frac{s^2}{s^2}$ comes out as low as 0.73, which indicates a fairly high degree of auto-correlation in the residuals. The reason for this is not far to seek. Over the period covered by our sample, the spread has grown at a fairly steady trend rate. However, in explaining the spread we have used variables that are cyclical in character, the result being that we have introduced a cycle into the residuals which is inversely related to the cycle followed by the variables used.

In this case we have made an experimental calculation which removes the serial correlation of residuals, *i.e.*, we have transformed all the variables to first differences.¹ With the first-difference transformations we have made a simple least-squares estimate of (2e) to compare with the same kind of estimate of the equation expressed in its original form. The two least squares estimates are,

$$\begin{aligned}
 (2e') \quad (w_e - w_r)_t &= 1.55h_t + 0.538(P/hE_p)_t - 211.38 - 0.414Q_{1t} + \\
 &\quad (0.67) \quad (0.118) \quad (56.95) \quad (0.93) \\
 &\quad 1.10Q_{2t} + 4.75Q_{3t} \\
 &\quad (0.94) \quad (1.39) \\
 (2e'') \quad \Delta(w_e - w_r)_t &= 1.737\Delta h_t + 0.044\Delta(P/hE_p)_t - 0.00657\Delta Q_{1t} + \\
 &\quad (0.365) \quad (0.09) \quad (0.3275) \\
 &\quad 0.346\Delta Q_{2t} + 0.205\Delta Q_{3t} \\
 &\quad (0.377) \quad (0.87)
 \end{aligned}$$

The estimated equation (2e') is roughly similar to that in (2e), and the degree of serial correlation in the residuals is equally high. On the other hand, (2e''), the estimate from transformed data, has no significant serial correlation in the residuals but gives a different figure for the relative importance of productivity in accounting for the spread. If anything, our estimates in (2e) are over-generous in attributing fluctuations in the spread to those in productivity.

Our results are not inconsistent with the hypothesis that the spread is largely influenced by the level of demand. As pointed out above, hours

¹ These differences are from quarter to adjacent quarter and not over four quarter periods as in equation (1e).

worked constitute a very sensitive indicator of the level of demand. In one sense hours worked contribute to the spread in a purely accounting manner, as do, other things being equal, increases in output per man hour for piece-workers.¹ We are unable, however, to separate out the relative importance of these two influences. No doubt, overtime, bonus payment, premium rates and changes in the length of the "official" working week have all been important. On the demand side, it is often argued that a high level of demand has led to payments above the "official" rates to bid labour away from some firms into others. Our results are consistent with either hypothesis alone or a combination of both.

It is interesting to compare our results with those obtained by the Swedish economists Bent Hansen and Gösta Rehn in their study of the Swedish economy.² They start from the assumption that wage-rates are fixed institutionally, and that the influence of economic forces is reflected in the spread between earnings and wage-rates, which they describe as the "wage drift." In our model we have not followed this assumption about rate fixing, putting forward instead the hypothesis that changes in wage-rates are influenced by changes in the cost of living, by the demand for labour and by the political climate.

The procedure followed by Hansen and Rehn was to take a sample of annual data 1947-54, applied to eight main groupings within Swedish manufacturing industry. Briefly, their findings suggested that the main influence determining the "wage drift" in Sweden over these years has been excess demand. They tested the further influence of "excess profits," and the hypothesis that increases in productivity have contributed substantially to the drift in the manner outlined above. Neither was found to be significant.

The relations estimated were between the rate of change of the "wage drift," the level of excess profit, the level of excess demand and the rate of change of productivity. It may be pointed out that while in our model productivity makes a significant contribution to the spread between earnings and wage-rates, when all variables are expressed as levels, productivity ceases to be a significant factor in our least-squares computation, in which rates of change were introduced. It would appear therefore that our findings are not inconsistent with those of Hansen and Rehn. However, it must be re-emphasised that we have included an index of hours worked in our computation, which is an indicator of the direct influence of demand in the spread, but which also reflects other influences, and we have used weekly and not hourly earnings. Hansen and Rehn, on the other hand, construct

¹ Among other things equal here, would be the timing of particular jobs. The effects of higher productivity on the spread may be to some extent offset by re-timing and re-estimating after the introduction of new capital equipment. We are unable to make any reliable estimate of the importance of this factor in cancelling out productivity changes.

² B. Hansen and G. Rehn, "On Wage Drift, a Problem of Money Wage Dynamics," printed in *Economic Essays in Honour of Erik Lindahl* (Stockholm; Ekonomisk Tidskrift, 1956, pp. 87-139).

an index of excess demand for labour, in some cases by taking the difference between unfilled vacancies and numbers unemployed, in others, where the numbers unemployed in the industry could not be ascertained, by using vacancy statistics alone.¹ We have included an explicit labour-demand variable, in the form of an average of the last year's unemployment, in the wage-rate equation (1e). This reflects our belief that the demand for labour can affect the wage bargain as well as the "wage drift."

We include the equation for hours worked in the sub-system to illustrate one of the ways in which demand enters the inflationary process. Our estimates in (3e) produce a correlation coefficient of 0.93 and a serial correlation measure of 1.23. Again we find evidence of slight serial correlation in residual variation.

It appears that the most important factor explaining the level of hours worked is the level of production. The coefficient of production is small, but many times its sampling error. The small size of the coefficient is explained by the relative magnitudes of the range of the hours and production indices over the sample period. Hours worked are, on the whole, a fairly stable input into the productive process. Our estimates show that roughly a 10% change in the industrial production index is required to produce a 1% change in the hours index. The unemployment variable adds little to the computation. The coefficient is small, as one might have expected, but the sign differs from that anticipated, and the sampling error is sufficiently large to admit the hypothesis of either a positive or negative true value.

Thus it would be possible to substitute industrial production for hours worked in equation (2e), illustrating the relation of the spread between earnings and wage-rates to the force of demand. Under the assumptions made in our model, this would, however, obscure the underlying chain of relationship.

Equation (4e) is the price or "mark-up" equation. As pointed out above, the price index here used is that of the general price level of consumer goods. In the econometric model we also determine the price of investment goods, as a function of earnings and imported non-ferrous metal prices. These two price indices can be weighted together to obtain an index of the general price level if so desired. The result established by the limited information estimate of (4e) is among one of the firmest in the model. The degree of correlation corrected for degrees of freedom is 0.996, which is very high. The measure of serial correlation $\frac{\delta^2}{\bar{s}^2}$ is 1.16, which indicates a moderate amount of serial correlation in the residuals.

¹ Hansen and Rehn attach significance to the use of vacancy statistics in addition to those on unemployment in order to measure excess demand during a period of sustained inflationary pressure. Following the suggestion of J. C. R. Dow, we have made experimental computations with a measure based on vacancy statistics (unemployment deflated by index of vacancies), and found the results on the basis of least-squares estimates not to be different in any essential way from those using ordinary unemployment figures. Cf. J. C. R. Dow and L. A. Dicks-Mireaux, *loc. cit.*, for comparisons of vacancy and unemployment statistics.

It will be observed that the coefficients of $(w_e)_t$ and $(p_i)_{t-2}$ are many times their sampling errors. The coefficient of our measure of the influence of indirect taxes, however, is small, and very much smaller than its sampling error. As stated, we have expressed total indirect taxes paid, less subsidies, as a proportion of consumers' expenditure. Variations in this ratio have, on the whole, been small compared to the substantial changes in the indexes of earnings and import prices, which are also included in the equation. Since the simple correlations between $(w_e)_t$ and p_t and between p_t and $(p_i)_{t-2}$ are so high, there is very little residual variation to be explained by the indirect tax variable. As changes in indirect tax rates are specifically directed, it might, perhaps, be argued that such changes tend to affect prices of, and expenditures on, particular goods, but will show little effect on the consumer price level as a whole, since the effects of the imposition of higher taxes on specific goods, may simply be to divert the expenditures elsewhere.

In the case of this equation there is remarkably close agreement between the results of the estimation by the method of limited information and by the method of least squares, which is largely a result of the high overall level of correlation. Applying the least-squares procedure to the equation in the form presented above, we obtain the result

$$p_t = 0.418(w_e)_t + 0.217(p_i)_{t-2} + 0.023(T)_t + 34.83 + 0.94Q_{1t} + 2.06Q_{2t} + 1.18Q_{3t} \\ (0.013) \quad (0.030) \quad (0.160) \quad (12.92) \quad (0.50) \quad (0.50) \quad (0.49)$$

In a prototype model, based on a decade of annual data, this equation was estimated using Theil's two-rounds method,¹ in the form $p_t = \lambda + \lambda_1 (w_e)_t + \lambda_2 (p_i)_t$. Using annual data, the seasonal variables and the lag in import prices disappear. We then obtained $\hat{\lambda}_1 = 0.48$ and $\hat{\lambda}_2 = 0.34$, where the $\hat{\lambda}_i$ are estimates of the λ_i . There is thus a very close relation between these three results, all obtained using different methods of estimation, and in the latter case using annual rather than quarterly data.

We experimented, using least-squares estimates, with the introduction of productivity into the equation. In formulating the "mark-up" in his statistical study, Dow introduced productivity as an alleviating factor on the influence of a rise in earnings on prices.² This follows the general custom of arguing that prices will be stabilised provided that earnings do not rise faster than productivity, or more generally that in times when productivity is rising fastest, prices will rise less fast than earnings. The least-squares estimate yielded a small coefficient of productivity with a positive sign contrary to expectations, and the sampling error was sufficiently large for the "true" value to be assumed to be either positive or negative. We,

¹ H. Theil "Estimation of Parameters of Econometric Models" in the *Proceedings of the 28th Session of the International Statistical Institute* (Rome, 1953), Tome XXXIV 2eme Livraison, pp. 122-9.

² Dow, *loc. cit.*, p. 269.

therefore, concluded that our result afforded no evidence for the belief that the "mark-up" varies in response to changes in productivity. Our results suggest that earnings have been roughly twice as important, at the margin, as import prices in determining the level of consumer prices over the period of our sample. It must not, however, be forgotten that in some sense we have established an average relationship, and this does not rule out the possibility that over certain periods within the sample, import prices may have contributed very much more than earnings to the rise in prices, as, for instance, at the time of the Korean War and shortly afterwards. How close we are to having established a basic structural relationship depends on the representative character of the sample, and as we have suggested elsewhere in this paper, there are reasons which give rise to doubt whether our sample is a representative one.

CONCLUSIONS

Our particular model of the inflationary process brings out points that have been raised by different authors and attempts to follow through some inter-related patterns of behaviour in the sphere of wage and price determination. This model contains elements of both cost and demand inflation. Our model is not unique, as judged by its agreement with observed data, and it contains flaws; nevertheless, it appears to be reasonable, and the difficulties that it encounters are inherent in the nature of our basic economic information. We are not able to discriminate between certain competing key hypotheses, but it does not appear that other methods of analysis will be able to do so either.

The weaknesses of our results, such as serial correlation of residuals and some relatively large sampling errors, may perhaps be erased in a model of the same general type that specifies different relations among the variables or uses some more complex methods of parameter estimation, but we know of no such findings. Our model is aggregative, as is most of the analysis of the inflation problem, but one disaggregative type of analysis has been mentioned by several writers, namely, that changes in productivity in certain leading and rapidly advancing industries have been influential in securing favourable wage bargains in those industries in the first instance and that such awards have later spread, on equity grounds, to other sectors of the economy where productivity has advanced less rapidly. Phenomena like these are not adequately handled by our aggregative model, but they are not necessarily contradictory with what we find.

The model we have presented does not have irreversibilities that often are ascribed to the behaviour of wage-rates in modern society, but for the period under review a model with irreversibilities will probably not function much differently from ours. The test of the irreversibility hypothesis is yet to come, but we remain unconvinced of its correctness.

Our statistical analysis covers the post-war period as a whole. It gives

a set of average relationships, which do not rule out dispersion. In the Korean period import prices were obviously of extraordinary significance. Some writers, such as Robbins, point to mainly demand inflation until the most recent months, when cost influences are considered to be dominant. Our only claim is that our model will tend to depict behaviour on the average.

We cannot say whether there is a lag of a quarter or more in the adjustment of wage rates to price changes (see equation (1e)), but if there were definite evidence in favour of a lag we would lean more to the side of demand than to the side of cost inflation in the absence of substantial changes in import prices. The main element of demand inflation in this equation is through the effect of unemployment on wage-rates. Our estimates of equation (1e) indicate the importance of the demand for labour, as represented by our variable on unemployment, on changes in wage-rates.

Demand also enters the process through the strong influence of hours worked per week on the earnings-rate spread—the wage drift. Through the mark-up equation we have a measure of the influence of labour cost on price and import cost on price. These are both aspects of cost inflation, but the respective origins of cost must be kept firmly in mind when assessing the blame for inflation and formulating remedial policy action.

On the basis of our results we should be inclined to rule out the direct structural influence of productivity on wage-rate setting or on price mark-ups.¹ We similarly exclude profits from direct consideration in the rate equation. This is not to deny obvious chains of indirect influence of either variable throughout a larger and more complete inter-related system.

Our main point is that we should keep the inter-related structure of the economy more firmly in mind and not fall into the trap of ascribing our present difficulties with control of inflation to any single source in the economy.

As these pages are written, some countries in the Western World are experiencing a cyclical sag in output and accompanying growth in unemployment. Many people have been impressed by the fact that even during this cyclical phase, prices and wages have continued to rise, and they have taken this to be evidence that the prevailing structure should be described as cost, rather than demand, inflation.

Two points may be made in this connection. In the first place with regard to changes in the general consumer price level, it has been pointed out by Clague, that in the case of the United States, rising prices during the early stages have been a feature of previous recessions.² The American Consumer Price Index continued to rise for some time after the fall in production and employment and finally fell in the summer of 1958. The

¹ Subject to the above-cited influence of pace-setting industries.

² E. Clague "The Consumer Price Index in the Business Cycle," *Monthly Labor Review*, Vol. 81, 1958, pp. 616–20. G. H. Moore, in his work on statistical indicators of business cycles, confirms this laggard behaviour of consumer prices at the upper turning point. See G. H. Moore, "Measuring Recessions," *Journal of the American Statistical Association*, Vol. 53, 1958, pp. 259–316.

explanation of this is largely to be sought in the make up of the index (which closely resembles the price index used in our computation). The food component carries a large weight, and the comparative volatility of food prices is well known. Although, as Clague shows, durable goods prices respond fairly quickly to changes in the level of production, food prices are closely related to seasonal factors, and to the prevailing weather conditions. A poor harvest prior to the onset of a recession may keep food prices rising, the overall effect of which may offset falls in other prices to the extent that the total index rises rather than falls. The exceedingly great harvest of 1958 has been mainly responsible for the summer and autumn drop in American prices. In the longer run, it appears, food prices have adjusted themselves to the changed *general* economic conditions. Finally, a recession may get under way just before a normal seasonal pressure on food prices, so that the total index may still rise as output and employment fall. If the recession were sufficiently brief, it is possible that other prices may recover before the special factors keeping up the total index have worked themselves out, so that the impression is given that the recession has had no effect on prices. It follows that considerable care must be taken in interpreting movements in the consumer price level, when drawing conclusions about the character of current inflation. Wholesale prices, during the current American recession, did fall shortly after production turned down if food prices are excluded from the index.

Secondly, with regard to wage changes, it may be argued by those placing emphasis on demand that wage increases were much more difficult to obtain during the recession, and are much reduced in size. Bargaining power was distinctly impaired by unemployment. Both sides in the controversy have elements of truth in their arguments.

The dampening of demand had not fully checked inflation at the time of writing, and a simple theory leading to an equally simple policy proposal does not seem adequate to the situation. But neither would a "deal" with the trade unions on a national wages policy, advocated by some extremists on the side of cost inflation, do the trick.¹ It is a very simple solution based on a very simple theory. Just as we argue here for more tolerance for alternative points of view on the matter of a theoretical description of behaviour, we feel that a broader approach to policy control based on both demand and cost aspects of the situation would lead to more fruitful results.

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¹ B. C. Roberts, "Lessons of a National Wages Policy," *The Times Review of Industry*, May 1958, pp. 36-7. Roberts remarks that even if a sensible agreement is worked out on a national scale among government, employers and trade unions, "No trade unionist is able to stand silently by, as the Dutch have found out, when profits are rising rapidly." We may prefer to cite another measure of activity besides *profits*, but the point of Roberts' observation remains well founded.

THE EXPENDITURE TAX AND SAVING¹

THE publication of Mr. Kaldor's book *An Expenditure Tax*² has revived interest in the characteristics of a system of taxing personal expenditure rather than income. In addition to the many reviews of the book³ there have also been contributions on specific points.⁴ This note is confined to what is historically the central question about an expenditure tax: what will be the effect on personal saving if an expenditure tax is substituted for an income tax?

Let us consider first the case of a representative man who performs one single act of saving in year *o*. The return to saving is in cash form and is paid yearly at the fixed rate of $100r\%$ per annum (gross), the whole of this annual interest income being spent in the year of receipt. At the end of *n* years the principal is spent on consumption goods.

Professor Pigou⁵ has shown that with a proportional income tax at a rate of $100x\%$, the amount of tax payable in year *o* on £1 of income is £*x* if none of it is saved; if all consumption out of it is deferred until year *n*, the present value of the total tax payable is given by

$$£\left[x + x(1-x)\left\{1 - \frac{1}{(1+r)^n}\right\}\right]$$

Alternatively, if we look at net sums consumed and saved, the saving of £1 attracts a stream of future tax having a present value of $£x\left(1 - \frac{1}{(1+r)^n}\right)$,⁶ whereas no tax is payable at all in future years in respect of £1 consumed now.

If we now take a tax on expenditure levied at what may be called an equivalent rate to the income tax, we have a tax rate of $100e\%$, where $e = \frac{x}{1-x}$. £1 of consumption in year *o* will attract £*e* in tax; £1 saved

¹ I have benefited a great deal from discussions with Sir Dennis Robertson, Milton Friedman and A. D. Roy on this topic.

² London: Allen and Unwin, 1955.

³ E.g., R. A. Musgrave in *American Economic Review*, March 1957; I. M. D. Little in *Economic Journal*, March 1956; A. R. Prest in *Lloyd's Bank Review*, October 1956.

⁴ E.g., E. Cary Brown, "Mr. Kaldor on Taxation and Risk-bearing," *Review of Economic Studies*, October 1957.

⁵ *A Study in Public Finance* (London: 3rd edition, Macmillan), Chapter X.

⁶ It might be argued that the more appropriate rate of discount in the income-tax case would be the net rate of return, i.e., $100r(1-x)\%$ rather than the gross rate, $100r\%$. If so, the present value of tax payable on £1 of temporarily unconsumed income becomes $£\left[x + x\left\{1 - \frac{1}{(1+r(1-x))^n}\right\}\right]$. This refinement does not affect the general lines of our argument, but we shall take it into account when necessary.

in year o and dissaved in year n will attract tax to the present value of $\mathcal{L}\left[\frac{e}{(1+r)^n} + e\left(1 - \frac{1}{(1+r)^n}\right)\right]$, which reduces to $\mathcal{L}e$. Therefore the same (present value of) tax is payable whether $\mathcal{L}1$ is saved or spent. Similarly, it will follow in the expenditure tax case that the ratio of (the present value of) tax to income will be the same whatever the relative amounts spent in year o or saved until year n .

Hence, the substitution of the equivalent rate expenditure tax for the income tax must normally be expected to increase saving, the price of future consumption having been reduced relatively to that of current consumption. The increase in saving will depend on the exact extent of the reduction in the price of future consumption (given by x , r and n) and people's reactions thereto.¹

Or at least this would be so in the absence of uncertainty about future tax rates. In the case of the income tax, a rise in the tax rate in future years will mean a larger tax deduction from investment income; in the expenditure tax case it will mean not only a greater tax on investment income but also a greater tax on the spending of the principal in year n . It might be said that the most reasonable assumption in year o is that equal probabilities can be assigned to future tax increases and decreases. But this does not dispose of the point. On the usual risk-theory basis, some sort of premium must be deducted from the savings-incentive of the expenditure tax to allow for the possibility of future tax changes. How great this should be and how much it would offset the savings advantages of the expenditure tax is a matter of conjecture.

Up to this point we have compared equivalent rate income and expenditure taxes. But if we take $e = \frac{x}{1-x}$, the expenditure tax will clearly not produce as much revenue as the income tax to the Government when saving is positive, as it usually is, for society as a whole. To get the same revenue² we must postulate an expenditure tax rate of $100e'\%$ where $e' = \frac{x}{c(1-x)}$, c being the fraction of Personal Income (net of the tax payable in the income tax case) which is consumed in the expenditure tax case. It will still be the case that with an expenditure tax the tax burden on an individual in respect of $\mathcal{L}1$ saved will be the same as that for $\mathcal{L}1$ spent, but it will now

¹ We ignore the case where the supply curve of saving with respect to interest rates is negatively inclined.

² It might be argued that it is not necessary to secure the same revenue to the Government in both cases, on the grounds that the greater private saving with the expenditure tax may compensate for the smaller (greater) government surplus (deficit), thereby keeping employment and national income at the same level in both cases. In such an eventuality, however, one is not simply comparing the relative effects of an income tax and an expenditure tax on saving but rather the relative effects of an income tax and an expenditure tax plus the change in monetary conditions associated with the smaller (larger) government surplus (deficit).

be $\mathcal{L}e'$ in both cases and not $\mathcal{L}e$. The fact that the Government collects more revenue in year o in the e' case than in the e case will mean a smaller post-tax income for the private sector and hence, presumably, both less consumption and less saving, than in the e case. But saving will continue to be greater than in the x case.

There is another point to note at this stage. Although for any one individual the effect on his saving of the change in tax liability from $100x\%$ of income to $100e'\%$ of expenditure will depend on x , c , r and n and his reactions thereto, there is a further degree of complication for society as a whole. The change of basis will obviously improve the post-tax position of people with a high average savings/income ratio and will worsen the post-tax position of those with a low average savings/income ratio.¹ It does not necessarily follow that people with a high average savings/income ratio have a high marginal propensity to save nor that those with a low average savings/income ratio have a low marginal propensity to save; in fact, the reverse may be the case.² Therefore these "income effects" have to be weighed in with the previous "substitution effects" in any analysis of the overall effect of the tax change on saving.

We now come to a rather different sort of argument. We have so far concentrated on the effect of the tax change on the division of any given income between taxation, spending and saving. We now have to ask whether there are any reasons why income may not remain given.

The most obvious point to explore first is the effect of the tax change on the incentive to work. In so far as the choice between leisure and work depends on the price of *current* consumption, one must expect to get less work in the expenditure tax case as the tax cost of current consumption ($100e'\%$ of expenditure) will then clearly be greater than in the income tax case (when the tax cost is $100x\%$ of income) to an extent depending on c , the fraction of post-income-tax Personal Income consumed in the expenditure tax case. In so far as the choice between leisure and work depends on the rate of tax on *future* consumption, the position is more complex. In the income-tax case the present value of tax payable on $\mathcal{L}1$ of unconsumed income is, assuming saving for n years,

$$\mathcal{L}\left[x + x(1-x)\left\{1 - \frac{1}{(1+r)^n}\right\}\right]$$

In the expenditure tax case the present value of the tax payable on $\mathcal{L}1$ of saving is $\mathcal{L}e' = \mathcal{L}\left(\frac{x}{c(1-x)}\right)$. There is no certainty about which tax system will give the better terms of exchange at the margin between leisure and

¹ This also applies to the change from x to e ; but it will be more pronounced if the change is from x to e' .

² Cf. A. Henderson, "Direct v. Indirect Taxation," *ECONOMIC JOURNAL*, December 1948.

future consumption: the answer will depend on c , r , n and x . But the empirical values seem to be such that it is perfectly possible for the attractiveness of leisure to be increased relatively to future as well as current consumption in the expenditure tax case.¹ Paradoxically, the more successful the expenditure tax is in stimulating saving out of a given income, the higher must the rate be to keep equal revenue and the greater the disincentive to work either for the purpose of current or future spending.

More generally, we may say that the introduction of an equal yield expenditure tax will change relative prices so that leisure certainly becomes more attractive at the margin relatively to current consumption and may become more attractive relatively to future consumption. The final answer will depend on the degree of complementarity on the demand side between leisure, current and future consumption and the relative weights attached to each relationship. If, for instance, leisure is in a relationship of complementarity on the demand side with future consumption and one of substitutability with current consumption, work incentives will almost certainly be impaired. If the relationships between the demand for leisure *vis à vis* those for current and future consumption are reversed, it is possible (but not certain) that work incentives will be improved. All in all, it seems fair to conclude that work incentives are more likely to be impaired than improved.

The importance of this point is simply that, even though the introduction of the expenditure tax may have caused an increase in the fraction of post-tax Personal Income saved, total Personal Income may fall so much that the absolute amount of saving could be less than in the income-tax case.

Parallel arguments can be brought forward about the relative incentives to take risks under an income-tax system and under an expenditure tax. In so far as one thinks that a differential margin is necessary to push investment from non-risky to risky industries, the tax of $100e'\%$ on spending will eat up more of this margin than the income tax in the case of current consumption and may eat up more in the case of future consumption. However, in so far as losses can be offset against other forms of income, there is little to choose between the risk-incentive effects of lump sum, accrued income and expenditure taxes. So therefore the net effects on risk-taking, and hence the effects on total income via this route may not be very great.²

A final very small point might be that the greater administrative cost of

¹ E.g., if $x = 0.5$, $r = 0.05$ and $n = 1$, the present value of tax on the marginal £ of unconsumed income in the income tax case is £0.512. If we take c as 0.9, the corresponding tax under the expenditure tax system is £0.526. If, on the other hand, $n = 10$ and the other variables are unchanged, the expenditure tax wins (£0.526 against £0.597 in the income tax case).

The substitution of the net rate of return as the rate of discount in the income tax case would make no difference in the first example, but would slightly improve the margin of superiority of the expenditure tax in the second.

² Cf. E. Cary Brown, *op. cit.* Also N. Kaldor, "Risk-bearing and Income Taxation," *Review of Economic Studies*, June 1958.

running an expenditure tax will also reduce the amount of national product available for division between personal spending and saving.

These seem to be the principal arguments in the general case of a single act of saving destined to last for n years. We must now ask how the argument should be modified in other possible cases.

First, suppose we imagine that the interest income during years $1 \dots n$ is saved and not spent. In this case, the relative tax burdens on spending and saving are unchanged in the income tax case; in the expenditure tax case the tax is $£e'$ if £1 is consumed now, and the present value of tax $£e' \frac{(1+r)^n}{(1+r)^n} = £e'$ if £1 is saved now and dissaved (together with accrued interest) in year n . Therefore nothing has happened to change the previous conclusions.

Second, suppose we imagine that an act of saving is meant to last for ever but that the interest income is spent. In the income tax case $£x$ is payable on £1 of income if none of it is saved, but $£[x + x(1-x)]$ is the present value of tax payable if none of the £1 of income is consumed.¹ In the expenditure tax case, as before, $£e'$ is payable whether £1 is saved or spent. It must therefore be concluded that in this case there is a greater incentive to substitute saving for consumption as a result of the change in the tax system than in our main example. As for work incentives, the considerations on the price of current consumption remain as before; but there is now much more likelihood that the tax rate on unspent income will be less in the expenditure tax case than in the income tax case.² Therefore the damping effects of work incentives on saving will be less.

Third, suppose we envisage not only a permanent act of saving but also the saving of the interest earned thereon. The tax burdens in the income tax case are the same as those in the preceding paragraph. In the expenditure tax case they will be $£e'$ for £1 of current consumption and zero for £1 of saving. This is clearly the strongest case of all those considered for the expenditure tax, both in respect of the substitution of saving for consumption and in respect of the relative effects on work incentives.

A few subsidiary points may now be noted. If the changeover were not permanent but merely temporary (e.g., for the duration of a war) an expenditure tax can have more powerful savings incentives than if the switch is for good. Taking the case of saving for more than the duration of the

¹ Pigou, *op. cit.*

² Depending on the comparison of $\frac{e'}{1+e'}$ with $x(2-x)$. In order for the price of saving to be greater in the expenditure-tax case, we should need to have $\frac{(1-x)}{(2-x)} > c$, and this would seem very unlikely empirically.

period concerned with interest income spent as it is earned, the present value of tax burdens in the income-tax case are once more $\pounds x$ in the case of $\pounds 1$ of unsaved income and $\pounds \left[x + x(1-x) \left\{ 1 - \frac{1}{(1+r)^n} \right\} \right]$ in the case of $\pounds 1$ of unconsumed income. In the expenditure tax case tax on $\pounds 1$ spent now is $\pounds e'$; as any sum saved is assumed not to be spent until after the withdrawal of the expenditure tax the present value of tax on $\pounds 1$ saved is $\pounds e' \left\{ 1 - \frac{1}{(1+r)^n} \right\}$, and so there are stronger incentives to saving in this case than in that of the permanent change.

We have not considered the case of progressive as distinct from proportional taxes, but this does not make any basic difference to the argument. In so far as people mainly save when their incomes are high (*e.g.*, when young) and dissave when incomes are low (*e.g.*, when old) the expectation of a favourable change in tax rates by the date of dissaving will be greater to anyone now contemplating saving. But in this case the expenditure tax rate structure may have to be geared to the fractions of income saved by the different income brackets rather than the fraction for the whole community, if equal revenue is to be forthcoming to the Government. This could have a wide variety of possible effects on incentives to work and to take risks, depending on exactly how the tax burden was distributed.

A final theoretical point is that we have not troubled ourselves about the definition of the base of an income tax or expenditure tax. It can soon be seen that there are endless complications here, and two examples may be indicative. If we have an income tax excluding capital gains, we then have to ask how far people save in the expectation of non-taxable income of this sort. The possibility of a fall in interest rates, and hence a larger tax base at the time of dissaving, would be a risk in the expenditure tax case to which there is nothing corresponding in the income tax case. On the other hand, if expenditure is defined to include education and training and saving is defined to include house-building, one of the major effects of changing the tax base might be to encourage the latter and discourage the former, as distinct from any change in the total of "saving."

If we were to consider an expenditure tax system seriously as a means of stimulating saving in the United Kingdom, the first empirical question to be answered would be the extent to which the rich are currently dissaving. The present income tax system offers encouragement to saving in a large number of ways (self-employed pensions, life insurance, tax-free savings certificates and premium bonds, etc.), but does not offer the discouragement to dissaving provided by an expenditure tax—and particularly the discouragement to dissaving by rich people. I know of only one United Kingdom sample survey specifically concerned with upper-income-group

savings, and this shows no evidence of actual dissaving by upper-income groups, apart perhaps from the elderly.¹

The second empirical question to be answered is the motivations behind saving. We have seen that expenditure tax advocates are on the strongest ground when saving is for ever and on the weakest when it is for a short period only. Evidence in the United States² suggests that saving for the purpose of bequeathing to one's heirs is extremely rare and that the usual reasons (or at any rate rationalisations) for saving are to meet emergencies or specific goals such as educating children. It may well be that these reasons do not apply in the United Kingdom, but until this is firmly established it seems reasonable to be cautious about the extent to which the substitution of an expenditure tax for the income tax would increase saving in the United Kingdom today.

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¹ Klein, Straw and Vandome, "Saving and Finances of the Upper Income Classes," *Bulletin of the Oxford University Institute of Statistics*, November 1956.

² Cf. G. Katona "Attitudes towards Saving and Borrowing," *Consumer Instalment Credit* (National Bureau of Economic Research and Federal Reserve Board, 1957), Pt. II, Vol. I. Also *Consumer Optimism Weakening, Foundation for Research in Human Behaviour* (Ann Arbor, Michigan, July 1957).

EXPORT PROSPECTS AND ECONOMIC GROWTH: INDIA¹

Food, domestic finance and foreign trade seem to have operated as the unholy trinity thwarting India's efforts to initiate a process of sustained economic growth. The subjects have been examined by numerous expert bodies and often appeared almost solved. But, invariably, the respite so eagerly looked for was only temporary and these ugly problems always reappeared, sometimes in more menacing forms, stressing the need for a re-examination all over again. The recent foreign-exchange crisis is a striking illustration of this phenomenon.

During the last two years, imports have been running at a level 40-50% higher than in the years preceding the start of the first five-year plan. Export earnings, on the other hand, have remained stubbornly unchanged, showing a marked decline during the first half of 1958. At present they cover no more than about two-thirds of the imports, in contrast to 90% or more between 1950 and 1956. This obstinacy of the export trade, rather ungracious to the efforts that India has invested in export promotion, may well continue, as this paper suggests, over the years to come. The import needs, on the other hand, are increasing. Perhaps more important still, the burden of repayment of capital and interest on foreign loans, now amounting to less than 2% of the export earnings, is expected by the Planning Commission to rise during the 'sixties to nearly a fourth of the present export proceeds. There is thus a need for re-appraising the export performance over the last few decades and seeing whether this provides any background for viewing the export prospects for the years to come. This paper is an attempt in this direction. The past experience is reviewed in Section I, whereas Sections II and III are devoted to outlining the perspectives and their implications for economic growth.

The exports from India accounted for 1-1½% of the world trade in post-war years. As in some of the very large countries of the world, such as the United States, the Soviet Union and China, foreign trade in India on the whole touches a relatively small, though a very important, part of domestic economic activities. The total value of India's exports amounts to no more than 6-7% of the national income. Although exports account for such a small share of the national income, they play a very strategic role in the Indian economy. They provide the means—foreign-exchange earnings—with which the pressing import needs can be financed.

¹ The author is a member of the Secretariat of the United Nations Economic Commission for Europe at Geneva. The views expressed herein, however, are his personal views and should not be interpreted as those of the Organisation.

I. LONG-TERM TRENDS

Stagnation in Exports

The developments that have taken place in the foreign trade of India over the last quarter of a century are shown in Tables I, II and III. These

TABLE I
Developments in the Foreign Trade of India

Index numbers, 1953 = 100

	1929.	Average of the years:			1955.	1956.	1957.
		1937-39.	1946-50.	1951-55.			
1. <i>India</i>							
<i>Volume</i>							
Exports . . .	169	162	103	102	115	110	119
Imports . . .	148	131	115	118	125	147	168
<i>Unit value</i>							
Exports . . .	37	24	87	114	98	102	102
Imports . . .	35	25	81	102	95	99	107
<i>Terms of trade</i> . . .	106	96	108	112	103	103	95
2. <i>Volume of exports</i>							
World exports . . .	—	71*	77†	102	114	124	131
Burma, Ceylon, India, Pakistan: exports . .	—	118*	93†	102	111	110	110

Source: United Nations, *Statistical Yearbooks*, 1953 and 1955, and *Monthly Bulletin of Statistics*.

Note: The indices should be used only as approximations in view of the very serious changes in the geographical coverage, composition of trade and methods of calculation over such a long period. Data for 1937-38 refer to present territories of both India and Pakistan, and 1929 data include Burma also.

* 1937-38 only.

† 1948-50 only.

changes, however, have to be interpreted with a great deal of caution, since very important territorial changes,¹ affecting significantly the needs of imports and availabilities for exports, have taken place in this period. The indices of volume and unit value of exports and imports (see Table I) are at best very poor reflectors of these changes. Bearing these qualifications in mind, several broad observations can be made.

(a) It is striking that the volume of exports has declined sharply over the last three decades, whereas the volume of imports has changed much less. A proper comparison with 1929, of course, requires adding the present volume of exports and imports of Burma and Pakistan and subtracting the trade among the three countries. A rough-and-ready adjustment in this direction suggests that the volume of exports from these three countries in the last quinquennium is still a seventh lower than in 1929; the volume of imports, on the other hand, would seem to be higher by about a seventh.

¹ The regions now constituting Burma and Pakistan were separated from the sub-continent of India during the 'thirties and 'forties, respectively.

The quantum of world exports has increased by more than two-thirds in the course of the last two decades, but the combined volume of exports from Burma, Ceylon, India and Pakistan is now actually lower by a tenth.¹ Thus, the vast expansion in world trade over the last three decades has simply by-passed the Indian sub-continent.

(b) These developments were reflected in strains on balance of payments in recent years. Instead of the export surpluses, common in the pre-war period, there are now continuous import surpluses. The long-term developments have thus created a basic imbalance between exports and imports, a situation which efforts at export promotion and continued import restrictions in post-war years have failed to alter significantly.

(c) In the development of this imbalance, the changes in terms of trade do not seem to have played a major role (except during the 'thirties). The favourable change in the terms of trade in the early post-war years has disappeared, with the terms of trade now falling to the 1937-38 level.

Direction of Exports

The historical trend in the decline of the volume of exports assumes great significance when the data on the direction of foreign trade are related to it (see Tables II and III). In general, the changes in the direction of India's foreign trade have remained minor over the last few decades. It is true that the proportion of exports going to the United Kingdom has increased slightly; in contrast, there was a sharp decline in exports going to continental western Europe, particularly Germany. Trade with the dollar area is now somewhat higher, whereas the share of Japan remains far below that in 1928.

Broadly speaking, the private-enterprise industrialised countries have continued to account for over 70% of India's exports. This, when read with the changes in the volume of trade pointed out in (a) above, suggests that the industrialised countries have increased the volume of their exports to India but have reduced the volume of their imports from India.

The share of the other pre-industrial countries in India's exports rose very sharply in the early post-war years when the exports from the European countries had not fully recovered. Since then, the proportion has continued to decline, it now being a little higher than in the late 'twenties.

So far, the centrally planned economies do not account for a significant proportion of exports from India, although their share has increased very rapidly in the last two to three years.

As can be seen from Table III, the value of imports from all primary-producing countries into western Europe, the United States and Canada has nearly trebled between 1928 and 1956. Their imports from India, Pakistan, Burma and Ceylon, however, have increased very slightly. Since prices

¹ The decline would be obviously much greater if trade among these four countries and exports of cotton textiles to other countries (which were previously used within the sub-continent) are excluded.

during this period have risen considerably, it is obvious that the volume of imports from these countries into the industrialised countries, India's traditional trade partners, has fallen markedly. The stagnation of India's exports is thus not due to the fact that India was dealing with trade partners whose imports were stationary or declining. Indian exports have simply failed to share in an otherwise expanding market.

TABLE II
*The Direction of Exports from India **

Destination.	1928*	1938*	1950	1956
Value in millions of current U.S. dollars.				
<i>Total exports</i>	1,250	617	1,172	1,251
of which to	Share in % of total.			
1. <i>Private-enterprise industrialised countries</i>	77	75	62	71
United Kingdom	22	34	22	31
Western Germany	10†	5†	2†	2
Other western Europe	18	13	8	9
Australia, New Zealand and Union of South Africa	3	3	6	5
Dollar area	14	11	23	19
Japan	10	9	1	5
2. <i>Centrally planned economies</i>	2	3	1	6
China	2	1	—	1
Eastern Europe and U.S.S.R.	—	2	1	5
3. <i>Other pre-industrial countries</i>	21	22	37	24
Rest of the sterling area	10	15	27	16
Middle East	...	3	4	6

Sources: League of Nations, *Network of World Trade* (1942); United Nations, *Direction of International Trade* (1953, 1955 and June 1957).

* Pre-war data refer to India, Pakistan and Burma for 1928, and to India and Pakistan for 1938.

† Whole of Germany.

The stagnation in the exports from India is associated with a fundamental shift that has taken place in the import demand of the industrial countries.¹ Throughout the nineteenth century and the first quarter of the twentieth century, agricultural products accounted for over 90% of the imports into the industrial countries from the primary producers. In recent decades, however, these commodities are giving way to petroleum and mineral products, the exports of which now account for nearly two-fifths of the total exports from the primary producers to industrial countries. Among the agricultural exports, the decline was much sharper for raw materials than for food products.

This has acted very unfavourably on the exports from many countries. Thus, for instance, the combined current value of exports from Burma, India, Pakistan, Ceylon, Indonesia and Indo-China in Asia, Egypt in Africa

¹ For a detailed discussion, see United Nations Economic Commission for Europe, *Economic Survey of Europe in 1957* (Geneva, 1958), Chapters IV and V. This and the next paragraph are a broad summary of the findings in this study.

TABLE III

Imports into Industrial Countries from India, Pakistan, Burma and Ceylon

Destination.		Imports from		Imports from India, etc., as a share of imports from all primary producers, %.
		All primary producers.	India, Pakistan, Burma and Ceylon.	
		Billion U.S. dollars at current prices.		
Western Europe	1928	4.5	0.70	15.6
	1937-38	3.9	0.47	12.1
	1952	10.2	0.73	7.2
	1956	12.2	0.79	6.5
United States and Canada	1928	2.0	0.20	10.1
	1937-38	1.4	0.11	8.2
	1952	6.0	0.34	5.7
	1956	6.5	0.31	4.7
Total western Europe, United States and Canada	1928	6.5	0.91	14.0
	1937-38	5.3	0.59	11.2
	1952	16.2	1.07	6.6
	1956	18.8	1.11	5.9

Source: United Nations Economic Commission for Europe, *Economic Survey of Europe in 1957*, Chapter IV, p. 18, and Appendix Table IX. The figures are based on the statistics of the importing countries and are therefore not strictly comparable with those in Table I.

and Argentine in Latin America in 1956 was about the same as in 1928, although the current value of world as well as primary producers' exports increased two and a half times during this period. In 1928 these areas constituted nearly half the population of primary producing countries and accounted for slightly less than half the exports of the primary producers. As a result of the sharp structural shift in the import demand of the industrial countries, however, the share of these primary producers has now shrunk to only a sixth of the total exports from the primary producers.

Commodity Composition of Exports

This structural shift in the import demand of the industrialised countries in the west, India's traditional trade partners, is illustrated by the performance of the commodities that predominate in India's export trade. The level of exports of a country is influenced by several factors, such as the changes in demand (external as well as internal) and the adaptability of output to these changes. An analysis of the developments in the demand and supply of the major export items that figure in India's foreign trade is thus essential for a proper understanding of the stagnation in the overall volume of exports.

Agricultural commodities, whether in a raw form as tea, tobacco, spices, etc., or in a more elaborate form as cotton and jute textiles and vegetable oils, form the bulk of India's exports, accounting for nearly three-fourths of the total. Thus, for instance, jute manufactures make up nearly a fourth, tea a fifth and cotton textiles a tenth of the total (the three together are responsible for half of the total); another quarter is added by nuts and

vegetable oils, spices, tobacco, fruits and vegetables, gums and resins, and hides, skins and leather. Non-metallic minerals and metal ores (mainly mica, iron ore and manganese) account for only 3-5%. The factors influencing the exports of some of the major items are examined below.

Jute and Manufactures. The data on changes in world demand and output over the last half a century are summarised in the table below:

	In thousand tons (annual averages)					
	1900-9.	1910-19.	1920-29.	1930-39.	1940-49.	1950-54.
World consumption	1,300	1,527	1,681	1,711	1,469	1,789
of which: United States	266	314	383	323	293	302
Others	1,034	1,213	1,298	1,388	1,176	1,487
Output (India plus Pakistan)	1,312	1,556	1,522	1,499	1,420	1,684

Source: FAO, *Jute, Commodity Series No. 28*, pp. 52-3, 63-4.

On the whole, the world consumption of jute has expanded but little over the last five decades; while weather may have played some part in restricting the output of raw jute in certain years, it is clear that the major reason for this stagnation is the limited world demand rather than the ability to expand output. The peak output in the years 1912-14 (First World War), 1939 and 1940 (Second World War) and 1951 and 1952 (Korean conflict) was nearly equal in all these instances—illustrating that a rise in demand could induce sizeable increases in output. In the more recent period, the output in the two years 1951 and 1952 was some 40% above that in the preceding as well as the following three years.

Tea. Next in importance is tea. As the data in the table below show, world supplies have increased faster than world imports, and output in India has again increased considerably faster than exports. The increase in world imports has lagged behind growth in population.

	Annual averages				
	1934-38.	1949-51.	1952-54.	1949-51.	1952-54.
	Million lb.			Index No.	
				1934-38 = 100	
World supplies *	981	1,166	1,233	119	126
World imports	871	913	996	105	114
Consumption in producing countries †	110	253	237	230	215
India: ‡ supplies	(390)	610	625	156	160
Domestic absorption †	(70)	170	190	243	271
Exports	(320)	440	435	138	136

Source: International Tea Committee, *Report 1954-55*, pp. 28-9.

* Defined as output in the main tea-producing countries and exports from China and Japan.

† Derived by subtraction.

‡ Pre-war data adjusted to refer to present-day Indian territories.

Cotton Textiles. The general decline in import demand for cotton textiles is too well known to need additional emphasis. For instance, world trade in cotton and rayon piece goods declined by 10% between 1910-13 and 1926-28, by 13% in the next ten years, and by a further tenth between 1936-38 and 1951¹ (notwithstanding the statistical boost given by the partition of India, whose cloth output previously consumed by Pakistan

¹ ECE, *Economic Survey of Europe Since the War* (1953), p. 191.

began to appear in world exports). Although the world population increased by more than half during this period, the actual volume of textile exports was thus nearly 30% lower.

The changes in output and exports of cotton cloth in India in recent years are shown below:

	1950.	1951.	1952.	1953.	1954.	1955.	1956.
	Million yards						
Output	3,660	4,080	4,596	4,884	5,004	5,100	5,306
Exports	1,116	780	600	600	864	744	744
	In % of output						
Exports	30	19	13	12	17	15	14

Source: *Monthly Abstract of Statistics* (New Delhi).

Thus, output has gone up by nearly 45%, but exports have fallen by a third from the peak levels in the early post-war years.

Groundnut Oil. The developments in the output and exports of groundnuts and oil (the most important item in the group nuts and vegetable oils), as may be seen from the table below, run again on similar lines.

	1934-38.	1948-52.	1953-55.	1948-52.	1953-55.
	Thousand tons in nuts-in-shell equivalent *			Index numbers pre-war = 100	
	Annual averages				
<i>Groundnuts</i>					
World output	9,100	9,900	10,930	109	120
World exports	3,240	1,810	2,070	56	64
Consumption in producing countries †	5,860	8,090	8,860	138	151
India: output	3,196	3,197	3,836	100	120
Exports	1,050	240	240	23	23
Domestic availability †	2,146	2,957	3,596	138	168

Source: FAO, *Yearbooks on Production and Trade*, and *Monthly Bulletins*.

* Conversion factors used are: oil equivalent = 30% of unshelled and 43% of shelled weight of groundnuts.

† Derived by subtraction.

World output in recent years is a fifth higher than in pre-war years, whereas world exports are a third below: exports have declined from 36% of total output to only 19%. In India, output has increased by 20% (as a result of partition and natural increase, the population supplied from domestic output is now somewhat lower than in pre-war years), whereas exports have fallen to only a fourth of the pre-war level.

The brief review of the four major commodities that figure in India's exports indicates that in all cases domestic supply has increased and can be increased even more over a period of time if there was an adequate demand. As far as the exports of minerals and ores (mica, manganese and iron ores) are concerned, export demand rather than ability to expand is again the decisive factor. The stagnation in the overall volume of exports (the increase in the volume of tea being offset by the decline in others) has continued despite strenuous efforts at export promotion and a preparedness to export even if it meant restrictions on, or very limited increases in, domestic supplies, thus underlining stagnant export demand as the main cause in keeping the export level stable.

Against this background, it is not surprising at all that the export expectations of the First Five-year Plan were not fulfilled. The Planning Commission had estimated that the volume of exports at the end of the Plan period would rise by 30% compared with that in 1948-49, and by 10% compared with the boom year 1950-51. In practice, the average volume of exports for the Plan period was only 10% higher as compared with 1948-49, and about 10% lower as compared with 1950-51. The increases expected in the exports of the major commodities were also not realised.

The reason for the stagnation in India's export trade may be simply expressed: India has been trying to sell more of the wrong things to the wrong places. The efforts at export promotion can no doubt be increased, and high-pressure salesmanship in export markets may increase somewhat the exports to some destinations. The main fact, nevertheless, remains that offering more of the same things to the same people, who no longer want more of them, lies at the basis of a remarkably poor performance in the export trade over the last three decades.

II. EXPORT PROSPECTS

Targets for the Second Plan Period

The export perspectives for the Second Five-year Plan (1956-60) were set out by the Planning Commission as shown in Table IV. With the poor performance of this sector during the first Plan as a background, it was to be expected that the targets during the second Plan would be modest. The value of total exports in the last year of the Plan (1960-61) is to be some 9% higher than in 1954 (a year with low exports), though not much different from the annual average for the last eight years. The annual average exports for the Second Five-year Plan period as a whole, however, are in fact expected to be some 3-4% lower than the annual average for the last eight years.

There are some interesting changes with regard to export expectations for several commodities as compared with the first Plan. Instead of hoping to raise the volume of exports of jute yarns and manufactures from 929,000 tons in 1948-49 to 1 million tons in the last year of the first Plan (1955-56), it is now recognised that it will be good if the exports during the second Plan run at an annual average of some 875,000 tons.¹

The target of raising the exports of cotton textiles to 1,000 million yards, unrealised during the first Plan, reappears as a hardy perennial for the second Plan. "Cotton textiles," wrote the Planning Commission, "is one of the oldest industries in the country, and it would be natural to expect it to play an increasing role as a foreign-exchange earner. On the other hand, the domestic demand for textiles is on the increase."² Here the Commission

¹ Planning Commission, *First Five Year Plan*, p. 458.

² Planning Commission, *Second Five Year Plan* (New Delhi, 1956), p. 97.

TABLE IV

Estimated Exports During the Second Five-year Plan, 1956-60

Items.	1954 current prices, Rs. crores.†	Annual average 2nd plan.	Last year of Plan 1960-61.	Change between 1954 and 1960-61.		Crude * indica- tions for 1975, Rs. crores† in 1955 prices.
				Rs. crores † in 1955 prices.	%	
1. <i>Items showing little change</i>						
Tea	131	127	133	2	2	150
Jute yarns and manufactures	122	122	118	-4	-3	110
Cotton yarn and manufactures	72	75	84	12	18	60
Hides, skins and leather	29	28	28	-1	-3	30
Cotton, raw and waste	19	22	22	3	17	20
Coal and coke	6	5	3	-3	-50	5
Chemicals, drugs and medicines	5	5	5	—	—	10
"Others"—(miscellaneous)	130	145	150	20	15	175*
Total above	514	529	543	29	6	550-570
2. <i>Items showing marked rise:</i>						
Vegetable oils	11	22	24	13	118	40
Tobacco	12	15	17	5	42	30
Metallic ores, scrap iron, steel. Cutlery, hardware, vehicles, electrical goods and apparatus and machinery	23 3	23 4	27 4	4 1	17 33	180-280*
Total above	49	64	72	23	47	250-350*
Total exports	563	593	615	52	9	800-900
Total imports	620	868	786	166	27	800-900
Trade balance	-57	-275	-171	-108		

Source: Planning Commission, *Second Five Year Plan* (New Delhi, 1956), pp. 97, 99.

* These are only illustrative figures for emphasising that an expansion by a third to a half in total proceeds would require very big increases in the exports of "items showing marked rise," particularly ores, etc., and the new products such as "cutlery, hardware, etc."

† Equals ten million.

was worried that rising home demand would encroach upon export availabilities. The experience of the cotton textile trade over the last half century, and of India in the seven to eight years, however, suggests that this is a narrowing market in general. A more likely probability, clearly borne out by the data for late 1957 and early 1958, is that the volume of cotton textile exports will be lower than in 1954. The actual value of textile exports may show even a greater fall owing to the decline in the unit price of exports. Instead of a rise in earnings by Rs. 12 crores, there will probably be a fall of some Rs. 10 crores or more from the earnings in 1954.

The additional proceeds of Rs. 20 crores expected from the "other" miscellaneous items are "assumed to bring in the same level of earnings as at present (1955)."¹ The level in 1955, however, was higher by Rs. 20

¹ Planning Commission, *Second Five Year Plan* (New Delhi, 1956), p. 98. One crore = ten million.

crores than in 1954. The increase expected is therefore not the result of any development, but only a statistical illusion caused by comparison with a low base year.

There has been considerable uninformed talk about the gains that may be realised from increasing the exports of the products of many of the new industries in India. The Planning Commission itself had raised hopes in this respect in the first Plan. During the second Plan period the expectations put forward by the Planning Commission are rather modest, since at present this group accounts for only $\frac{1}{2}\%$ of the total exports. As the Planning Commission expressed it:

"Mention may, however, be made of the export possibilities for some of the new industries which have developed in recent years. The hope was expressed in the first Plan that new lines of export, particularly in the light engineering field, *e.g.*, sewing machines, electric fans, cycles, etc., will assume increasing importance as the economy gets diversified. These exports have not yet reached a level where their earnings could be regarded as quantitatively significant. It will be some time before these new industries establish themselves firmly and secure sizeable export markets."¹

The change expected from this item during the second Plan is, as can be seen from the table, negligible.

Thus, there is little real basis for expecting any marked increase in the export proceeds during the second Plan. All that can be said is that they may go up a little or down a little, but certainly will not rise significantly. A rise in the volume of exports of some items may even be offset by a fall in the export price. On the basis of the experience of the first Plan, as well as from prospects outlined above for the second Plan, it is fair to conclude that India's export earnings for the decade as a whole will show little change, although the national income, assuming that the expectations of the second Plan are fulfilled, will have risen by over 45%. This conclusion only reinforces the generally stagnant character of India's export trade as discussed in the first section of this study.

Long-term Perspectives

With the imperfections inherent in the tools of economists, any attempt at looking into the future can only produce a rather faint and foggy image. This is true in general, but, given the basic direction and composition of India's exports, there are a number of fairly firm elements in the export set-up of India making it possible to hazard guesses without undue fear of being too much in the wrong. Since the Planning Commission have generally used the year 1975 for some of their projections, the same year will also be used in this study to indicate the broad lines along which the export sector may be expected to proceed (see Table IV).

¹ *Second Five Year Plan*, p. 98.

Tea. Only a few countries predominate in the imports of tea. For instance, the United Kingdom alone accounts for 88% of the total imports into all the western European countries. With the present consumption of tea at more than six cups a day¹ per person aged ten and over in the United Kingdom and Ireland, it is barely likely that imports of tea will expand much. Other countries may increase imports of tea, but tea is only a marginal drink for them. Here again, severe competition, particularly from the new plantations in Africa, may be expected. All in all, world imports of tea may be higher by about a fifth, and it should be possible for India to maintain the present volume of exports, and maybe improve it somewhat.

Jute and Manufactures. The exports of jute yarn and manufactures have shown many ups and downs in the last forty years or more, but at all the peak points the export volume has remained roughly stable. This is in the main the result of a decline in the grain trade, increased use of paper, plastics and other packing materials in place of jute and growth of bulk transport and reduced handling. In addition, the competition from Pakistan is growing, and jute manufacturing has developed in a number of continental west European countries. In the light of these factors, the probabilities are very strong that the volume of jute textile exports from India will also decline during the next two decades. There may be some gain in volume if a marked fall in unit price were to make jute a more competitive commodity; but in that case the fall in unit price will only serve to offset the increases in volume, so that the net proceeds will in fact be lower.

Cotton Textiles. Looking forward to the next two decades, it is fairly certain that both the volume and unit price of cotton textile exports will fall. This has been the long-term trend and recent expansion in the use of synthetic fabrics will accelerate it. Moreover, with many newly developing countries now starting their own textile mills, it would be highly imprudent to expect a larger share in a narrowing and increasingly more competitive market.

Hides and Skins, Leather, Cotton (Raw and Waste), and Coal and Coke. These items account for about 10% of total exports. The volume of exports of the first two has fallen over a long period, and the exports of coal and coke are nominal, in that they are by and large for bunkering purposes. The exports of none of these items will increase; there may well be a decline.

"Other" Miscellaneous Items. The group "other," accounting for nearly a fourth of the export trade (see Table IV), consists of various items, such as spices, shellac and resins, mica, coir and manufactures, woollen and silk cloth, sugar, cement, cashew kernels, etc. The Planning Commission assumed that export proceeds from this group during the entire Second Five-year Plan will remain at the level in 1955. The exports of the commodities that constitute this group have either remained stagnant or fallen

¹ Assuming that 1 lb. makes 200 cups of tea.

over the last few decades. No real increases can therefore be expected from this group over the next two decades.

When all the items described above are added up, 90% of the exports from India are already accounted for. The only commodities that remain are some agricultural products (tobacco and vegetable oils), ores, metals and scrap, and products of the new industries, such as chemicals, drugs and medicines, and cutlery, hardware, vehicles, electrical goods and appliances, and machinery. Of these, the agricultural products, such as tobacco and vegetable oils, account for half of this group or 5% of the total exports and may be expected to increase at the most by around 50% by 1975.

As far as 95% of the exports from India are concerned, there is thus little likelihood of any marked increase even over the next two decades. In the past three decades the volume of exports of these items has fallen, and the trend may continue.

The prospects of growth in the other items may be considered bright in that they form the core of the dynamic or the most rapidly expanding items in world trade. The actual level of the exports, however, will largely depend on the advantages, in terms of prices, quality and terms that Indian products will offer in the world markets, which can only be expected to become more and more intensely competitive.

How hard is going to be the task of export expansion in India may be arithmetically illustrated. In order to obtain an increment of one-third in total export proceeds, it will be necessary to expand the exports of the dynamic commodities, now accounting for 5% of the total, by nearly seven times. An expansion of the total by half would require more than a tenfold increase in the dynamic sector. These are large magnitudes indeed! They will require a basic change in the commodity composition of the export trade. The dynamic items will have to rise from 5% to between a fourth and a third of the total exports to attain an increment of a third to a half in India's export earnings over the next twenty years.

The difficult but very essential task of expanding foreign trade may also be examined from the angle of the direction of the trade. The basic feature of India's trade relations with its traditional trade partners, the private-enterprise economies of the western world, has been stagnation or decline. This was also true of the centrally planned economies, but for different reasons (see Table II). The stagnation resulted from the absence of trade and other contacts, so that these markets cannot be considered saturated for India's traditional export items. In the last two or three years India's exports to these areas have expanded two to three times, although their share is still only 6% of the total.

Here the possibilities of expansion, it would seem, are not closed in the economic sense, that is, simply not needing more of the products that India usually offers. The expansion of these economies over the last few decades has proceeded without recourse to the materials that India usually exported

to the western economies, and it may well be that these materials, so far not available on the markets of the centrally planned economies, will command a marked cost advantage over the substitutes now in use there. The barriers against increased trade are thus non-economic in character. If they can be overcome, this will open up the prospects of expanding the exports of the non-dynamic commodities. In that case, India's exports can grow immediately without waiting for a thorough diversification of the commodity structure of exports. In view of the low share of the centrally planned economies in India's exports, it should be emphasised that the expansion would have to be in terms not of a few per cent but of a few times over the present magnitudes of the total exports to them.

This rather long discussion may be briefly summarised thus: even a modest increase of a third to a half in the export proceeds over the next two decades would require a big change both in the direction and in the commodity structure of exports. Without such changes, Indian exports can only be expected to drift along, as they have done in the past, a downward curve, despite the increasing number of expert bodies and export promotion councils that may be appointed to reverse it. It is better part of wisdom to accept this unwelcome and brutal fact and to prepare to live with it.

III. EXPORT PROSPECTS AND ECONOMIC GROWTH

The development of the export trade affects the Indian economy in two ways: one direct and the other indirect. In the first place, growing export demand means a greater scope for the expansion in the production of the commodities involved. The increased output of these commodities will be reflected in the overall growth of the economy. Since most of the commodities exported from India are unlikely to show marked increases over the next two decades, the expansion of their output will be limited, and to that extent the overall rate of growth will also be depressed.

The lagging behind of output in the export sector would not be a matter for much concern as long as the rate of growth of output in the other sectors of the economy could be raised sufficiently to offset the slow development in the export sector. It is in this field, however, that developments in the export trade can be expected to exercise a decisive influence on the growth of the Indian economy by affecting the volume and the rate of expansion of import capacity. Since India depends on imported capital equipment for more than half of the total equipment used in investments in the country, the slow expansion in export proceeds, and hence import capacity, places a serious limitation upon increasing the imports of capital-goods. The volume and the rates of growth of investment, and therefore real income, in the country are thus inhibited.

The points raised above have an important bearing on the economic development of the pre-industrial countries in general. During the last decade considerable attention has been devoted to finding a solution to the

widening gap in the levels of living between the industrial and the pre-industrial countries. Reversing this process has now been accepted as an essential ingredient in the work of all the international organisations. The gap, however, has continued to widen in recent years,¹ suggesting that, despite the widespread recognition given to the need for narrowing international disparities in living, the analytical tools used to approach this problem perhaps need much refashioning.

Simply stated, to initiate the process of narrowing the gap requires that the rate of growth of real product in the pre-industrial countries has to be higher than in the industrial countries and the rate of growth of investment and, therefore, of capital-goods supplies has to be substantially higher still. Since, under the present international division of labour, capital-goods are almost exclusively imported from the industrial countries, expanding the supplies of these goods means that import capacity (*i.e.*, export proceeds) has to increase correspondingly. The growth of import capacity, however, is dependent not upon the need for enlarging the supplies of capital-goods for development but upon the import demand of the industrial countries, which has in general tended to grow at a rate lower than that of the real product in industrial countries. Thus the needs of capital-goods imports outrun by far the supply potential of capital-goods as a result of the much slower growth of import capacity.

Consequently, whenever a pre-industrial country embarks upon a determined drive to expand investments, it is soon faced with a serious balance-of-payments crisis. The pace falters, the drive slackens and the will evaporates in a frustrating stagnation. The reality of the trade relationship between the industrial and the pre-industrial countries and the present international division of labour which it reflects, thus act as a built-in depressor leading to a continuous widening rather than a narrowing of the gap. Admittedly, the analysis of the trade flows here is a very simplified, and an incomplete, version of the causal mechanism which lies at the root of the widening gap; problems such as domestic mobilisation of resources and capital exports from the industrial countries are kept out of consideration. Even in its bare essentials, however, it would seem to have more to commend itself as a much closer approximation to reality than the postulates of the more sophisticated theory of international trade.² In the quest for unravelling the complex, the simple and obvious is too often forgotten.

The basic argument of the preceding paragraphs may be understood

¹ See United Nations, *World Economic Survey 1955*, p. 5 and *Economic Survey of Europe in 1958*, Chap. V, p. 5.

² Folke Hilgerdt, well known for his contributions to the present ideas about the "network of international trade," noted this in a recent paper. "The fact," he wrote, "that many underdeveloped countries do not derive the advantages from modern transportation and commerce that theory seems to demand is one of the most pertinent facts in the present international situation and cannot be easily dismissed." Paper at the *World Population Conference*, Rome, 1954. Cited by Gunnar Myrdal in *Economic Theory and Underdeveloped Regions* (Bombay, 1958), p. 159.

more clearly from Table V, where illustrative projections for various sectors of the Indian economy are shown. By assuming certain capital/output ratios, the Planning Commission estimated that in order to increase real income by 150% by 1975-76, the volume of investment will have to be raised by 500%. According to the indications in Section II of this study however, the volume of exports, and hence of import capacity, may not be expected to rise by more than 33-50%; that is, the expansion in the import capacity will be lower than in the real income and much lower than in the investments. Imports, assumed to equal export proceeds, can thus rise by no more than a third to a half.¹ Expressed in other words, the changes in export proceeds (and import capacity) and investments as a proportion of

TABLE V
Illustrative Example of the Implications of Export Prospects for India

	Actual.	Plan.	Percentage change between 1955-56 and 1975-76.
	1955-56.	1975-76.	
	Billions of Rupees in 1952-53 prices.		
National income	108.0	273.0	150
Exports	6.0	8.0-9.0	33-50
Imports	7.5	8.0-9.0	10-20
Net investment	8.0	47.0	500
of which			
Wages and salaries *	3.0	19.0	500
Capital goods *	5.0	28.0	500
of which			
Domestic capital goods out- put	2.0	23.0	1,100
Imported capital goods † .	3.0	5.0	66
	Index numbers.		
Industrial production ‡	100	650	550
	Percentages of national income.		
Exports	6	3-4	
Imports	7	3-4	
Investment	7	17	

Note. The figures are rounded to the nearest five or ten. The estimates of national income and net investment in 1975-76 are taken from the Planning Commission, *Second Five Year Plan* (New Delhi, 1956), p. 11.

* Net investment in 1955-56 broken down into the following rough proportions: wages and salaries about two-fifths, domestic capital-goods one-fourth and imported capital equipment one third. 1975-76 estimates based on maintaining the ratio of wage costs at two-fifths of total investments.

† 40% of total imports in 1955-56 and assumed to be at the most 60% of imports in 1975-76

‡ Assuming an increase of 60% during each plan period.

national income will be in inverse directions; export earnings will fall from 6% of national income in 1955-56 to only 3-4% in 1975-76, whereas investment will rise during this period from about 7 to 17% of a much larger volume of national income.

¹ In the 'sixties, the burden of repayment of loans and interest alone may be expected to be as high as a fifth of the export proceeds. Imports will in fact have to be correspondingly lower.

By continued and strict import controls, it may be possible to raise the share of capital goods imports in total imports from 40% at present to between 50 and 60% by 1975-76. In view of the fact that the growth of industries will also necessitate enlarged imports of essential raw materials and fuel oils, this can hardly be considered an easy task, though it may have to be done. It will then be possible to raise the volume of imported capital goods faster than total imports—say by about two-thirds. But the volume of investment and, assuming a constant proportion of labour costs and capital goods in investment, the availability of capital-goods has to increase by 500% to attain a growth of 150% in real income. As stated above, the slow development of the exports will thus act as a built-in depressor upon economic growth.

Obviously, if economic development is to be assured, it will be necessary to raise the domestic output of the capital-goods sufficiently fast to supply all the capital-goods that are needed for net investment. As can be seen from Table V, the present domestic supply of capital-goods will have to be raised more than eleven times, or doubled every five years.¹ Since total industrial output may not rise by more than 50-60% every five years, the output of consumer goods will obviously increase even less rapidly. The basic parameters in the example may be varied to suit somewhat different assumptions. But the central implication of the slow growth of India's export trade will still remain that the domestic output of capital-goods will have to be raised much faster than that of consumer goods; otherwise there will not be an adequate volume of capital-goods forthcoming for investments needed to assure economic growth. Many pre-industrial countries, with export prospects not altogether different from India's, will be faced with a similar situation.

In the light of the foregoing, it would seem that the whole question of the development of capital versus consumer goods industries has been discussed without serious reference to the long-term economic realities in India and other pre-industrial countries. The enthusiasts for alleged benefits of the present international division of labour have in the main concentrated on the cost advantages that follow from continuing to export agricultural raw materials and finished products based thereupon and to import capital equipment. These are often reinforced by suggesting that the investments should be made preferably in the consumer-goods industries, which require less capital and are quick-maturing, rather than in the capital-goods industries; and this would lead to a faster growth. What is assumed, of course, is that the export trade of a pre-industrial country would invariably be expanding fast enough to provide for the capital-goods imports needed for

¹ The actual requirements will be even higher because with the cumulative expansion of the stock of the capital, the proportion of replacement investments in total gross investments will also rise. See E. D. Domar, "Depreciation, Replacement and Growth," *ECONOMIC JOURNAL*, March 1953.

the rapidly rising volume of investments. In reality, however, this assumption is valid for a few petroleum-producing countries only, which account for less than 5% of the population of the pre-industrial world. For most of the other pre-industrial countries, however, it will be necessary to break the stranglehold of the slowly rising export proceeds (and hence ability to import capital-goods) upon economic growth by a sharply accelerated rate of domestic capital-goods output; the situation will be substantially different if capital transfers on a *very* large scale will be available.

The protagonists of the heavy industries, on the other hand, have often used the crude but telling argument: India has mountains of the highest-grade iron ore, so why not smelt iron and steel and fabricate it? Some of the left-wing economists have cited the experience of the Soviet Union and other centrally planned economies in support of the priority for capital-goods industries; the association of an ideology with an argument made it easy for the non-believers to reject it.

Apart from considerations of ideology or of theoretical elegance, however, the inevitable implication of the stagnation of exports and hence import capacity is that an adequate flow of capital-goods for investments cannot be assured without a much faster development of the heavy industries than any other sector of the economy for some years to come. The import policy will, therefore, have to be strictly and rigorously selective, not just encouraging all capital-goods imports as against consumer goods, but even within capital-goods imports giving the highest priority to the imports of the capital-goods that produce capital-goods. This generalisation also applies to those pre-industrial countries which cannot expect a rapid enough increase in their export proceeds to provide adequately for the needed capital-goods imports. In the process, the present international division of labour will be altered. India, a primary producing country, will reduce rapidly its dependence on capital-goods imports for economic growth in the same manner as the industrialised countries have reduced their dependence on the exports of the agricultural raw material producing countries.

Geneva.

SURENDRA J. PATEL

THE TERMS OF TRADE OF MANUFACTURES

I

MOVEMENTS in the terms of trade of manufactures reflect changes in the scarcity of manufactures in the world economy. W. A. Lewis has shown¹ how the terms of trade normally offset changes in the relative volumes of manufactures trade and other trade, so that the value ratio of manufactures trade to total trade remained fairly constant within two points of 37% from the 1880s to 1950. This held until 1951. If 1950-51 is compared with 1930-38 the volume of manufactures trade was 27% higher, but this was "corrected" by a 30% deterioration in the terms of trade of manufactures—so that the value ratio of manufactures trade to total trade fell by 3% of 37·7 of the total in the thirties to 36·8% of the total in 1950-51. Although values slightly over-corrected volumes, the movement in values was in the expected direction, and the value ratio of manufactures stayed in the expected range. After 1951, however, the accustomed relationship no longer applies. A comparison of 1952-57 with 1950-51 shows the ratio of the volume of manufactures trade to the volume of total trade 7% higher, yet the terms of trade improved by 12%. The value ratio of manufactures trade to total trade rose by 20% of 36·8% of the total in 1950-51 to 44·3% of the total in 1952-57.²

Were the terms of trade of manufactures in 1952-57 by 20% too favourable? If so, the terms of trade at the time of the Korean War were "normal" and the subsequent improvement "abnormal." Or is it more relevant to note that the terms of trade of 1952-57 were at about the same level as prevailed before 1914, so that the Korean terms of trade were "abnormal" and 1952-57 "normal"?³ Is the volume of manufactures

¹ W. A. Lewis, "World Production, Prices and Trade," *The Manchester School*, May 1952, pp. 106-9.

² In Lewis' statistics, *op. cit.*, the manufactures ratio never exceeded 41½ and exceeded 40 only three times. Estimates for the 1950s derived from United Nations statistics and linked to Lewis' result for 1950:

result for 1950.	1950.	1951.	1952.	1953.	1954.	1955.	1956.	1957.	
Manufactures Ratio at constant terms of trade, %	36·4	38·7	39·3	40·2	40·2	39·8	40·5	40·8	
Terms of Trade (1950 = 100) . .	100	104	91	90	96	91	89	87	
Manufactures Ratio at current terms of trade (Lewis Ratio), %	36·4	37·2	43·2	44·7	41·9	43·7	45·5	46·9	
^a "Excess" of the volume ratio of manufactures trade to total trade in 1950-57 over:									
1881-89.	1890-99.	1900-9.	1910-13.	1921-29.	1930-38.	1950-51.	1952-57.	1957.	
%	15	22	21	19	23	30	5	-1	-3

Derived from F. Hilgerdt's revised series: "The Development and Composition of World Trade, 1871-1938" and United Nations. Linked with aid of Table VI in H. Tyszinski: "World Trade in Manufactured Commodities," *The Manchester School*, September 1951.

trade untenably high when the volume share of manufactures trade to total trade is between 15 and 30% higher than in any decade from the 1880s to the 1930s? It was untenable without unfavourable price movements until 1951, but subsequent increases in the ratio of manufactures trade to total trade were supported by a favourable trend in prices. What special concatenation of events enabled manufactures after 1951 to lead in world trade by volume *and* unit value? ¹

II

An attempt to answer such questions requires in the first place an examination of the annual movement in the terms of trade. This reveals certain relationships that may assist in an assessment of future prospects. The annual movement in the terms of trade is best regarded as a marginal movement. If the quantities that determine the relative scarcity of manufactures are constant, relative values are constant; if these quantities vary at unequal rates, relative values change. The relative marginal changes in the relevant quantities determine, or are determined by, the direction of the terms of trade. The relevant quantities are the volume of manufactures trade, the volume of total trade and the volume of industrial production. It was possible to relate the first two only and get an adequate picture of the probable behaviour of the terms of trade until 1951, but this is no longer possible. It is not claimed that the relation between changes in the three quantities mentioned will suffice to explain the probable course of the terms of trade for all time, but it does for the 1950s. Nor do these relations show by how much the terms of trade change, but they show in what direction the terms of trade are likely to move.

To avoid tedious repetitions of long phrases, the following symbols will be used:

m = the annual percentage growth over the previous year of the volume of manufactures trade;

t = the annual percentage growth over the previous year of the volume of total trade;

w = the annual percentage growth over the previous year of the volume of industrial production.

The table on p. 510 shows that the normal twentieth-century relation is for m to be greater than t , though there are some exceptions. The relation between t and w was less regular. From the relative behaviour of m , t and w it is possible to deduce the direction of the accompanying terms of trade as long as $m > t$.

The figures in the table are five-year averages. Five years is long

¹ The unfavourable movement from 1953 to 1954 (see footnote 2 on p. 507) interrupted the trend only temporarily. It is similar to jerks in the terms of trade that have sometimes occurred at this particular stage of the trade cycle, *i.e.*, well ahead of a Juglar recession (*e.g.*, from 1926 to 1927), but this cannot be proved until it is known whether the 1957–58 recession turns out to be of the past Juglar type.

enough to eliminate most purely temporary changes and short enough not to eliminate major fluctuations. The five-year periods are grouped according to the relative magnitudes of m , t and w . The table extends from 1892–96 to 1953–57, excluding war and post-war years, for which all the relevant statistics are not available on a world-wide basis. 1882–86 to 1891–95 could be included but would merely add ten more rows to section D without adding to its analytical value. Figures are shown to one decimal only for ease of reading the table. Where smaller fractions matter, their direction is indicated. This suffices for the purpose of the present analysis, where it may matter whether, say, $\frac{t}{w}$ is greater or less than 1.0 but does not matter whether it is 1.01 or 1.02. The figures are averages for five annual growth rates over each previous year; hence the relationship of m , t and w is not necessarily the same for each constituent year as in the five-year period shown.

The relation of m to t shows whether the marginal change of m and t from one period to the next has made traded manufactures scarcer or less scarce relatively to all traded goods. If $\frac{m}{t} > 1$, a 1% increase in total trade is accompanied by a greater than 1% increase in manufactures trade: manufactures are relatively more abundant in trade. Were $\frac{m}{t}$ the only relevant relationship, the terms of trade of manufactures would have deteriorated throughout most of the present century, including the 1950s.

m must also be related to w . If $\frac{m}{w} > 1$, a 1% increase in industrial production is accompanied by a greater than 1% increase in manufactures trade. In the previous case of $\frac{m}{t}$, m in t is a significant relation, since m is part of t . m in w is not a meaningful relation: m comes out of w , but the two need not necessarily move in the same direction (even if they normally do). The significant relation is w for m . If $\frac{m}{w} > 1$, the growth of industrial production does not keep pace with the growth of manufactures trades. There is greater scarcity of manufactures for trade. (Any other interpretation would imply world-wide dumping of manufactures. This is inconceivable in prosperity. Even in depression this is not likely to be practised on a world-wide scale and, if it were attempted, it would not be tolerated.)

If then $\frac{m}{w} > \frac{m}{t} > 1$, the enhanced scarcity of manufactures *for* trade outweighs the enhanced abundance of manufactures *in* trade. The net scarcity of manufactures has increased. The terms of trade of manufactures can improve because $\frac{m}{w} > 1$ despite $\frac{m}{t} > 1$, if $\frac{m}{w} > \frac{m}{t}$ (or $t > w$).

TABLE I

Five-year Averages of Annual Percentage Changes in the Terms of Trade of Manufactures, and in Growth Rates and Relative Growth Rates of Volumes of Manufactures Trade (m), Total Trade (t) and Industrial Production (w)

(Note regarding signs: terms of trade: + = deterioration
- = improvement
growth rates: + = increase
- = decrease
relative growth rates: modulus)

	Years.	Terms of trade.	Growth rates.			Relative growth rates.		
			m.	t.	w.	$\left \frac{m}{w}\right $	$\left \frac{m}{t}\right $	$\left \frac{t}{w}\right $
Section A(1) where $ m > t > w $ and $\left \frac{m}{w}\right > \left \frac{m}{t}\right > \left \frac{t}{w}\right > 1$	1892-1896	-1.2	+5.5	+3.1	+2.5	2.2	1.8	1.2
	1901-1905	-1.0	+5.7	+5.1	+5.0	1.1+	1.1	1.0+
	1904-1908	-0.5	+4.4	+2.7	+2.0	2.2	1.6	1.4
	1925-1929	-0.7	+6.7	+6.0	+5.9	1.1+	1.1	1.0+
	1930-1934	-3.8	-7.1	-4.5	-3.4	2.1	1.6	1.3
	1931-1935	-1.2	-3.8	-2.1	+1.5*	2.5	1.8	1.4
	1951-1955	-1.6	+8.2	+6.1	+6.0	1.4	1.3	1.0+
	1952-1956	-2.6	+6.4	+5.5	+4.8	1.3	1.2	1.1
Section A(2) where $ m > t > w $ and $\left \frac{m}{w}\right > \left \frac{t}{w}\right > \left \frac{m}{t}\right > 1$	1893-1897	-0.2	+4.2	+4.0	+3.3	1.3	1.1-	1.2
	1908-1912	+0.4	+4.3	+4.2	+3.7	1.2	1.0+	1.1
	1923-1927	+4.4	+7.6	+7.4	+5.8	1.3	1.0+	1.3-
	1924-1928	-0.3	+6.7	+6.6	+5.6	1.2	1.0+	1.2-
	1926-1930	-3.3	+3.8	+2.8	+1.8	2.1	1.4	1.6
	1953-1957	-1.0	+7.3	+6.7	+5.0	1.5	1.1	1.3
Section B where $ m > w > t $ and $\left \frac{m}{t}\right > \left \frac{m}{w}\right > 1 > \left \frac{t}{w}\right $	1902-1906	+0.2	+5.9	+4.6	+5.2	1.1	1.3	0.9
	1903-1907	+0.5	+5.0	+3.8	+4.2	1.2	1.3	0.9
	1922-1926	+5.5	+9.9	+8.6	+8.9	1.1	1.2	1.0-
	1928-1932	-6.8	-7.0	-3.5	-5.9	1.2	2.0	0.6
	1929-1933	-5.5	-7.9	-3.8	-4.1	1.9	2.1	0.9
Section C where $ w > m > t $ and $\left \frac{m}{t}\right > 1 > \left \frac{m}{w}\right > \left \frac{t}{w}\right $	1895-1899	+0.4	+5.3	+3.9	+6.7	0.8	1.4	0.6
	1896-1900	+1.8	+2.3	+2.1	+4.7	0.5	1.1	0.4
	1898-1902	+0.9	+3.0	+2.4	+5.7	0.5	1.3	0.4
	1905-1909	+0.2	+3.9	+3.9	+4.0	1.0-	1.1	0.9
	1909-1913	+1.5	+5.8	+5.0	+6.4	0.9	1.2	0.8
	1932-1936	+1.9	+0.6	+0.5	+6.8	0.1	1.2	0.1
	1933-1937	+4.6	+8.0	+5.5	+11.5	0.7	1.5	0.5
	1934-1938	+2.5	+6.7	+3.6	+6.9	1.0-	1.9	0.5
Section D where $ w > t > m $ and relative growth rates below 1 and no consistent relation between them	1894-1898	-0.4	+4.3	+4.7	+5.8	0.7	0.9	0.8
	1897-1901	+1.3	+1.4	+1.7	+5.1	0.3	0.8	0.3
	1899-1903	+0.6	+2.5	+2.7	+4.4	0.6	0.9	0.6
	1900-1904	-0.1	+2.0	+2.8	+2.9	0.7-	0.7	1.0-
	1906-1910	+0.6	+2.5	+3.2	+3.2	0.8	0.8	1.0
	1927-1931	-4.9	+0.4	+1.1	-1.6	0.3	0.4	0.7
Section E where $ t > m > w $	1907-1911	+0.2	+3.1	+3.5	+2.6	1.2	0.9	1.3

* w was positive, but a growth rate of +1.5% is a depressed rate. Even without the modulus it would appear legitimate to read from left to right and compare the three depression rates, so that $m > t > w$ holds in order of depression.

Sources: 1950s: United Nations.

1890s to 1930s: Terms of Trade: W. A. Lewis, *op. cit.*, Table II.
Manufactures Trade and Total Trade: F. Hilgerdt, *op. cit.*
Industrial Production: W. A. Lewis, *op. cit.*, Table I, series A.

If $\frac{m}{w} > \frac{m}{t} > 1$ states the basic condition for an improvement in the terms of trade of manufactures when $m > t$, it is by itself no guarantee that this will be so. The result is certain only if $\frac{t}{w}$ stands in appropriate relation to $\frac{m}{w}$ and $\frac{m}{t}$. The two A sections of the table depict fourteen cases where $|m| > |t| > |w|$. In twelve of these (all A(1) and some A(2)) the terms of trade of manufactures improved, but the result was certain only in the nine A(1) cases where $\left|\frac{m}{w}\right| > \left|\frac{m}{t}\right| > \left|\frac{t}{w}\right| > 1$.

Now, if $m > t > w$, then $\frac{m}{w} > \frac{m}{t} > 1$ and there is a possibility of a favourable movement in the terms of trade of manufactures. Alternatively, if $m > t > w$, $\frac{m}{w} \div \frac{m}{t} = \frac{t}{w} > 1$. This is a truism, but points to $\frac{t}{w}$ as balancing factor. $\frac{t}{w}$ is the growth of total trade relatively to the growth of industrial production. It consists of $\frac{m}{w}$, which is industrialists' enhanced offer of manufactures for trade, and of $\frac{\text{non-}m \text{ part of } t}{w}$, which is the enhanced volume of non- m trade relatively to industrial production. $\frac{t}{w}$ may be called the changed industrial offer for trade. As long as $\frac{t}{w}$ is not greater than $\frac{m}{w}$, the rise in industrialists' offer for trade does not offset the enhanced net scarcity of manufactures ($\frac{m}{w} > \frac{m}{t}$). If $\frac{m}{w} > \frac{m}{t} > \frac{t}{w} > 1$, then the conditions are set for manufactures to lead by unit-value as well as by volume.

In the A(1) cases where $\left|\frac{m}{w}\right| > \left|\frac{m}{t}\right| > \left|\frac{t}{w}\right| > 1$ applies, $(m - w) > (m - t) > (t - w) > 1$ applies as well; and if $(m - t) > (t - w)$, then $(m - w) > 2(t - w)$. The excess of m over w (which makes $\frac{m}{w} > 1$ and causes the enhanced scarcity of manufactures for trade) exceeds $(m - t)$ (the cause of the enhanced abundance of manufactures in trade) and $(m - t)$ exceeds $(t - w)$ (the cause of the enhanced industrial offer for trade); and the enhanced scarcity of manufactures for trade is at least twice as great as the enhanced industrial offer for trade. The enhanced net scarcity of manufactures—which can be written $(m - w) > (m - t)$ —is assured by the condition above which implies $(m - w) > 2(t - w)$.

When $t > w$, development is outward looking; when $m > t$ as well, the outward looking development is especially marked in manufactures.

There is a trade drive, and in particular a manufactures trade drive. To ensure its success—in the sense that it is supported by favourable terms of trade and not frustrated by unfavourable terms of trade—it is not enough for $m > t > w$, but the difference between m and t must be greater than the difference between t and w . As long as m is sufficiently larger than t and w , t and w can be nearly equal (and $\frac{m}{w} \simeq \frac{m}{t} > \frac{t}{w} > 1$, as happened in 1901–5, 1925–29 and 1951–55). The net scarcity of manufactures may be only slightly raised. It is nonetheless effective as long as $\frac{m}{w}$ is a fraction above $\frac{m}{t}$, and $\frac{t}{w}$ is below $\frac{m}{t}$.

The A(1) conditions prevailed in 1951–55 and 1952–56. There are six precedents. Two are negative ones: m fell more than t , and t fell more than w (1930–34, 1931–35). Another two occurred when t and w were rather close (1901–5, 1925–29) and were soon replaced by inward-looking development ($w > t$). The remaining two occurred when w was rather low (1892–96, 1904–8) and were not maintained when w recovered. It is possible that manufactures will continue to succeed in the 1950s where past success was rare and short-lived, but this cannot be taken for granted: m , t and w influence each other, and a rise in any one of them can induce changes in the others and so transform an A(1) situation into any of the others depicted in the table. 1953–57 was still an A case, but only an A(2) one.

If m rises, t is raised but less than proportionately since m is part of t but not the whole of t . The rise of $\frac{m}{t}$ is damped down but not offset. If t rises, $\frac{t}{w}$ is raised, so that a rise in $\frac{m}{t}$ implies a lesser rise in $\frac{t}{w}$. A rise in m also raises $\frac{m}{w}$. But while a rise in m must raise t in appropriate ratio (m being part of t), it is uncertain how w will be affected. True, traded manufactures are taken from industrial output, but the increase in m may be compensated by a lesser growth of domestic sales of manufactures. Complete compensation seems unlikely, since a rise in m makes industrial production more profitable. m is more likely to induce some rise in w , but a less than proportionate one since only part of industrial output is affected by m . Moreover, the m -induced rise in w is likely to be less than the m -induced rise in t , since the share of industrial production devoted to international trade is less than the share of manufactures in international trade. So far, the m -induced changes in t and w reinforce rather than weaken the excess of $\frac{m}{w}$ over $\frac{m}{t}$. But the rise in w also induces a rise in t . It may or may not raise the m part of t , and once more reinforce the lead of m . It is bound to raise the non- m part of t , since more industrial production

requires more primary products. A rise in w then raises t relatively to m , again less than proportionately if only the non- m part of t is markedly raised. Thus m raises the m part of t and w raises the non- m part of t . As long as the combined effect of m and w on t does not raise $\frac{t}{w}$ above $\frac{m}{t}$

situation A(1) prevails. But once $\frac{t}{w}$ becomes greater than $\frac{m}{t}$, the manufactures trade drive is no longer certain to be supported by a favourable movement in the terms of trade of manufactures.

With $\frac{m}{w} > \frac{t}{w} > \frac{m}{t} > 1$, there is still enhanced net scarcity of manufactures ($\frac{m}{w} > \frac{m}{t} > 1$), but this is now partly offset by $\frac{t}{w}$ being relatively high.

If the excess of $\frac{t}{w}$ over $\frac{m}{t}$ is slight, the effect is negligible. In all but one of the A(2) cases ($t - w > m - t$) applied. Hence, $(m - w) > 2(t - w)$ did not apply and the net scarcity of manufactures was not assured. In 1926–30, $(m - w) > (m - t) = (t - w)$ applied and $(m - w)$ was still twice as large as $(t - w)$. The net scarcity of manufactures was hardly offset. In 1953–57 the ordinary A(2) relation $\frac{m}{w} > \frac{t}{w} > \frac{m}{t}$ applied and $(m - w) > (t - w) > (m - t)$ applied as well, but again the excess of $\frac{m}{w}$ over $\frac{m}{t}$ was substantially higher than the excess of $\frac{t}{w}$ over $\frac{m}{t}$. The net scarcity of manufactures was somewhat offset, but still substantial. The results for 1926–30 and 1953–57 resemble A(1) cases. In all other A(2) cases, m and t were rather close. If $(t - w)$ exceeds $(m - t)$ and $(m - t)$ is small, $\frac{t}{w}$ approaches $\frac{m}{w}$ or t approaches m . There is no clear lead of manufactures. $\frac{m}{t} \simeq 1$ means the enhanced abundance of manufactures

in trade is negligible, and $\frac{m}{w} \simeq \frac{t}{w}$ that the enhanced scarcity of manufactures for trade roughly equals the enhanced industrial offer for trade. In general, when $\frac{t}{w}$ exceeds $\frac{m}{t}$, total trade has become high relatively to industrial production, and this reinforces an abundance of traded goods (including manufactures) rather than their scarcity. But $\frac{t}{w}$ does not exceed $\frac{m}{w}$, so that the enhanced industrial trade offer does not entirely outweigh the enhanced scarcity of manufactures. Normally, when $\frac{t}{w}$ approaches $\frac{m}{w}$, no sharp movement in the terms of trade is to be expected. This held in three

of the four clear A(2) cases (where $t \simeq m$), but not for 1923–27. For 1923–27, the average annual movement in the terms of trade is influenced by an abnormally sharp movement in 1923 (+19.3%), which was probably due to special post-war problems. The average movement in the terms of trade in 1924–27 was +0.7% (which was moderate by inter-war standards: the average for the inter-war periods shown in the table was 3.5%, disregarding sign).

Section A(1) of the table depicts clear cases of manufactures trade drives. The A(2) cases are periods when the lead of manufactures was less marked. In sections B, C and D, $w > t$: industrial production grew faster than trade. In B, C and D cases development is inward-looking. The balancing factor $\frac{t}{w} < 1$. In section B $|m| > |w| > |t|$. $\frac{m}{w} > 1$, so there is

scarcity of manufactures for trade. But this is outweighed where $\frac{m}{t} > \frac{m}{w}$, which implies net abundance of manufactures. In B cases, where all three growth rates are positive, the terms of trade of manufactures must deteriorate.

In a B situation the terms of trade of manufactures can improve only in severe depression when all three growth rates are negative. Then the fall in the abundance of manufactures in trade exceeds the fall of the scarcity of manufactures for trade. With $\frac{t}{w} < 1$, the general abundance of trade relatively to industrial production also falls. A net scarcity of manufactures is created through a fall in abundance. Compare this with the A(1) depression cases where $-\frac{m}{w} > -\frac{m}{t} > -\frac{t}{w} > -1$ and the net scarcity of manufactures is enhanced relatively to the industrial trade offer.

In the C and D cases, $w > m$. In section C, $w > m > t$ so that $\frac{m}{w}$ and $\frac{t}{w}$ are less than 1 and $\frac{m}{t} > 1$. There is an excess supply of manufactures: they are relatively abundant in and for trade. The terms of trade of manufactures deteriorate.

In section D m is behind both t and w . All three fractions are less than 1. There is no consistent relation between the relative growth rates and the terms of trade. The result is indeterminate. Should a $w > t > m$ situation arise again, information will be needed about the growth of world primary production as well as of primary product trade, so that the scarcity of primary products for trade can be estimated as well as the scarcity of primary products in trade. When primary products lead in trade, such information is needed for determining the direction of the accompanying terms of trade.

Section E shows one instance where $t > m > w$. It was a transition period from D to A(2). With $t > m$, the result is again indeterminate. $t > w > m$ did not occur. t -leads are freak positions as long as the terms

of trade of manufactures are determined by what happens in manufacturing production and in manufactures trade. For most of the twentieth century the choice has been between outward-looking industrial development (lead of m) and inward-looking development (lead of w). Nothing else is likely to become "normal" as long as the bulk of world trade is with or between industrial countries.

The favourable conditions for manufactures in the 1950s can last as long as trade continues to grow faster than industrial production and manufactures trade grows even more relatively to total trade. It did not last in the 1920s because t became too large relatively to m and w . It did not last in the 1890s, the 1900s and the 1930s because m or t or both became too low relatively to w or w became too high relatively to the other two.

III

The outward-looking development, with manufactures in the lead, enabled the favourable marginal movement in the terms of trade of manufactures in most of the 1950s. The average level of the terms of trade of manufactures in 1950–57 was about the same as before 1914. If 1913 = 100, the average terms of trade of manufactures in 1880–1913 were 96 (maximum 104, minimum 90) and in 1950–57 again 96 (maximum 106, minimum 89). In the 1930s the marginal movement was unfavourable to manufactures in recovery, but the average level was favourable (74). If it is the function of an appropriate marginal movement in the terms of trade to restore a given equilibrium relationship between the unit-value of traded manufactures and other traded goods, the marginal movement in the terms of trade of manufactures in the 1950s might be regarded as an incomplete fluctuation around such an equilibrium level. Perhaps "96" is the equilibrium unit-value relationship between traded manufactures and other traded goods when w fluctuates around 4% or more. (4% is the average for 1881–1913; 5% applies to the 1950s; in the inter-war years it averaged only 3%.) If so, the normal equilibrating movement of m , t and w at full or near-full employment may come into play and offset the very favourable movement in the terms of trade of manufactures after 1951. Korean or near-Korean terms of trade would be an only slightly exaggerated unfavourable end of a normal fluctuation around 96, while the terms of trade of 1957 (89) would be at or near the favourable end of such fluctuation. Were the value ratio of manufactures in total trade (the Lewis ratio) still the same constant as in the past, it would be tempting to argue that with full employment the terms of trade fluctuate around "96."

If there is an equilibrium level of the terms of trade when there is industrial full employment, and an appropriately lower one when there is

industrial unemployment, the marginal terms of trade must cancel out over time and the long-run $\frac{m}{t}$ ¹ must equal 1. 1882–1913 fulfils these requirements to near-perfection: the average marginal movement of the terms of trade was –0.04% and the long-run $\frac{m}{t}$ was 1.04%. For 1922–38 the figures are: average marginal terms of trade –0.6%, long-run $\frac{m}{t}$ 0.9%. These periods are not strictly comparable. They indicate, however, that in the very long run the marginal terms of trade tended to cancel out in the past. There was no significant long-run bias in the marginal terms of trade before 1914. In the inter-war years there was some bias in favour of manufactures, but having regard to the long-run $\frac{m}{t}$ being below 1, this just helped to keep the Lewis ratio fairly constant. The Lewis ratio was never over 39.2% in any five-year period before the last war,² but came to 40.7% in 1950–54 and then rose to 44.5% in 1953–57. With the volume ratio of manufactures now higher than at any time in the recorded past, this may yet mean that the value ratio (Lewis ratio) will settle down at some higher level than in the past and the marginal terms of trade will fluctuate around some equilibrium level to maintain it.

This is possible, but by no means certain. Conditions may become less favourable to manufactures and the old Lewis ratio may be restored after a drastic fall in manufactures values. Or, the favourable conditions of the 1950s may become “normal” for some time and a new Lewis ratio far off. The second part of this paper has shown how manufactures can lead in trade by volume and unit-value if $m > t > w$ and $(m - w) > 2(t - w)$, and that m , t and w interact. As long as the offsetting forces are strong enough always to restore the old value-relationships, it is possible to think of an equilibrium value ratio of manufactures in trade and of equilibrium terms of trade. If there is full employment, w is high. If w is high relatively to t but below t , and m is appropriately higher still, then there is a movement towards a higher Lewis ratio supported by a favourable movement in the terms of trade of manufactures. The limit is set by the condition that industrial activity must not rise faster than trade ($t > w$ must continue). If industrial activity rises faster than trade, the terms of trade of manufactures deteriorate: the terms of trade act once more as stabiliser of the Lewis ratio. Full employment was an important factor in producing the favourable circumstances for manufactures in the 1950s. It was not the

¹ The long-run $\frac{m}{t}$ is the total of the five-year averages of m , having regard to sign, divided by the total of the five-year averages of t , having regard to sign. It is not comparable with the total of the $\frac{m}{t}$ s in the table, where signs are ignored.

² *Op. cit.*

only factor. There was full employment in the 1940s, but the terms of trade of manufactures deteriorated. Full employment can ensure that w is high relatively to t , but it cannot by itself ensure that $t > w$ nor that $(m - t) > (t - w)$. Manufactures did well in the 1950s because of a combination of full employment and improved trading conditions.

In the 1950s $\frac{t}{w} > 1$ prevailed. $\frac{t}{w} > 1$ means an enhanced industrial offer for trade and outward-looking development in the industrial world. In other words, industrialists' offer for more international transactions rises relatively to industrialists' offer for more domestic transactions. True, w does not measure the increase in all domestic trade which depends on the national income; but w measures changes in the quantity the industrial sector of the economy can offer for domestic and international trade. As far as transactions by and with industrialists are concerned, internationally traded goods are relatively more wanted than domestically traded ones. The terms of exchange between all internationally traded goods and domestically traded goods move in favour of the internationally traded ones. (Terms of exchange, rather than terms of trade, is used to denote the exchange ratio between domestic and internationally traded goods, while terms of trade is reserved for the international terms of trade of manufactures over other goods.) International values are higher relatively to domestic values than they otherwise would be.

When $t = w$, industrialists are indifferent between more international trade and more domestic trade. When $t < w$, industrial development is inward-looking. Industrialists prefer more domestic trade to more international trade, so that internationally traded goods lose value relatively to goods industrialists trade at home. International trade lags behind domestic industrial development and the terms of exchange move against all internationally traded goods.

Although $\frac{t}{w} > 1$ implies an improvement in the terms of exchange of internationally traded goods, it does not indicate whether the terms of trade will move in favour of manufactures or other goods. As has been shown before, manufactures are certain to gain only if m is greater relatively to t than t is relatively to w .

If $m > t > w$, the outward-looking development is most noticeable for manufactures, and all the more so if $(m - t) > (t - w)$. To keep m sufficiently high relatively to t and w requires increasing international specialisation. There is no lack of new ideas that can be applied to industry; and if each can concentrate on the development of his own ideas rather than duplicate others', each will reap the maximum reward. This is ancient free-trade doctrine. In the present context it means $\frac{m}{w}$ rises. Given full employment, w is high. Given freer trade in manufactures, m

risers relatively to w . When m and w rise, t rises too, but in a normal twentieth-century relation t rises by less than m . Thus full employment and freer trade together encourage the relative growth of m , t and w in a $\frac{m}{w} > \frac{m}{t} > \frac{t}{w} > 1$ relation. The rewards of full employment in the manufacturing world are reinforced by a favourable movement in the terms of trade of manufactures.

If full employment is accompanied by increasing trade restrictions, w is raised and m lowered. The terms of trade of manufactures are unlikely to move favourably for long. If industrial countries impose tariffs on manufactures, this lowers the unit values manufacturers can hope to get for their exports to industrial countries; such trade becomes less profitable and its volume falls before long. Quantitative import controls reduce volumes directly. Moreover, if manufactures are kept out of manufacturing countries the scope for specialisation is reduced and the scope for duplication is increased. More duplication means more like goods and probably more lower-value goods. For the higher skills cannot be used to maximum advantage when effort has to be applied to duplication. In any case relatively more like goods are offered for trade, and this depresses their unit-values. Once more there is less profitability in such trade, and volumes are likely to fall again. m falls and drags down t . Restrictions on manufactures trade in times of full employment move the terms of exchange against internationally traded goods, and especially against internationally traded manufactures. Only if there is a deep depression, with m , t and w negative, can the terms of trade of manufactures improve with trade restrictions increasing: for the increase in trade restrictions on manufactures depresses m even more relatively to t and w . In prosperity restrictionist manufacturing countries lower each other's potential gains. Nor do they benefit others: if the terms of trade of others improve, the terms of exchange of internationally traded goods are still kept low. In prosperity a restrictionist manufacturing world hurts others less than it hurts itself. In depression it hurts others more than itself.

If development is outward looking, specialisation can increase. Instead of duplicating each others' efforts, manufacturers can concentrate on what sells best—until such development is halted by restrictions. Now, the 1950s cannot be described as an age of free trade, but have been an age when the major industrial countries reduced trade barriers. There have been set-backs and there is still plenty of opportunity for further reductions in trade barriers. There were decades in the past when trade was freer. But from the 1880s to the 1930s there was no decade without one or more of the major industrial powers increasing obstacles to trade, especially to trade in manufactures. The 1950s are the first decade since the 1860s with a general movement towards freer trade. While past protection lowered volumes and unit values of manufactures trade, as intended, the

beginnings of its removal had the opposite effect. Exports of manufactures rose to all destinations and especially to manufacturing countries.¹ Instead of attempting to sell more like goods to the poor, manufacturing countries now offer a greater variety as well as quantity of goods to tempt the rich and the poor. Of special importance are falling trade barriers in rich manufacturing countries which presents opportunities for selling more high-value goods. Past protection contributed to keeping unit values and volumes of manufactures trade down and contributed to the constancy of the Lewis ratio. It is not suggested that it was the only cause, but it was not insignificant. Equally, the reduction in trade barriers in the 1950s was not the sole cause of the rising share of manufactures in world trade, but it was an important contributory factor.

From 1882 to 1913, industrial production grew on average by 4% a year and manufactures trade by 3%; in 1922–38 the rates were 3% and 2%; in the 1950s 5% and 7%. While it cannot be expected that the growth of manufactures trade can persistently outstrip the growth of industrial production, as long as there is scope for increasing specialisation rather than duplication of industrial effort manufactures trade might well be expected to rise faster. In any case it should not persistently fall behind. While protection is still strong, further reductions in trade barriers are possible, and this by itself will tend to raise m relatively to t and w . m -induced rises in t and w will not equilibrate the marginal terms of trade or the Lewis ratio, as long as an artificial stimulus can always be applied to m by a persistent and gradual removal of obstacles to the rise in m . By fostering the rise in m relatively to t (when $t > w$), manufacturing interests can confer on themselves the enjoyment of further improvements in the terms of trade of manufactures. By doing so, they benefit themselves most. They benefit primary producers relatively less, but need not harm them: the terms of exchange of all internationally traded goods are more favourable when industrial development is outward-looking than when it is inward-looking. Primary producers may be better off with more favourable terms of exchange and less favourable terms of trade than if it were the other way round. This applies as long as m , t and w are positive. When m , t and w are negative, m -raising by itself would bring m closer to t and w and eventually $-m$ might become less than $-t$ and $-w$. This might help to arrest an outward-looking contraction and so improve the terms of exchange, but the terms of trade of manufactures would deteriorate.

None of this implies that a movement in the terms of trade in favour of manufactures is in itself desirable. It depends on the cause. For instance, if the terms of trade of manufactures were artificially improved by manufacturing countries raising tariffs on primary products, such selfish action would adversely affect the terms of exchange available to all international trading interests. Again, if manufacturing interests restrict output in

¹ See, e.g., *United Nations World Economic Survey for 1956*, p. 59.

recession to maintain their prices at artificially high levels, they force primary producers to intensify competition amongst themselves for marketing outlets at lower primary product prices, and the effect on the terms of exchange is as unfavourable as if primary products were specially taxed. All that is claimed here is that, when $m > t$, artificial restrictions on manufactures trade lead to less favourable terms of trade for manufactures and to less favourable terms of exchange for all internationally traded goods. Normally when manufactures lead in trade, primary values are only certain to rise when manufactures values have risen. The implied time-lag may move the terms of trade somewhat in favour of manufactures and yet the terms of exchange may improve for all internationally traded goods. Most primary producing countries' prosperity depends on international trade, so that they have no interest in any policies that artificially depress international values. Economic warfare amongst manufacturing countries does not help third parties.

The last problem to be considered in this paper is whether favourable terms of trade of manufactures can persist, having regard to 1953-57 being an A(2) case. To transform an A(2) position of $\frac{m}{w} > \frac{t}{w} > \frac{m}{t}$ into a safer

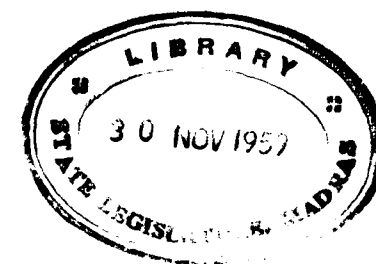
A(1) position of $\left|\frac{m}{w}\right| > \left|\frac{m}{t}\right| > \left|\frac{t}{w}\right|$ requires *either* that t be lowered relatively to m and w or that m and/or w be raised relatively to t . To lower t is an inferior solution. It would reduce $(t - w)$ and so be a step in the A(1) direction. However, the normal twentieth-century relation is $m > t$, so that a fall in t can be expected to lead to a more than proportionate fall in m and this narrows the gap between m and t . The lead of m is restored only when both have become negative. Thus a fall in t leads the $m : t$ relationship farther away from an A(1) position or into slump. There is no case here for restricting the non- m part of t , if a slump is to be avoided. It may be that t cannot remain at the fairly high level of 1953-57. If so, manufactures will fare better if such fall in t is accompanied by increase in m and/or w . Now to w . To raise w narrows the gap between w and t , and this is a move towards an A(1) position. Now a rise in w is more certain to raise t than to raise m , so that the gap between m and t narrows and the A(2) position hardens. Thus the rise in w by itself is only half a solution towards insuring expansion with favourable terms of trade for manufactures. Moreover, if w raises t less than proportionately, as is likely, w may rise above t and m and a $w > m > t$ position might ensue—in which case the terms of trade move against manufactures. It should be noted that w was relatively low in 1953-57 because of two lapses from a high level of activity in 1953 ($w = 0$) and 1957 ($w = 1.7\%$). The w of 1953-57 was not particularly low by past standards, nor was it at record heights. A somewhat higher w would not be unprecedented. Whether w can be raised or not, to raise w alone leaves the difference between m and t uncertain; moreover, w must

remain below t if the terms of trade of manufactures are to improve. To ensure this a rise in w should be supplemented by a rise in m . A higher m (with $m > t$) widens the gap between m and t and is fairly certain to raise w . It is unlikely to raise w as much as it raises t , so that w may lag behind t . To raise m alone is usually not enough. It should be supplemented by a rise in w , unless m can rise exceptionally fast so as to widen the gap between m and t sufficiently more than the gap between t and w . To observe the rule $(m - w) > 2(t - w)$, m can go on rising without limit as long as $t > w$ and the terms of trade of manufactures will improve. At present m is fairly high. Nevertheless, the limit to a further rise in m relatively to t and w is not set by the terms of trade as such, but by obstacles to m that do not affect w or the non- m part of t . These special obstacles to m limit the possible expansion of m and this causes a premature reversal in the terms of trade. An improvement in the terms of trade of manufactures cannot go on indefinitely, since the natural limits to the relative expansion of m must be reached at some stage, but the point of reversal can be put off. As long as trade barriers against manufactures can be whittled down further, a fully employed manufacturing world has an opportunity (if it takes it) of enjoying an expansion of manufactures trade in world trade with a favourable movement in the terms of trade of manufactures.¹

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¹ The propositions in this paper have been arrived at by economic reasoning. Where the presentation requires mathematical formulation I received kind advice from Miss L. G. Button and Mr. F. R. Oliver (Lectures in Mathematics and Economic Statistics respectively in the University of Exeter).



THE ECONOMIC POSITION OF BRITISH AGRICULTURE

THE ECONOMIC JUSTIFICATION FOR POST-WAR AGRICULTURAL EXPANSION

IN a series of articles in the *ECONOMIC JOURNAL* in 1950 and 1951¹ a number of economists discussed the theoretical justification for the post-war programme of agricultural expansion, and the principles on which it should be based. It may now be a suitable time to look once more at the agricultural situation and see to what extent the developments foreseen in the early 'fifties have occurred.

The economic (as distinct from the political, social or strategic) argument for agricultural expansion was based on the anticipated difficulty of expanding exports sufficiently to solve the post-war balance-of-payments problem. The argument took two forms, one short-term and one long-term. The short-term form (as outlined by Robinson and Marris) was based on the assumption that the demand for our exports was inelastic. They could be increased only if their price were reduced, and the marginal receipts derived from them would therefore be considerably lower than their previous average price. If the elasticity of demand for exports were 2·8 or less (as Robinson and Marris assumed) it would pay to use as much as £200 worth of resources to replace food imports costing £100 rather than use these resources to increase exports.² This justified a high level of subsidisation of home agriculture.

In a recent article³ Professor Robinson elaborates the same thesis. He first argues that the subsidies to agriculture are merely the counterpart to tariffs on manufactured goods, and are at roughly the same rate. This familiar argument may contain an element of truth, but it needs two important qualifications. First, the reduction of tariffs on manufactured goods

¹ "Import-replacement by British Agriculture," C. H. Blagburn, March 1950. Notes by E. A. G. Robinson and R. L. Marris, March 1950 and March 1951, and by G. D. A. MacDougall, September 1950.

² From the normal relation between marginal revenue and price, and assuming perfect transferability of resources, it follows that if P is the price of imported food and E the elasticity of demand for exports it is worth using resources worth $P \left(1 - \frac{1}{E}\right)$ to produce the same amount of home-grown food. This expression equals unity when $E = \infty$. Elasticities of supply and demand for imports can be incorporated, if desired, in a more complicated expression. An analysis of this type (which reaches a diametrically opposite conclusion to that of Robinson and Marris) is given in a recent article ("The Effect of Agricultural Price Supports on the Balance of Payments of the U.K.," Alan R. Bird, *Journal of Farm Economics*, December 1957). In this article a startling conclusion, that agricultural support causes a net loss of foreign earnings, is arrived at via mathematical calculations based on assumed elasticities. There may be some truth in this conclusion, but it is hardly justified by the arguments presented.

³ "The Cost of Agricultural Import Saving," *The Three Banks Review*, December 1958.

through the creation of free-trade areas is being very seriously considered at the moment. Secondly, subsidies and tariffs are not necessarily comparable. If, for example, the guaranteed price for eggs is 33½% above the import price the subsidy will ensure that farmers are paid this guaranteed price. If, on the other hand, there is a 33½% tariff on motor cars it does not necessarily follow that British motor-car manufacturers will raise their prices to a level 33½% above those of corresponding imported cars. In this instance an examination of prices and the success of British motor-car exports clearly shows that they have not done so.

Professor Robinson then estimates that the tariffs on manufactured goods reduce imports by £200 million and that the subsidies to agriculture reduce food imports by £300 million. If these restrictions were removed and imports increased by £500 million our export earnings would also have to be increased by £500 million, or 15%. Taking a figure quoted by MacDougall of 2·5 as the elasticity of substitution of British and other exports of manufactures, Professor Robinson shows, by irrefutable logic, that to increase our export earnings by 15% to pay for these imports would necessitate a 50% increase in the volume of exports. This would imply spending £340 of home resources to obtain additional imports worth £100. On these grounds Professor Robinson concludes that "we are using our resources economically."

There are several objections to this conclusion, some of which Professor Robinson mentions. In the first place, the fact that the United Kingdom would be buying more imports would mean that other countries would be able and willing to buy more from us. Professor Robinson dismisses this by saying that we should still be worse off unless this raised the elasticity of substitution to 5, which he considers unlikely. This approach, however, ignores the way in which the imports of food-exporting countries are in fact affected by their export earnings. When these earnings fall they are forced to impose restrictions on imports, and in the case of New Zealand and Australia there has been a close relationship between the value of sales of agricultural produce to the United Kingdom and imports from the United Kingdom.

The whole "elasticity" argument is really the old one that a tariff improves a country's position by turning the terms of trade in its favour. One of the best discussions of this argument is in Marshall,¹ and his criticisms of it still have considerable force. In the first place, the conclusion follows only if there is no retaliation by other countries; if there is retaliation both countries may end up by being worse off. Secondly, the degree of protection justified by this argument depends on the elasticities that are assumed. If, as seems likely, elasticities are greater in the long than in the short run the degree of protection justified is correspondingly less. No one has convincingly shown that the elasticity of demand for exports is less than 2·8,

¹ *Money, Credit and Commerce*, Book III, Chapter VIII.

or any other number. The attempts to measure elasticities empirically have been criticised on statistical grounds,¹ and it is even questionable, because of the constantly changing conditions in the real world, whether, in the case of manufactured goods, the concept of elasticity can be regarded as an empirical phenomenon. It is useful for analytical purposes to assume that sales are solely dependent on price, other things remaining constant. In the real world, however, other things are constantly changing, and these changes (such as variations in attitude brought about by advertising) are often more important in determining the level of sales than are changes in price. The argument is, in short, an essentially static one, and it is doubtful how far it can be usefully employed as a basis for policy in the real world.

The long-term argument, which was more convincing, was that the United Kingdom would experience increasing difficulties in its export trade because of structural changes in the world economy.² As the under-developed countries began to industrialise and their underfed millions began to compete for the world's food supplies it seemed likely that many of the United Kingdom's traditional export markets would be lost and that the terms of trade would turn increasingly against manufactured goods. Mr. Colin Clark, writing in 1941,³ forecast that if 1925-34 were taken as 100, the price of primary products, in relation to the price of industrial goods, would have risen to 190 by 1960. On the basis of predictions of this kind it was argued that, since agricultural production cannot be turned on like a tap, it should be increased in good time to meet the worsening in the United Kingdom's trading position. A later calculation of primary product prices in 1960 by Professor Lewis⁴ gave the more cautious result of between 78 and 139 (1925-34 = 100).

In the event, the movement of the terms of trade against manufactured goods has been nowhere as great as was expected by the earlier forecasters. On the basis of an average for the period 1925-34 (which covered both boom and slump) of 100, the United Kingdom terms of trade for all goods increased (*i.e.*, worsened) to 130 in 1951, but have since declined, falling to 113 in 1956 and 110 in 1957. The change from before the War appears greater or less according to the base period taken, because of the well-known tendency for the terms of trade to move against primary products in a depression. Thus they fell from 109 in the years 1925-30 to 91 in 1930-38;

¹ Guy H. Orcutt, "Measurement of Price Elasticities in International Trade," *Review of Economics and Statistics*, May 1950.

² See Professor E. A. G. Robinson, "Agriculture's Place in the National Economy," in *Agriculture in the British Economy*, the report of the 1956 I.C.I. Conference. Also "The Future of British Imports" and "The Problem of Living Within our Foreign Earnings," *Three Banks Review*, March 1953 and March 1954. In a subsequent article, "The Problem of Living Within our Foreign Earnings Further Considered," June 1958, Professor Robinson somewhat modifies his position.

³ *The Economics of 1960*.

⁴ "World Production, Prices and Trade. 1870 to 1960," W. A. Lewis, *Manchester School*, May 1952.

the frequently made comparison with the immediate pre-war years is rather misleading. The present problems of international trade in agricultural products are of surpluses rather than shortages and, whatever may happen in the more distant future, there is no prospect that this position will be reversed in the next few years.

There are admittedly many people in the world who do not have sufficient to eat, and the world population is rising. However, there is still considerable scope for increasing agricultural production. An F.A.O. publication states "if 20 per cent of the unused tropical soils were utilised for cultivation the present total arable area in the world would increase by approximately 40 per cent. If this were done, an agricultural revolution, comparable to the opening up of the Americas and Australia, would lie ahead."¹ In the case of the country with the world's largest population—China—enormous increases in food production seem to have been achieved in recent years. Between 1949 and 1955 the production of food grains increased by 63%, according to information which appears to be reasonably reliable.²

Even if a world food shortage did show signs of developing in the future, there would probably be enough time to organise an expansion of home production provided that the necessary basis for expansion was retained. During the War the net output of British agriculture (according to the Ministry index) rose by 24%³ in spite of a severe shortage of resources. If more resources could have been devoted to the task an even more rapid increase in output could have been obtained. There would therefore seem to be little danger of not being able to expand agricultural production rapidly enough if a long-term movement of the terms of trade made this desirable.

It is therefore necessary to look carefully at the cost of support for British agriculture. In the following sections a survey is made of the relative costs of home production and imports of the main agricultural commodities. Since it is sometimes argued that "world" prices are artificially (and thus temporarily) low as the result of the disposal of surpluses at subsidised prices, the question of whether "world" prices truly represent the costs of overseas production is discussed. Finally, a few conclusions for agricultural policy are suggested.

SUBSIDIES TO BRITISH AGRICULTURE

British agriculture is protected by subsidies rather than by tariffs or import quotas (except in the case of fruit and vegetables). There are no tariffs on many commodities, and those on others are generally low. Subsidies to British agriculture in 1956/57 amounted to £251 million (Table I). This was approximately one-fifth of the sale value of the subsidised products and three-quarters of farmers' estimated net income. On the assumption

¹ *Agriculture in the World Economy*, 1955.

² Solomon Adler, *The Chinese Economy*, p. 129.

³ *Output and Utilisation of Farm Produce in the United Kingdom 1946-47 to 1955-56* (H.M.S.O.), p. 8.

that there are 300,000 farmers in the United Kingdom the subsidies averaged over £800 per farmer.

One point regarding the agricultural subsidies needs to be cleared up. It is sometimes argued that they are really subsidies to the consumer, designed to keep retail prices down. If this means that if the subsidies were removed prices would rise proportionately the argument is incorrect, in most cases, at the present time. A substantial proportion of our supplies of most foodstuffs comes from imports, competing freely with home produce, so that market prices are very largely determined by world prices. If subsidies to

TABLE I
United Kingdom Agricultural Subsidies 1956/57

	Subsidies.	Producer price.	Average subsidy †	Subsidy as percentage of price.
	£ million	s. d.	s. d.	
Wheat	19.1	29 9	6 9 per cwt.	23
Barley	10.5	28 6	4 5 " "	24
Eggs	35.1	4 1½	1 3 " dozen	30
Fat cattle	45.4 *	160 0*	40 0* " lb. live weight	25
Fat sheep	9.3	3 2	6 " lb. live weight	16
Fat pigs	31.5	49 7	9 4 " score dead weight	19
Milk	22.1			
Other	12.2			
" Production " grants	66.3			
	251.5			
Total value of price review products (at farm gate)	1,172			
Subsidies as percentage of value	21%			
Estimated farmers' net income	317			

* Including calf subsidy.

† Estimate.

Source: Parliamentary Estimates, and *Annual Review and Determination of Guarantees 1957*.

British producers were reduced, and home production consequently fell, prices would probably rise, the amount of the rise depending on the extent of the fall in home output and its effect on the world market. This effect would largely depend, in the short run, on the proportion of world exports taken by the United Kingdom. This country is the world's largest importer of agricultural produce, taking 17% of world wheat exports, 55% of beef exports and 90% of mutton exports; it is therefore likely that a significant fall in home output, particularly in meat output, would at first cause prices to rise. The rise in price would not, however, be enough to offset the fall in the guaranteed price. In the longer run the rise in price would depend on the scope for expanding world production and the marginal costs of such expansion. Although diminishing returns undoubtedly operate in agricul-

ture after a point (and probably operate in this country), most agriculturalists seem to consider that there is still scope for expanding production in many parts of the world without incurring substantially higher costs. There would be little difficulty in obtaining marginal supplies of cereals, dairy produce and pig meat. The only commodities whose production might be less readily increased are mutton and beef.

A different situation exists in the case of milk prices. On the liquid market milk faces no competition from imports, and its price is (at present) fixed by the Government in conjunction with the Milk Marketing Board. Supplies for the manufacturing market are kept separate from the liquid market and sold at prices competitive with imported milk products, the receipts from the two markets being pooled to give the price to the producer. By setting a high price in the liquid market, in which demand is relatively inelastic, a producer price has been established high enough to stimulate a level of production well above the requirements of that market. This surplus is used for manufacturing into butter, cheese and other milk products; butter and cheese production is, in effect, subsidised out of the liquid-milk price. Thus the government subsidy on milk (which has now been almost eliminated by raising the retail price) provides no indication of the relative costs of home-produced and imported butter and cheese.

AGRICULTURAL PRICES IN BRITAIN AND EUROPE

A comparison of the average prices received in 1956/57 by farmers in the United Kingdom and eight other European countries is given in Table II. These price ratios, which are based on current rates of exchange, do not take into account possible differences in the costs of farm inputs in the various countries. The United Kingdom prices, for example, include subsidies on particular commodities but not the general "production" subsidies, such as the fertiliser subsidy, which cannot be allocated to particular commodities.

The general pattern shown in these figures is the same as that revealed in an earlier study by Professor Nash.¹ United Kingdom prices for wheat are lower than in some European countries, although prices for other crops are generally slightly higher. Cattle prices are generally lower, but prices for milk, eggs and pigs are markedly higher. Farmers in Denmark, the Netherlands and Eire received prices comparable with market prices (as distinct from producer prices) in this country. Farmers in France, Germany and Italy, however, received prices well above this level. In these countries the internal price level is maintained by tariffs and import controls,² so that exports, where they occur, are subsidised in some way or another. Except in the case of French wheat, however, these countries have always been

¹ "The Competitive Position of British Agriculture," *Journal of Agricultural Economics*, June 1955.

² For a review of import controls in these countries, see *Agricultural Policies in Europe and N. America* (O.E.E.C., 1957).

TABLE II
*Prices Received by Farmers in Europe, 1956/57 **
 United Kingdom Prices = 100

	Belgium.	Denmark.	France.	Germany.	Italy.	Netherlands.	Sweden.	Ireland.
Wheat . . .	113	81	130	116	139	82	99	93
Barley . . .	90	89	97	120	127	89	82	85
Potatoes . . .	93	68	100	93	139	84	94	97
Sugar-beet . . .	80	80	97	91	82	69	87	93
Average 4 crops . . .	97	78	108	107	129	83	91	92
Milk . . .	61	64	75	76	77	75	82	69
Eggs . . .	86	61	91	91	98	63	70	60
Cattle . . .	112	88	98	104	121	120	111	75
Pigs . . .	70	83	90	103	95	78	101	81
Average 4 livestock products . . .	79	73	86	90	94	84	91	71
Average all products . . .	83	74	90	94	101	84	91	76

* The price statistics in this article are based on figures from the following: *F.A.O. Monthly Bulletins*; *E.C.E. Prices of Agricultural Products and Fertilisers 1956/57*; Commonwealth Economic Committee reports on "Meat," "Dairy Produce" and "Grain Crops"; *U.K. Trade and Navigation Accounts*. Prices have in some cases been adjusted to give comparable figures.

mainly self-sufficient in foodstuffs, without significant exports, so that the prices they pay their farmers do not directly affect world prices.

THE RELATIVE COSTS OF HOME AND IMPORTED PRODUCTS

To judge whether "world" prices, as reflected on the British market, are a genuine indication of the cost of overseas production or "dumped" prices, it is necessary to compare the payments received by producers in the main exporting countries with the corresponding export prices. From these figures it is possible to make a reasoned guess at what I call the "import supply price," *i.e.*, the price which imported products would command if the major exporting and importing countries removed subsidies and tariffs, after allowing time for producers to make alterations in production. This would be the price at which foodstuffs could be obtained from overseas without the (possibly temporary) advantage of subsidised exports. The figures used are those for 1956/57. Prices of some commodities have subsequently fallen even more, but prices in 1956/57 were mid-way between the high level prevailing during the post-war period of shortage and the low level reached during the present (1958) recession in commodity trade. They are therefore probably a reasonable guide to prices in the medium-long run.

The market price of British products is related to that of imports, but is not necessarily the same, home-killed beef and mutton, for example, commanding a premium because of their freshness, and cereals generally selling at a discount because of lower quality. From the estimated "import supply price" it is possible, however, to estimate the prices that home products

would command if there were free trade in agricultural products. The ratio of this price to the current guaranteed price I will call the "import price ratio." This figure does not pretend to have any scientific accuracy, which is not possible in a hypothetical estimate of this kind. It is simply a reasoned guess, and is only intended to give some indication of the relation between the current guaranteed price and the probable cost of imports in the medium-long run.

Professor Nash (*op.cit.*) made detailed calculations of what he called the "import parity price" of agricultural products, which was the price that British farmers would receive if British subsidies and tariffs were removed. The "import supply price," on the other hand, is the price that would prevail if subsidies in the exporting countries (if any) were also removed. For some products the two terms come to the same thing, but because of the difference in definition I shall not follow Professor Nash's terminology.

IMPORT PRICE RATIOS FOR CEREALS

Table II shows the average price paid to producers in the main wheat-exporting countries in 1956, average export prices and the prices at which wheat was imported into the United Kingdom. The fact that the United Kingdom producer is lower than the import price is probably due to the higher quality of imported wheat and the difference between mill prices and ex-farm prices. In the United States and France the guaranteed price to producers was considerably above the export price, *i.e.*, exports were subsidised. In the United States, however, acreage controls were in operation, suggesting that the same quantity would have been produced at a lower price. In Canada and Argentina, on the other hand, producers were unsubsidised; but the Argentinian price must be viewed with caution, as there had only recently been a devaluation and a radical change in agricultural policy. Until 1956 Australian producers were also unsubsidised, but in that year a small payment to producers was made from a fund built up when prices were higher.¹

Heavily subsidised exports from France must, to some extent, have tended to lower world prices; and the same is true of the United States, although the acreage controls would counteract this tendency somewhat. Canadian producers, however, apparently managed to make a living at world prices. It seems likely, therefore, that the import supply price would be only slightly, say 5%, higher than the actual import price. Since imported and home-grown wheat competed freely, there was probably a fair price relationship, quality for quality, between them. The relationship between the average price received by British growers increased by 5%, and the guaranteed price should therefore indicate the import price ratio: this ratio was 78%.

¹ *The Wheat Situation*, March 1957, Australian Bureau of Agricultural Economics. No. 275.—VOL. LXIX.

TABLE III
Wheat Prices (*s. per cwt.*) 1956

	United Kingdom.	Canada.	United States.	Australia.	Argentina.	France.
Producer price . . .	29.8	18.9	26.5	20.6	15.2	38.8
Export price . . .	—	23.6	22.6	20.0	—	24.2
United Kingdom import price . . .	28.3	29.4	27.4	26.8	27.2	24.3
Market price of home-grown wheat . . .	22.7	—	—	—	—	—
Percentage of world exports* . . .	—	29	39	11	8	4

* Wheat and wheat flour.

Source: Oral Parliamentary answer, *Hansard* December 2, 1957; F.A.O. and E.C.E.

Unlike wheat, barley does not normally come under the United States price-support programme. Table IV shows that the United States producer price for barley was low in relation to that for wheat and only slightly higher than the Canadian. As barley was not subsidised in any other major producing country, it seems likely that the 1956 world price was a true supply price. The import price ratio, calculated as for wheat, was 84%.

TABLE IV
Barley Prices (*s. per cwt.*)

	United Kingdom.	Canada.	United States.	Argentina.
Producer price	28.6	14.5	16.3	11.2
Export price	—	16.6	—	—
United Kingdom import price	25.7	—	—	—
Market price of home-grown barley	21.7	—	—	—
Percentage world exports	—	27	31	10

Denmark and the Netherlands were the main exporters of eggs, and in both countries production was unsubsidised, at prices roughly 60% of those in the United Kingdom (Table II). The United Kingdom subsidy amounted to 30% of the guaranteed price. It seems likely, therefore, that the supply price ratio would be, at most, 75%. Denmark and the Netherlands used to furnish a considerable proportion of British supplies, but home production has now replaced these imports. Apart from liquid milk, this is the only one of the commodities discussed in which the United Kingdom is self-sufficient.

IMPORT PRICE RATIOS FOR MEAT

The prices received for beef by producers in the meat-exporting countries of the Southern hemisphere were roughly similar, and less than half those received by producers in this country. In New Zealand and Australia,

however, producers' prices in 1956/57 were slightly higher than export prices. Under agreements made with the United Kingdom after the end of Ministry of Food buying, the price to producers was to be made up if it fell below a certain level, and in 1956/57 deficiency payments had to be made, particularly on frozen beef, for which there was a falling demand in the United Kingdom; the difference between the "producer price" and the "export price" represents these payments. Producers in Argentina were unsubsidised, and the country has many advantages for beef production, but the financial situation there is still not clear. Prices in Canada and the United States were very much higher, in the case of the United States higher than the producer price in this country. Cattle do not come under the United States price-support system, and there are officially no import restrictions or significant tariffs, but veterinary prohibitions and other controls serve to keep prices very much above the Argentinian level.

TABLE V
Beef Prices 1956/57 (*per cwt. Liveweight, Good-quality Steers*)

	United Kingdom.	Argentina (chilled).	Australia (frozen).	New Zealand (chilled).	Eire.	Canada.	United States.
Producer price . . .	<i>s.</i> 160	<i>s. d.</i> 52 5	<i>s. d.</i> 68 2	<i>s. d.</i> 67 4	<i>s. d.</i> 123 6	<i>s. d.</i> 157 8	<i>s. d.</i> 179 3
Export price . . .	—	—	55 10	65 11	—	—	—
Import price into United Kingdom . . .	—	77 0	78 5	77 0	122 0	—	—
United Kingdom market price . . .	120	—	—	—	—	—	—
Percentage of world exports of carcass beef . . .	—	45	15	15	2	—	4

It is difficult to comment with confidence on the import supply price for beef. The United Kingdom takes a large proportion of world supplies of beef, and supplies are exceptionally dependent on developments in Argentina. Until 1955 it looked as though a shortage of beef and high prices would continue indefinitely: the sudden resumption of Argentine chilled-beef exports on a pre-war scale and the consequent drop in world prices took most economists by surprise. In 1958 prices recovered and the deficiency payments on fat cattle in the United Kingdom were almost eliminated. Argentina seems to have run down her breeding herd, so that exports will be low in the near future, with prices correspondingly high. The production of beef in the United Kingdom is therefore likely to receive further encouragement from the Government. In the long run, however, it seems likely that Argentina, Australia and New Zealand can produce increased supplies of beef, and at prices relatively lower than those being paid to United Kingdom producers. Indeed, if Australia and New Zealand can

succeed in sending chilled instead of frozen beef their competitive position will be further improved. It is exceptionally difficult to forecast beef prices, but in the moderately long run the supply-price ratio seems unlikely to be more than 95%.

TABLE VI
Lamb and Mutton Prices (d. per lb. Deadweight)

	United Kingdom.	Australia.	New Zealand.	Canada.	United States.	Eire.
Producer price	38	21.5	22.5	32	30.5	28
Market price	32	—	—	—	—	—
Import price frozen lamb and mutton	20.5	—	—	—	—	—
Percentage of world exports	—	9	71	—	—	—

Unlike those of beef, United Kingdom prices of mutton and lamb are high, in relation not only to New Zealand and Australia but also to the United States and Canada. Very nearly all the world's exports come from New Zealand and Australia to the United Kingdom. Market prices have so far been well above the floor prices fixed in New Zealand and Australia, so that no deficiency payments have been made in these countries. The deficiency payments in the United Kingdom have also been relatively low, largely because of the premium consumers are prepared to pay for home-killed lamb: the ratio of the market price to the guaranteed price was 84%. Although the United Kingdom takes such a large proportion of the world's exports, there is substantial scope for increased production in Australia and New Zealand. Market prices may therefore be taken as the supply price, which was thus 84%.

TABLE VII
Pig-meat Prices (per Score Deadweight)

	United Kingdom.	Denmark.	Australia.	Germany.	United States.
Producer price	s. d. 49 7	s. d. 42 3	s. d. 40 0	s. d. 52 5	s. d. 27 4
Percentage of world exports (imports in case of United Kingdom)	72	50	—	—	7

Danish producers, who supplied over two-thirds of British imports of bacon, received 83% of the British producer price. Danish bacon was thus able to compete with British bacon in spite of a tariff of 10% and a subsidy of approximately one-fifth of the guaranteed price. As Danish production is capable of expansion, the import price ratio may be taken as 83%. Apart from Germany, pig prices in most countries were lower than in Britain; the

price in the United States was exceptionally low, presumably mainly as a result of cheap supplies of maize.

IMPORT PRICE RATIOS FOR MILK PRODUCTS

The United Kingdom producer price for milk is high compared with those of other countries; in the twenty-two countries listed in the F.A.O. and E.C.E. reports only those for Greece and Finland were higher. The average United Kingdom price was 3s. 2½d. per gallon. The New Zealand producer, on the other hand, received 2s. 4d. for milk sold to the liquid market and 1s. 2½d. for milk sold for manufacturing. The price for liquid milk was higher because production has to take place throughout the year, whereas production for manufacturing can be concentrated in the summer, when milk can be produced most cheaply.

TABLE VIII
Milk Prices

	United Kingdom.	Denmark.	New Zealand.	Finland.	United States.
Price per gallon	s. d. 3 2½	s. d. 1 11	s. d. 2 4* 1 2½†	s. d. 4 3	s. d. 2 4
Average fat content, % . . .	3.6	4.09	—	4.25	—
World exports of milk products, %	—	13.0	18.0	1.0	18.0

Prices quoted are the average producer prices except for New Zealand.

* Liquid price.

† Manufacturing price. Estimated from butter-fat price, assuming 4% butter-fat.

Although the producer price in the United Kingdom (and the retail liquid price) is relatively high, the prices of butter and cheese are among the lowest in the world. Until the proposed controls on butter imports from certain countries which were announced in May 1958, there were no import controls and only low tariffs on supplies from non-Commonwealth sources. Most supplies come from Denmark and New Zealand, both of whom rely on exports of agricultural produce to obtain foreign exchange, and in both of which the producer prices are generally determined by export prices. In recent years, however, there has been a severe fall in butter prices in the United Kingdom, and in the last two years the New Zealand Marketing Commission has paid out to producers the equivalent of 6d. per lb. from a Reserve Fund built up when prices were higher. Table IX shows how the price of New Zealand butter was constituted in 1956/57.

Although the New Zealand and Danish producers are generally recognised as the lowest-cost producers in the world, they are in serious difficulties with the present level of butter prices. The New Zealand Reserve Fund is nearly exhausted, and unless prices rise some New Zealand producers will

probably be forced out of production. The market price of butter, alone among the products considered, is undoubtedly well below the supply price. The same applies in a slightly less degree to cheese and condensed milk.

TABLE IX

New Zealand Butter Prices 1956/57 (d. per lb.)

Producer price for butter-fat in N.Z.	37.5 = 14½d. per gallon at 4% butter-fat
Cost of manufacture	4.0
	41.5
Converted to price for butter (7% moisture)	34.1
Less payment from Reserve Fund	6.0
	28.1
Cost of shipping to United Kingdom	2.0
	30.1

Wholesale Butter Prices (per lb.)

	1954.	1955.	1956.	1957.
	s. d.	s. d.	s. d.	s. d.
London (N.Z. Butter)	3 4	2 9	2 6	2 3
Paris	5 6	5 9	6 1	5 7

Source: C.E.C. Dairy Produce.

The fall in price has been brought about by an increase in production, and the reason why the lowest-cost producers are in the greatest difficulty is that other supplies are subsidised in one way or another. Production in the United Kingdom, as mentioned above, has been in effect subsidised from the price of liquid milk, and output has increased considerably in recent years (Table X). In some other European countries prices have also been higher by an amount sufficient to cause increases in production; when this has exceeded home demands it has spilt over on to the British market, often being sold by government agencies at a price below that in the exporting

TABLE X

United Kingdom Butter Supplies (000 cwt.)

	1938.	1955.	1956.	1957.
New Zealand	2,592	2,436	3,117	2,924
Denmark	2,364	1,696	1,767	1,740
Australia	1,797	1,464	1,515	1,067
Other imports	2,765	554	683	1,575
Total imports	9,518	6,150	7,082	7,306
United Kingdom production	400	296	502	681
Total supplies	9,918	6,446	7,584	7,987

country (which is maintained by import controls). The increase in New Zealand supplies has also contributed to the fall in prices. New Zealand was encouraged by the British Government to increase production during

the period of post-war shortage, but by the time it had risen there was a world surplus of milk products.

On the basis of New Zealand costs the supply price of butter would probably be about 3s. per lb. (wholesale price, London). If the average United Kingdom price of 2s. 6d. per gallon during the six summer months is taken as the cost of milk for butter production, the cost of home-produced butter comes to 6s. per lb., which is the approximate price in France. The import price ratio is thus 50%—considerably lower than for any of the other products considered, even though hardly any actual cash subsidy is now paid on milk.

AGRICULTURE AND THE BALANCE OF PAYMENTS

The volume of agricultural net output (*i.e.*, output less the value of imported feeding-stuffs, seeds and store livestock) increased by 60% between the pre-war period and 1956/57.¹ The consequent changes in the proportion of our supplies produced at home are shown in the following table.

TABLE XI

Percentage of Home Production in Total Supplies

	1938.	1957.
Wheat	12	22
Meat (all types)	49	59
Eggs	67	99
Milk for liquid consumption	100	100
Condensed milk	76	124
Butter	4	8
Cheese	23	49
Sugar	20	29 *
Potatoes	96	96 *
Fresh vegetables	81	81 *
Fresh fruit	23	42 *

* Estimate.

The value of imports replaced by this expansion was calculated in 1953 as being just over £200 million,² since when the increase in production would probably be offset by the fall in import prices. On the other hand, a figure of £400 million has been mentioned by farmers' representatives and is generally quoted in political statements. The exact figure is not at issue, for the problem at the moment is that we have reached a stage when the expansion of home output has begun to affect United Kingdom exports by reducing the earnings of overseas suppliers. Early in 1958, for example, New Zealand had to cut imports by £50 million because of the fall in her reserves of foreign exchange, resulting mainly from the fall in butter prices. As the United Kingdom supplied £140 million worth of New Zealand's total

¹ *Annual Review and Determination of Guarantees 1958*, Cmnd. 390.

² J. R. Raeburn, "Agricultural Policy. Some Economic Results and Consequences," *Three Banks Review*, December 1953.

of £250 million worth of imports in 1957, many of the import restrictions fell on British exporters. The fall in butter prices is, of course, to the advantage of British consumers, at least in the short run, and, in so far as other countries have increased their sterling earnings from butter exports to the United Kingdom at New Zealand's expense, British exports can be switched to other markets. It is clear, however, that further expansion of net agricultural output in the United Kingdom, especially of dairy products, will bring about a corresponding fall in the demand for United Kingdom exports. In such a situation agricultural expansion does nothing to improve the balance of payments.

It does, however, reduce trade with the agricultural exporting countries such as Denmark and New Zealand. By hindering a beneficial international division of labour this restriction on trade harms both the overseas countries and the United Kingdom. In spite of the qualifications and limitations which modern theory has added to the classical theory of comparative advantage, it retains a great deal of truth in many cases. There are reasons of climate, soil and situation which make it advantageous for New Zealand to specialise in the production of butter and for the United Kingdom to specialise in the production of manufactured goods. To take the extreme case, it might be technically possible for the United Kingdom to become self-sufficient in all foodstuffs, provided that enough resources were devoted to this end, just as it has become self-sufficient in eggs. This would involve, however, a severe fall in the real national income of both the United Kingdom and countries such as New Zealand. The fall in living standards would admittedly not be uniform throughout the economies; some British farmers and some New Zealand manufacturers might be better off. The question therefore involves matters of interpersonal comparison on which the economist's views are of no more importance than anyone else's. The economist can point out, however, that the gain to the majority of people in both countries from continued trade is so great that only a small proportion of it would be needed to compensate (financially at least) the marginal British farmers who, in consequence, became redundant. Moreover, the severing of trade with traditional suppliers such as Denmark, Australia and New Zealand would lead to a considerable deterioration in political relations with these countries.

The reduction in imports brought about by an expansion of home production consists of the value of foodstuffs replaced minus the value of any increase in imports of farm inputs (mainly animal feeding-stuffs). Since the War feeding-stuff imports have risen with the increase in home production; in 1958 they equalled the pre-war quantity and cost £148 million. More important than the volume of these imports is the proportion which comes from the dollar area. A recent study¹ by the author indicates that

¹ "The Supply of Concentrated Feedingstuffs in British Agriculture," *Journal of Agricultural Economics*, Vol. XIII, No. 3, June 1959.

in 1957 roughly 50% of feeding-stuffs came from this source and that the expansion of home output between 1952 and 1957 caused an increase in dollar expenditure of roughly £30 million. Since only a small quantity of animal products was obtained from the dollar area, the post-war expansion in their production has substantially increased the United Kingdom's net dollar expenditure. Even the increase in the production of wheat has necessitated a greater use of "hard" American wheat to balance the "soft" British type. The imports displaced by home production have thus been solely from non-dollar areas, particularly those such as Australia and New Zealand, with whom we have not had balance-of-payments deficits.

In short, the post-war agricultural expansion has reduced our imports from non-dollar areas, but the extent to which it has improved our balance of payments is less certain. In the early post-war years, when larger supplies of food from abroad could not be obtained, our increasing home food production went to supplement our meagre rations rather than to reduce imports. In more recent years, when imported supplies have been available, the growth of home output has affected our exports to non-dollar food-exporting countries by reducing their ability to buy from us. In so far, moreover, as our problem has been a dollar problem rather than a general balance-of-payments problem, post-war agricultural policy has augmented rather than reduced it. The "balance-of-payments" case for increasing agricultural production has too often been accepted as self-evident without sufficient examination.

All this does not mean that the post-war agricultural expansion was wrong in the conditions in which it was embarked upon, or that a certain measure of support for British agriculture is not justified to-day. Some support for agriculture may be only prudent in such a predominantly urban nation as the United Kingdom. The point at issue is what degree of support is necessary; whether the post-war expansion of production has gone too far, not far enough or to the right level. It is tolerably certain that further increases in production would be of no benefit to our balance of payments. If it is regarded as desirable to maintain something like the present level of output, the increase in the efficiency of labour (for which there is still enormous scope) will render some farmers redundant. To give an example, in Wales 41% of all holdings over 20 acres are under 50 acres ((holdings of less than 20 acres are rarely full-time farms) and 76% are under 100 acres.¹ The typical 40-acre farm is a one-man dairy farm with up to about fifteen cows. The typical 70-acre farm is a two-man farm with up to twenty-five cows.² By contrast a typical New Zealand dairy farm, in a region of not dissimilar topography and climate, is one of 125 acres, on which two

¹ *Digest of Welsh Agricultural Statistics 1957*, Ministry of Agriculture, Fisheries & Food.

² Information collected by the Department of Agricultural Economics, University College of Wales, and quoted in "An Introduction to Farm Management Analysis," by the present author.

men look after seventy-two cows.¹ Given the necessary reorganisation of farms and farm buildings (which may not be easily accomplished), the labour efficiency of Welsh dairy farms could be raised considerably if the pressure of lower prices made it necessary. It is impossible to ignore the social problems that this involves. Nevertheless, the first consideration is to determine the desirable level of output; provision can then be made for marginal farmers. The reverse procedure, of setting United Kingdom producer prices at a level which will give a reasonable living to marginal farmers, can hardly be justified from the point of view of general welfare.

CONCLUSIONS

In 1956/57 the prices at which agricultural products could be obtained, on a commercial basis, from overseas suppliers were substantially lower than those paid to home producers. After allowance had been made for export subsidies and the rise in price needed to attract marginal supplies, the estimated ratio of import prices to home prices was 50% for butter and from 75% to 95% for other products. World prices in 1956/57 generally reflected prices paid to producers in the main exporting countries, the exception being butter, whose price was considerably below the long-term supply price as the result of subsidised production. Nevertheless, the cost of butter production in New Zealand and Denmark was relatively lower than that of any other commodity. Since 1956/57 prices of cereals, dairy produce and bacon have fallen still further, probably to below the long-term supply price in some cases. Commodity prices in 1956/57, however, were midway between the high level of the post-war period of shortage and the low level of the present (1958) recession in international commodity trade; they may therefore be taken as a reasonable guide to prices in the medium-long run.

The movement of the terms of trade in favour of agricultural products, which many economists predicted after the War, has not taken place. It may do so in the future, but there is no sign of its happening in the next, say, ten years. On the contrary, the problems of the next decade seem likely to be those of food surpluses, not necessarily in the sense that everyone in the world has enough to eat, but in the sense that more food is being produced than they can afford to buy. It is true that there has been a remarkably rapid change from shortages to surpluses in the last five years, and that the reverse process might occur equally rapidly at some future date. In the long run the Malthusian pressure of population on food supplies may have grave results, but for the next decade the problem is more likely to be that of increases in food production which outstrip increases in effective demand. In present conditions the expansion of home agricultural output does not benefit the United Kingdom balance of payments, since its effect on the earnings of

¹ "Productivity and Costs in Australia and New Zealand," by Colin Clark, *Journal of Agricultural Economics*, Vol. XII, No. 1, June 1956.

overseas suppliers causes a corresponding fall in British exports; it also increases net payments to the dollar area.

The framing of British agricultural policy involves judgments of value and considerations other than economic ones. It seems hard to avoid the conclusion, however, that, at the moment, there is no economic justification for a further subsidised expansion of home agricultural output. Higher prices and increased output may be advocated on social or strategic grounds, but the "non-economic" nature of these arguments should be recognised. A certain measure of support for British agriculture as an "insurance policy" is reasonable; but, provided that an efficient agriculture with something approaching the present level of output is maintained, it should be possible to organise further expansion if a shortage of overseas supplies shows any signs of developing in the future.

In recent years the guaranteed prices for some products have been lowered, and the Price Review White Papers have placed an increasing emphasis on reducing costs and less on increasing or maintaining the present level of output. It ought to be frankly recognised that for the next five years or so agricultural policy should not aim at an expansion of output. Indeed, there should be no cause for alarm if the output of milk, pigs and eggs falls slightly. A greater proportion of the agricultural subsidies should be devoted to assisting desirable changes in farm organisation rather than to supporting prices at a level which will give a living to farmers working within the present organisation. In particular, they could be used to assist the amalgamation and replanning of farms into more economic units. They could also be employed to aid the transfer of surplus population out of agriculture by the provision, for example, of re-training grants for young farmers and houses and pensions for older ones.¹

The narrowing margin between costs and prices has already created difficulties on many farms and forced those who run them to alter their farming methods. On farms of adequate size on reasonably fertile soil there is often scope for offsetting the effect of falling margins. The greatest difficulties are in those areas where farmers are earning low incomes because the farms are too small, the farming methods technically backward, or the land simply not worth cultivating at present prices. These farms are only a small proportion of our total agricultural area, but, because they are often concentrated in certain regions, they present a serious economic and social problem. Because of the relative immobility of farm resources this problem is unlikely to be capable of any quick or easy solution. Whatever measures are taken to moderate the impact of change in these areas, the long-term aim should surely be to build up a more economic form of organisation. Such a policy would involve a continuation of trends already in operation—the amalgamation of uneconomically small farms, the decline in the agricultural

¹ E. M. H. Lloyd, "Some Thoughts on Agricultural Policy," *Journal of Agricultural Economics*, February 1957.

labour force and the transfer of more marginal land to forestry. There is also a strong case for setting up more light industry in rural areas, to enable the population displaced from agriculture by increasing efficiency to be employed locally. It may be advisable, on social grounds, to compensate farmers who are hard hit by these changes, but there is no justification for trying to stop the changes themselves or for setting a level of agricultural prices designed to give a reasonable living to farmers on uneconomical farms: if it is desired to maintain the incomes of marginal farmers, special subsidies should be paid for this purpose and should be limited to the life of the farmer concerned. Because of the possible need to expand production at some period in the future it may be desirable to preserve farming on some areas of marginal land where it would not otherwise be justified. In this case, however, the farming should be of an extensive, livestock-rearing type which absorbs few resources; it should also be organised in units large enough to make efficient use of labour, so that these farmers can make a reasonable income without having to rely too heavily on special subsidies.

In all areas, however, the aim of agricultural policy should be to increase output per man rather than to maintain any particular level of production or keep any particular number of men on the land. This will involve changes in policy on such questions as land tenure. Such a change of emphasis does not mean going to the other extreme and abandoning agricultural support altogether. Some degree of assistance for British agriculture may be advisable: its level, however, should be judged by the more-efficient rather than the less-efficient farms, the marginal farms being treated separately, with measures designed ultimately to withdraw resources from conditions in which they cannot be profitably employed.

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COMMENT ON MR. HALLETT'S ARTICLE

I FIND it a little difficult to comment in the *ECONOMIC JOURNAL* on Mr. Hallett's article, since my own attempts to handle these problems are mostly to be found in *The Three Banks Review*, and I do not know how far readers of the *ECONOMIC JOURNAL* can be assumed to have them available. In the main I am prepared to leave judgment to others. There are, however, a few points that I think I should make immediately.

First, may I make the very elementary point that the argument in terms of the balance of payments does not rest on the assumption that the demand for exports is inelastic. It depends on the very different and very much

more probable assumption that the elasticity of demand for exports is less than infinity. I would guess that Mr. Hallett is not guilty of misunderstanding here, but his misuse of words might well confuse others.

May I come to the main issue? Could we, by throwing open our doors and welcoming all imports to a total of, say, an additional £500 million, increase the foreign earnings of those from whom we would buy to the extent that we could export an additional £500 million? This is a problem that far transcends agricultural policy, and one which requires, I suggest, more debate than it has had. I do not myself believe that one can safely count on a pound-for-pound expansion of exports.

My reasons for fearing that one cannot make that assumption are these. Those from whom we shall be buying will, it is true, find themselves in possession of larger incomes. But they will spend these incomes partly on the internal products of their own countries, partly on imports from other countries, partly on savings and only to a small extent on imports from this country. What will happen at the next stage depends on what the central banks and the governments of the countries from whom we have bought may do with their increased holdings of Sterling; and if the result of higher incomes has been larger imports from the United States, Germany and other manufacturing countries, on what their institutions do with the added Sterling. If, with larger incomes and larger holdings of Sterling, they successively increase their purchases of British exports *pro tanto*, reducing where necessary their tariffs to this end, all is well. If they decide to hold more Sterling, equally all is well. But can it reasonably be assumed that they will do so?¹

The point that I was seeking to make in the article that Mr. Hallett criticises is that the number of competing exporters of manufactures is not large. If, by devaluation, we increase our share temporarily at the expense of others, we must expect there to be repercussions on the balances of payments and the exchange rates of others, and these must be taken into account. One cannot apply the principles of atomistic competition to a problem that is essentially one of oligopoly.

I do not want to make a case for agricultural protection for its own sake. To the extent that we can secure greater agricultural imports at an economic

¹ Mr. Hallett quotes Mr. Bird as having reached an opposite conclusion. I find it a little difficult to be certain that I have fully understood the underlying assumptions of Mr. Bird's very interesting article (*Journal of Food Economics*, December 1957). Mr. Bird's model is, I think, essentially a perfect competition model. The supposed elasticity of supply of foreign produce reflects, I think, only the changing responses of foreign producers to alternative offers in their own currencies and not the increasing offers of British exports needed to acquire those currencies. He implicitly assumes, I think, that the rate of exchange is constant throughout, and in effect that the elasticity of demand for exports is infinite. He measures the cost to the nation of added agricultural output by the subsidies paid and not by the increase of resources used. His final conclusion that the same result could have been obtained more cheaply by other means records no more than a general impression. On the basis of what I believe to be Mr. Bird's assumptions, I would fully have expected his results.

cost we should do so. My fear has been that, by throwing open the doors, we shall create a situation appreciably worse than the present very artificial situation. Mr. Hallett assumes, to my mind too readily, a situation of full-employment equilibrium throughout, and his alternatives are alternative full-employment static equilibria. Lord Keynes taught us that we cannot invariably assume full employment. If we create a pattern of the economy with a high propensity to import and we fail to expand our exports to the extent required by full employment, the immediate effect is a deficit in the balance of payments, remedied by a high rate of interest. By limiting the level of activity and by slowing down expansion, we can reduce imports, on any propensity to import, to the point where there is a balance. But are we better off with a higher international division of labour and a lower rate of growth and of employment?

If we are justified in making all the favourable assumptions regarding elasticities, it may be right to open the doors. But I think that some of us who have watched the process of gradual adjustment to the post-war and post-1930 world are not unnaturally anxious that we should be allowed to make haste a little slowly and not allow the theoretically best to become in practice the enemy of the good, to the extent of creating an ideal economy which can only be run at half-throttle. Elasticities are likely to be higher in the long run than in the short. I think we ought to move progressively but not impulsively in the right direction.

While I am not myself wholly convinced by Mr. Hallett, the issues he raises are clearly of great importance and need discussion. The reason why I am unconvinced is that I believe that the import policies Mr. Hallett advocates would imply a fall in the value of Sterling which would make British agriculture competitive: that we are using resources in a pattern that is not very far from economic, but achieving this result in a very unorthodox way. But clearly such a view depends on what are the right assumptions to be made regarding the underlying elasticities.

These issues obviously underlie what one might perhaps call the macro-economic policy of agriculture. I cannot help thinking that the micro-economic policy of British agriculture—the choice of products to encourage—is at least equally important. Mr. Hallett's estimates of the relative costs of different agricultural commodities in different countries do a great deal to assist discussion of these problems.

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REVIEWS

The Long View and the Short. By JACOB VINER. (Glencoe, Illinois: The Free Press (London: Allen & Unwin), 1958. Pp. viii + 462. 42s.)

THIS volume consists of some forty essays, addresses and reviews by Professor Viner of Princeton University, collected and reprinted by his students and other friends in honour of his sixty-fifth birthday.

They cover an amazingly wide field. Did Eli Heckscher (no mean authority) say that for the Mercantilists power was the sole end of foreign policy to be pursued for its own sake, wealth being desirable only as a means to power? Viner says that he did, and that he was wrong. Did Elie Halévy (no mean authority) attribute to Bentham *qua* economist a belief in a natural harmony of interests inconsistent with his efforts *qua* jurist to find means of subordinating individual interests to the public interest? Viner says that he did, and that he was wrong. Were interdepositor transfers at the Bank of England possible from its foundation (1694), or only after the beginning of the recoinage of 1696? What exactly did the Jansenists mean by “un honnête homme,” and what was the role in Calvinist theology of “restraining grace”? Was price competition prevalent in the American cement industry in the 1920s or was it “totally or virtually non-existent”? On these and very many other questions this Viner has presented well-argued and well-documented views. Nor has the labour necessary for their formation dulled his brain for creative work in abstract theory. This volume contains the record of how, with his faithful but rightly rebellious Chinese draftsman, he first equipped us with *two* pairs—one long run and one short—of intersecting marginal and average cost curves, and first uttered the magical words “marginal revenue”—contains, too, one of the earliest and most penetrating reviews of Keynes' *General Theory*.

What relation, then, the uninstructed might well ask, is the Viner of this volume to the well-known authority on international trade—the historian and refurbisher of the classical doctrine of comparative advantage, the pioneer (even if he left some gaps for James Meade to fill) of the pure theory of Customs Unions? No relation, but the same person. Is, then, this cloistered and dedicated scholar a brother or a cousin of the trusted adviser who has been popping in and out of the Treasury and the State Department in Washington any time this last quarter-century and has left his beneficent mark on American foreign economic policy? He is not—he is the same person.

It is a privilege to take even the small part of a sign-post in the birthday salute to this great scholar, teacher and pugilist. Those who wish to get at the heart of his philosophy before being led down the fascinating byways

which I have indicated above should perhaps start with the group of addresses on "The short view and the long," "The role of costs" and "Tensions between government and business." On this point and that there will be some who want to hit back if they have not already done so, or even if they have. All who want to lay down the book with a pleasant taste in the mouth might defer to the last the "Modest proposal for some stress on scholarship in graduate training," which must have been even more enjoyable to listen to than it is to read.

A fourteen-page bibliography of all the works of all the Viners, with a couple of good indexes, worthily rounds off the volume.

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Decreasing Costs as a Problem of Welfare Economics. By C. J. OORT. (Amsterdam: Drukkerij Holland, 1958. Pp. xii + 187. \$1.50.)

DESPITE searching critiques of welfare economics over the last decade by several writers, foremost among them Arrow, Little and, more recently, Graaff, interest in the subject is far from being extinguished. Indeed, the familiar allocative criteria continue to be employed in the journal literature and, from time to time, some modest contribution to the subject appears, though usually one which consciously evades some of the basic difficulties brought to the surface in the last few years. Since the value of such contributions, and of all theorems raised on the base of the traditional allocation rules, can be redeemed only when, and if, the present fundamental objections are met, this phenomenon—this unabating intellectual investment in an apparently bankrupt project—might seem worth pondering on.

In part, of course, the explanation is that we are creatures of habit. Even those who have uncovered serious flaws in the foundations of the Paretian Optimum are apt to find themselves on occasion lingering pathetically at the margin in search of something to equalise. But in the main, the reason is, surely, that, though we make only positivist noises these days, as is the fashion, we are at heart, most of us, welfare economists. Let the debate on welfare economics take the most unpromising turn, the notion persists, "There must be something in it!" For one thing, we cannot stomach the opinion that the price mechanism is bereft of all virtue or, to put it more forcefully, that agreement that any conceivable plan of outputs and income distribution is worse, or better, than the existing one is not possible.

Thus, while admitting that the present substructure is in dire need of renovation, we continue using, and even developing, the superstructure in the faith that sooner or later the necessary work and repairs will commence. Nor is it unreasonable to believe this. The conclusion that nothing of

general applicability may be said once all implicit assumptions have been exposed is familiar enough in other branches of economics. But then, once all bounds are lifted from technological and behaviour possibilities, it is hardly a surprising conclusion. Certainly there is no warrant for further exploration to cease forthwith. Rather, it seems that the phase of subtle speculation on conceivable situations is drawing to a close and will be succeeded by some systematic exploration of the economic universe we live in. For only regard to the nature of this universe will enable us to restrict the limitless range of possibilities, to establish likelihoods and probabilities, to eliminate "perverse" cases and to restore to the subject a sense of perspective. In the meantime, however, and until such empirical research is attempted, welfare economics exhibits the rather depressing spectacle of an elegant edifice lying on its side for want of foundations to support it.

Although a good deal of Dr. Oort's work is on the ground floor, so to speak, it is not work on the foundations proper. If in the first chapter he directs his attention chiefly to the sticky problem posed by the interconnection between resource allocation and income distribution it is not in order to offer a solution but to curb his aspirations. Moreover, two other refractory problems, "interdependent utilities" and the theorem of the "second best," he explicitly ignores. The former goes out by assumption, and no more is said on the matter. Those who hold that social welfare must build on the individual experience of welfare may protest loudly on this and every occasion that a writer prefers facility of treatment to facing "the facts." But I believe there is a lot to be said for this arbitrary ruling. Where a man has become a purely status-seeking animal, whose chief occupation is to keep a step ahead of his fellows, it is likely that social welfare will benefit more from the efforts of the psychologist than from those of the economist. But if this is fair commentary with a country like the United States in mind, it has little relevance to the vast mass of the world's population. In regions still prey to drought and famine, a man is unlikely to feel much cheated if when he eats better he eats in company.

The "second best" is an old problem, but one which has only recently been faced in its generality by Lipsey and Lancaster. It proceeds from an acceptance of the Pareto Optimum to inquire what new rules are to be applied to the remaining sectors of the economy if in any other sector(s) the marginal equalities are infringed. No rules of general applicability emerge and, with less justification perhaps, Dr. Oort relegates the problem to the Appendix, where it is given passing comment and forgotten.

In contrast, we have a fairly thorough treatment of the Paretian conditions and the distribution of income. The present position can be summarised as follows: accepting as an improvement in social welfare a situation in which some individuals are better off and none worse off, a movement to a Pareto optimum is neither a necessary nor a sufficient condition for an improvement. It is not necessary, since other non-optimal

positions can meet the criterion. It is not sufficient inasmuch as some individuals in the optimal position may be worse off than they were in the non-optimal one. Furthermore, a Pareto optimum is myopic: it cannot distinguish between the tops of mountains and the tops of molehills.

All this is very frustrating for one who wishes to make a start on the problems of industry. The various compensation principles, Kaldor-Hicks, Scitovsky, Samuelson, which might be construed as an endeavour to realise sufficient conditions for this optimum, are rejected by Dr. Oort on ethical grounds. Instead, he proposes the more cautious principle of compensated adjustment which entails a double policy rule of imposing Paretian conditions first and afterwards effecting compensating transfers of income.

Having surveyed this difficult terrain, we prepare in Chapter II to make the excursion into the domain of policy by moving from the general rules of resource allocation—admittedly by themselves, insufficient—to partial rules which are applicable therefore to individual enterprises. A cost-benefit criterion is proposed—an algebraic sum of consumers' and producers' surpluses—which is to act as a guide in both marginal and non-marginal adjustments.

Professor Hicks, in his attempts to rehabilitate consumers' surplus, has already attempted something of this sort, but was more successful in defining various measures of the surplus under strictly given conditions than in revealing the relationships between these partial constructs, measured in terms of money, and overall models of welfare in real terms. Though Dr. Oort has faced up well to the latter difficulties, he has to admit that, at best, the cost-benefit criterion he proposes can only "approximate" the allocative ideal (insufficient as this, itself, is). Nevertheless, he accepts the criterion, provisionally at least, as the rationale of marginal cost pricing and moves on into the area of controversy. How should a public industry, especially one subject to decreasing cost, set its charges to the public? In particular, should it attempt to cover its full costs from the sales of its products, or should it depend in some degree upon government subsidies? It is these old but still unsettled questions to which Dr. Oort addresses himself in the remainder of the book.

If one feels some disappointment on putting the book down, it is the subject rather than the author that is to blame. Having selected the problem, and having investigated it conscientiously, his conclusions were unavoidable. Indeed, if the reader knows nothing of the present state of the subject, and does not suspect the worst from a cursory reading of the excellent summary of allocative problems in Chapter I, he is forewarned frequently enough of the conclusions which lie ahead. For like the old-time preacher, Dr. Oort first tells us what he is going to tell us; he then tells us; following which he tells us what he has told us. The message is unmistakable, and what it boils down to is this: a satisfactory solution of the decreasing-cost industry, or any industry for that matter, by means of the

marginal-cost principle is impossible. If the rest of the economy were already optimally organised—a condition which, in the nature of things, is unlikely to be realised—the fulfilment of the cost-benefit criterion in this industry would be akin to attaining a Pareto optimum in the economy as a whole. But, in considering how to cover the deficit in a decreasing-cost industry, we are impelled to recognise the distribution of income problem, a general problem which cannot be solved in any partial framework of analysis.

For those still interested in the marginal-cost pricing controversy, the latter part of the book, and the Appendix in particular, is a mine of references with much valuable commentary.

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Models of Man. By HERBERT A. SIMON. (New York: John Wiley (London: Chapman & Hall), 1957. Pp. xiv + 287. 40s.)

It is an excellent thing that these pearls have been dredged from the depths of learned-journal publication and exhibited to a wider academic world. Professor Simon has allowed small pieces of grit from other people's conceptual machinery to irritate him into building upon their ideas highly polished statements of technical problems in the humane sciences. The readership enlarged by hard-cover publication might have been wider still if he had been content to conceal instead of to excuse his use of mathematics. Why put the most elaborately technical and unreadable pieces in the most prominent positions and preface them with a long excursus on the duty of students in the social sciences to gird up their mathematical loins? If you must use algebra in the P.P.E. subjects you have to be a crab; first get your claws into the reader with your best literary pieces, then eat him at leisure with the mathematical jaws well hidden in the middle. By this anæsthetic method he will scarcely be aware of anything unusual.

In and out through the varied links of his chain of essays Professor Simon pursues the elusive notion of *cause*. Others beside him are determined to haul this notion out of Bertrand Russell's dust-bin. Herman Wold has pinned it down to what is involved when, by manipulating *A* under conditions of experiment, we find ourselves to be affecting *B*, but also find that we cannot thus by manipulating *B* affect *A*. "Under conditions of experiment" is, of course, a vital provision; it is only the fixed setting, the *ceteris paribus*, that gives us the right to pin responsibility for the change undergone by *B* to the change which we have deliberately effected in *A*. The conception as a whole is brilliantly epitomised by Wold's descriptive label for his definition, the "egg within a nest" definition. How does this conception compare with the formal mathematical relation of dependence of one variable upon another? It is plain that if we can write, for example, $y = ax$, there is nothing in pure algebra to prevent us from writing $x = \frac{1}{a}y$. The dis-

inction between the dependent and the independent variable is a matter of arbitrary convenience. "Cause and effect" is thus an asymmetrical relation of dependence, and no formal mathematical statement of the *inter-dependence* of a set of variables can in general be relied upon of itself to tell us which is "cause" and which is "effect." This knowledge is not formal but empirical, it must be brought in from outside, and our manipulation of the systems of equations by which formal inter-dependencies are stated must be voluntarily and consciously restricted to certain permitted types, if the relations of structural or inferential "precedence" which can be inferred from one set of equations are not to be entirely turned inside out when we exchange that set of equations, by operations which *algebraically* change nothing, for another set.

I hope that I have successfully paraphrased Professor Simon's argument. I should myself be inclined to be even briefer, and declare the conviction that cause, like time, has no place in pure mathematics. There is something empirical and non-formal about cause as about the human experience of time. However this may be, it is interesting, though ultimately a little disappointing, to trace with Professor Simon the intricate demonstration of his thesis for systems of linear equations. It is perhaps an unavoidable anti-climax when we read on p. 29 that the matrix of coefficients of the equation-system must not be subjected to elementary row-transformations in general (those which leave the *solution* of the system unchanged) but only to multiplication of rows by constants. It is indeed plain that the "scrambling" of equations which is brought about when, for example, a multiple of one row is added to another row, means that the "causal ordering" of the equations is also scrambled. To say this is not in any way to depreciate the interest and usefulness of the idea of the causal ordering of equations itself.

After a brief chapter on spurious correlation as another aspect of the problem of cause (rather heavy artillery being brought up to capture a position which is intuitively obvious), the author confronts in Chapter 3 the question whether we must confine the term *cause* to experimental situations. The gist of the highly technical argument might, I think, be fancifully envisaged as follows. If, distinguishing man from nature, we suppose that at first the universe consists only of the latter, and if to the eyes of a super-human observer it is plainly deterministic, will such an observer concern himself at all with such questions as "What would have happened *if* the constitution of the universe, or the initial impulse which set it on its way, had been different?" When man appears upon the scene, claiming for his own will a freedom from the determinism of the rest of nature, he finds certain "levers" ready to his hand. Different levers when manipulated produce different effects; his *experiments* allow him to deduce the connection between this "lever" and that "result," and thus he builds up a picture of the working of nature in terms of cause and effect. Once this stage of

the argument is established, can he or can he not abandon his claim to "free will," and say that whether or not his own actions are determined and predestinate, the cause-and-effect picture remains valid?

I think Professor Simon elected to put the set of essays on causation at the front of his book because this is the most fundamental of the notions that engage his mind. But how much better it would have been, within this set, to start with the brilliant and fascinating essay on "Bandwagon and Underdog Effects of Election Predictions" rather than the laborious and somewhat negative discussion of identifiability. The author is formidably widely read in politics, psychology, sociology and organisation as well as in economics, and in the rest of the book he provides samples of all. To the reader of Marshallian or Keynesian indoctrination this book will seem, I think, both impressive and irritating. It is unmistakably of top intellectual quality and packed with instruction.

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A New Prospect of Economics. An introductory text-book by members of the staff of the Department of Economics in the University of Liverpool, under the chairmanship of F. E. HYDE, and edited by G. L. S. SHACKLE. (Liverpool: Liverpool University Press, 1958. Pp. xiv + 498. 30s.)

THIS book covers most of the important branches of economic theory at a standard suitable for a general university course. The approach is broadly traditional: a general introduction, value and distribution theory, monetary and employment theory, a survey of market forms other than the perfectly competitive, international-trade theory, welfare economics and government activity, sources of statistical information and a conclusion which leaves the student to ponder the (mainly unsolved) problems of growth and development.

The authors have managed to pack a great deal into fewer than 500 pages. The ability to condense important material without completely distorting it is evident throughout, but especially so in the later chapters. An intuitively satisfactory summary of the "Protection and Real Wages" argument is given in less than a page, while, in the chapter on welfare, both the index-number problem for individual welfare changes and the Scitovsky compensation principle "paradox" for community welfare changes are outlined in a form which satisfactorily elucidates the general nature of the problems.

The book's main development of economic theory is rather austere: it belongs to that tradition which seeks to develop the theory in a slowly unfolding logical sequence. There are few examples of the sort which Boulding and Samuelson have used so effectively to serve the dual purpose of providing tool-using practice and of showing the student that the ultimate test of economic theory is its applicability to real situations (rather than, for example, its intuitive plausibility).

Part I, on the scope, method and tools of economics, written by Professor Shackle, contains some very good material. Chapter III, "The Tool-box of the Economist," suggests an excellent idea for future text-book writers: explain all the commonly used mathematical concepts and symbols in an introductory chapter, *and then* employ them throughout the text, referring the student back to the explanatory chapter whenever necessary. By the time he has finished, he should no longer be intimidated by the sort of mathematical notation so frequently encountered in the literature. Professor Shackle does the first half of this job in his lucid introduction to the mathematical concepts. Unfortunately the idea has not been exploited: almost nowhere in the rest of the book are either the symbols or the concepts used directly.

Part II, on value and distribution, is the least satisfactory part of the book: it exposes an obvious uncertainty about method and is severely handicapped by a lack of illustrative examples. The author of Chapter 5, on "Economic Motives," tries to meet many of the common objections to maximising theory in two ways: first, by showing that the theories are not as "unreasonable" as they might seem, and second, by re-interpreting them as tautologies so that they become irrefutable (and, of course, totally empty). The theory of consumer behaviour is justified by saying, first, that "... superficial observation seems to indicate that most heads of households spend their ... income in such a way that the material comfort of their families is maximised," and then that, if a consumer does something not calculated to increase material comfort, it merely shows that "... he gets more satisfaction from doing this than from doing something which would increase his material satisfaction" (pp. 55-6). Profit maximisation is defended against the charge of being "unrealistic" by the questionable arguments: first, that it is "... the most realistic of all possible postulates having the same degree of simplicity," and second, that "... the postulate completely reflects reality if we interpret ... [it] as meaning 'maximisation in the long run'" (p. 57). The student is thus left with the idea that assumptions about motivations are to be judged on grounds of intuitive plausibility; not a word is said about the widely accepted view that such assumptions are to be judged by testing empirically the predictions which follow from them.

A number of unusual arguments are to be found in the chapters on indifference curves and production. Convexity of indifference curves is deduced from the assumption that there is "... a technically best pattern of consumption to suit each individual's tastes" (p. 62). Later, p. 63, we are told that, unless we assume measurable utility, we cannot call indifference curves constant utility curves. On p. 105 we read that "... it is never justified to draw the long run AC cost curve envelope as a continuous curve; this is logically invalid. For it is supposed ... that all points joined up by the long run average cost curve lie at the lowest point of the respective short

run cost curves." There is no subsequent explanation of how the author supposes it is to be drawn.

The chapter on distribution is altogether admirable. The concept of a general theory of factor prices is stated and adhered to; the transfer earnings-rent distinction is made quite generally, without the confusion of separate chapters on rent and wages; the chart on p. 141 gives an excellent summary of the relations between the various parts of value theory, relations which are seldom forcibly pointed out in text-books.

Parts IV and V provide a good introduction to income theory. The analysis is well developed and easy to follow; useful summaries of alternative policy objectives are also given (p. 296). The important points of contrast between Keynesian and pre-Keynesian economics are excellently brought out, especially in Chapter 12. The treatment does, however, occasionally become rather naively "Keynesian." On p. 267, for example, we find the unqualified statement that "the rate of interest is a purely monetary phenomenon," and, when the consumption function is introduced, it is justified simply by quoting Keynes' "fundamental psychological law," which by now sounds almost archaic, without mention of the empirical evidence.

There is a general failure to distinguish satisfactorily between positive and normative economics, and in places the two are completely confused. Thus, positive descriptions of equilibrium conditions are followed (p. 334) by the statement that marginal cost pricing "is undoubtedly valid as an idea," and (p. 142) by the statement that: "This justifies the final conclusion in the theory of value that when individuals are left free to seek their own interests ... , satisfactions in the widest economic sense are maximised." In the section on government, positive and normative questions are combined in a single list. The student is seldom given warning that the change from questions about what is to ones about what OUGHT TO BE might involve a change of criteria.

The book will, nonetheless, be a useful addition to the stock of text-books. It will provide a widely acceptable introduction to some branches of economics and, used selectively, it should provide some instructive tutorial essays of the sort: "Compare the treatment of problem 'X' in this text-book with that in some other text (in a totally different tradition)."

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Introduction to the World Economy. By A. J. BROWN. (London: Allen & Unwin, 1959. Pp. 212. 18s.)

As its title implies, this stimulating little book is intended for newcomers to economics; the publishers mention particularly those, "whose interest in economics has been awakened by economic geography, economic history or the survey of current world affairs." It is, however, refreshingly different from the general run of first-year texts.

An economy is defined quite generally as "a system by which people get a living," so that Professor Brown can allow his discussion to range from the whole world to the parish of Headingley. This is a good start, for though most of the subsequent discussion concerns either the world economy or the various national economies, Professor Brown frequently reminds his readers that economic laws and economic problems transcend political boundaries.

The first two chapters discuss the nature of economic decisions and methods of economic organisation on fairly conventional lines, and these are followed by a chapter on "the flows of money" which sets out the relationship between the various components of national income, and discusses briefly the relationship between income flows and the money stock.

From this point, however, Professor Brown departs from the familiar pattern. Starting from the proposition that the standard of living of an economy will depend on its productivity and on the prices at which it can exchange its products for others, he has chapters on high and low productivity, the determination of prices and "good and poor livings." The main theme of the chapter on price determination, which will be familiar to readers of *The Great Inflation*, is the difference between methods of pricing primary products, manufactures and labour, and the key role of primary products in initiating general upward or downward movements in prices. There is much in the argument that is convincing, though one could wish to see it more closely integrated with other aspects of price theory.

The comparison of national economies ("good and poor livings") is followed by chapters on the conditions of growth, the changing balance of occupations in a growing economy, "the roots of specialisation" and the pattern of world trade. The chapter on specialisation differs from the usual elementary treatments of international trade theory in giving economies of scale an amount of space at least equal to that of differences in factor endowments.

Finally, there are sections on "the hazards of economic life" (changes in taste, changes in technology, depression and inflation) and on the organisation of the world economy. This last gives a very brief account of the main international agencies and an indication of what Professor Brown regards as "the next tasks"—the provision of larger resources for economic development, the increase of international liquidity and the development of better arrangements for stabilising primary product prices.

The great merit of the book is its sense of realism and its blend of factual material and analysis. Practical examples and statistical comparisons are abundant and well chosen, and the analysis is clear and rigorous, though it carefully avoids using the technical terms of economic theory. This may well be an advantage to the general reader, but the beginner who intends going further with the subject might prefer to be introduced to some of its specialised vocabulary at the start.

The newcomer to economics who studies this book thoroughly should

find it an interesting and invigorating, though far from easy task. At the end he will have covered a good deal of the ground of more orthodox introductions, though from a distinctly different point of view, and will have an acquaintance with some important problems that do not normally find their way into elementary texts. There will still, however, be important branches of the subject with which he will not be acquainted. The book contains very little value theory and no formal marginal analysis, while the determinants of saving and investment, the rate of interest, the rôle of money in the price system, and the trade cycle are discussed only very briefly. From this it is apparent that Professor Brown has not tried to write a substitute for the ordinary elementary text. He has, however, written a most valuable complement. It should be compulsory reading not only for all first-year undergraduate economists but also for those who try to teach them.

E. VICTOR MORGAN

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Trends in International Trade. A report by a panel of experts. (PROFESSOR G. HABERLER (Chairman), DR. ROBERTO DE OLIVEIRA CAMPOS, PROFESSOR J. E. MEADE, DR. J. TINBERGEN.) (Geneva: G.A.T.T., 1958. Pp. 138. 14s. 4d.)

Commercial Policy 1957. (Geneva: G.A.T.T., 1958. Pp. 67. 7s. 2d.)

THE report of the panel appointed by G.A.T.T. is one of the most important and most distinguished documents that have been produced by an international committee. The international statistics, provided as a background for its full analysis, discussion and recommendations, have been judiciously selected, and compiled, so as to draw attention to relevant totals. The report is excellently written, thoroughly intelligible and notably free of euphemisms and non-committal statements; the thought is tightly packed, and each paragraph is weighty. Furthermore, it seems that the panel has reached conclusions on which a majority of economists should be able to agree.

It is satisfactory to have the following point endorsed by these experts of high authority: "The avoidance of business cycles and the maintenance of a steady rate of domestic growth are the most important contributions which the highly industrialized countries can make to the stabilization of the markets for primary products" (para. 28). Again they say, "we emphasize once more that the maintenance of a high level of employment and full capacity production on the part of the industrial countries is of the utmost importance not only for them, but also for the less-developed countries" (para. 172). In relation to this they observe:

"It is now generally recognized that the modern kind of chronic inflation which confronts the developed industrial countries is not entirely of the 'demand-pull' type, but increasingly of the 'cost push'

variety . . . industrial countries should avoid dealing with the inflation problem merely by keeping the unemployment percentage higher than otherwise would be necessary" (paras. 169-70).

The report favours buffer stock schemes, national and international; it compares their merits carefully with those of buffer fund schemes. "Where a buffer stock is feasible, the two principles of a buffer stock and a buffer fund might well in some cases be combined" (para. 207). On national schemes for securing stability in the home market the authors write:

"The international interest suggests that schemes which rely on the 'buffer stock' principle are most to be preferred, that schemes which rely on variations in 'levies' and 'deficiency payments' to national producers are next to be preferred, and that schemes which rely on varying the incentives to import and export . . . cause greatest instability in other countries" (para. 198).

In the case of international buffer stocks they favour discretion for the manager when to buy and sell. This strikes me as optimistic in relation to the manager, and liable to cause de-stabilising speculation, the force of which might prove fatal to the scheme. I favour, on the contrary, a rigid scheme with floor and ceiling prices at which the stock would be bound to buy and sell without limit of quantity, the middle point to be moved slowly but automatically *over the long term* in accordance with the trend towards excess of supply or of demand, on the lines recently suggested by Mr. St. Clair Grondona. The objection to such schemes is that it would be far more difficult to get agreement to them in the first instance; in their favour is that they might really succeed in their job, because speculative forces would work with, and not against, them.

The authors naturally want an abatement of protection. They think that there may be some force in the contention that G.A.T.T. rules are slightly unfavourable, in their total incidence, to poorer countries. They have one suggestion that particularly arrests attention, namely that excise on raw products mainly produced in the poorer countries, even where it has no protective effect in richer countries, might be reduced, because this would expand *demand* for the commodities in question and therefore the incomes of their producers. Examples are tobacco, tea and coffee. But are not these "noxious weeds"? Was there no shred of sense in the nineteenth-century view that, if one had to discourage some form of consumption or other by taxation, then it would not be excessively paternalistic to discourage those that some may be inclined to consume in excess? It is rather paradoxical to suggest that mature countries should help the under-developed by a method that might impair their own health and vitality.

In the admirable and penetrating analysis of the Report we can already see that the importance of the recent United States recession is somewhat

overstressed. The flattening out of European demand for primary products, which in the preceding years had been larger and growing more rapidly than the American, anteceded the American recession, has outlasted it, and in the total picture of the last three years has been a much more important cause of trouble for the under-developeds.

Furthermore, it was naturally expected last year that the exports of the mature countries would in due course begin to suffer from the fall in the prices and incomes, and from the worsening in the terms of trade, of primary producers. The exports of the mature countries have indeed declined in most cases; but, as things have turned out, the main part of the fall has been in their exports *to each other*. The under-developed countries have manfully tended to maintain their purchases at the expense of their reserves or with the help of loans and grants. Of course that cannot go on indefinitely at the recent rate, if their export earnings do not revive. Meanwhile a number of European countries have been hesitating to adopt fully expansionist policies, on the ground that their own recessions have been largely due to declines in their exports, and that their balances of payments and reserves are not strong enough to risk the consequences of an expansion of their imports. What this argument on their part does not allow for is that it is mainly their exports to each other that have fallen. A good example of a vicious circle!

As would be expected, the panel favours an increase in the medium of international liquidity. It is odd, however, to find this report, with a view to assuaging excessive anxiety, following the precedent of the I.M.F. report, in quoting 1928 as a year in which the value of reserves of international liquidity was not higher in relation to the value of world trade than present reserves. What happened after 1928?

The Report on Commercial Policy, 1957, contains a great mass of interesting information.

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French Banking Structure and Credit Policy. By J. S. G. WILSON. (London: Bell for the London School of Economics, 1957. Pp. 453. 45s.)

Organized Business in France. By H. W. EHLMANN. (Princeton, New Jersey: Princeton University Press (London: Oxford University Press), 1958. Pp. xx + 514. 60s.)

The French Economy and the State. By W. C. BAUM. A Rand Corporation Research Study. (Princeton, New Jersey: Princeton University Press (London: Oxford University Press), 1958. Pp. xvi + 391. 60s.)

THESE three books have their diverse and contrasting strands drawn together in the current economic position in France. Professor Wilson describes the banking system and the mechanism of credit control. He

provides much of the material with which to assess the chances of keeping intact the thirteenth devaluation of the franc, which took place well after the book was published. Mr. Ehrmann's book is a highly critical but fair and balanced analysis of that most cohesive and powerful of all pressure groups in France, the *patronat*. It has been very much in evidence during the recent Free Trade Area-Common Market discussions. Mr. Baum treats France as "the sick man of Europe" and diagnoses a great deal of that sickness as the result of Government intervention. Yet the symptoms are waning at the very time when the grip of the Government is becoming somewhat stronger.

Here, then, are an intriguing series of questions and paradoxes which allow us to dip into these books and test them in terms of developments to which their argument is certainly germane but which occurred after the books were published, and were sufficiently dramatic to have been wholly unexpected by the authors and unanticipated in the arguments they deploy.

The most objective of these three books is Professor Wilson's scholarly study of French banking structure. It fills in what had previously been a gaping void in British bibliography on comparative banking. The author has used the published material, from Kaufmann to Dupont, with discrimination and provides the most up-to-date description of the French banking system, doing justice to such topical and intriguing developments as the evolution of the Banque de l'Indo Chine into a truly cosmopolitan *banque d'affaires*. In his discussion of credit policy Professor Wilson is careful to define the limits and reservations within which it can be effective. There has been no better object lesson of these limitations than that provided by French monetary policy over the past forty years. The ultimate responsibility for the stability of the currency lies with the Government, and the book aptly closes on the theme that "provided the Governments are able and prepared to follow a consistent fiscal and economic policy [should it not be 'correct and consistent'?), there should be little difficulty in maintaining effective regulation of credit conditions. No more than that can be expected of any central bank."

Mr. Baum, in his book on *The French Economy and the State*, casts the State in the joint role of bad fairy and blundering fool. The author is prepared to admit that faced with a war-devastated and stagnant economy, the French Government, after the liberation, had to assume major responsibility for reconstruction and modernisation. He also concedes that the emergency efforts of the State in the immediate post-war period contributed to a rapid recovery of production. But he suggests that the overriding priority given to the so-called Monnet Plan involved the sacrifice of other objectives, including the stability of the currency. Direct controls were quickly abandoned and the principal method of achieving the Plan became the provision of public funds for approved investments at preferential rates of

interest. Mr. Baum condemns even this attenuated form of State intervention by claiming that the French economy "has not grown at a pace that has permitted France to maintain its relative standing among European countries, let alone to make up for the inter-war stagnation." This is a very sweeping judgment indeed and one that flies in the face of statistics that should have been available to an author whose book was published in 1958.

One sector of the French economy which Mr. Baum's accusations fit to perfection is agriculture. The State has been inept, not so much in its positive intervention and initiative as in the weakness with which one government after another has succumbed to special interests. But France should not be singled out for criticism on this score. The plaintive song of Ado Annie, of Oklahoma! fame "I cain't say No" is not, as Mr. Baum suggests in this context, a French solo but a chorus that should be penitently intoned by the vast majority of present-day governments. In particular, it befits those highly industrialised countries which by their agricultural protection are doing so much damage to usually poorer neighbours, the primary producers.

Mr. Baum finds a more typically French target, and also drifts into Mr. Ehrmann's more specialised field, when he deals with the State as the willing accomplice of industry in maintaining France's protectionist tradition. The spirit of Colbert has not waned, though its identification has recently been blurred by balance-of-payments difficulties. These have served as justification between the protectionist policies of successive French governments and those of the *patronat* of which the latest and highly successful illustration is the effective blocking of the project for a Free Trade Area in Europe. And yet, one is faced with the fact that though France may have elbowed out the Free Trade Area, she has embraced the Common Market within which competition from Germany is likely to bear as harshly on French industry as any that would have come from a wider Free Trade Area.

The answer to the paradox is perhaps provided by the arrangements that are already being made between German and French firms for interlocking ownership and demarcation of spheres of interest. Mr. Ehrmann, writing well before the Treaty of Rome was negotiated, suggests that in the view of French industrialists "only an economic integration (of Europe) based on carefully worked out industry agreements can avoid upheavals which the economies of the Continent can ill afford." And he asks "given the past record of national and international cartels, is it likely that their action will embolden competition and thereby create the necessary conditions for accelerated economic development?" It is a rhetorical question. Mr. Ehrmann fears the worst—indeed, he knows the worst. His book is a remarkable study of French industrial pressure groups and of their activities from 1936 to the present day, or rather to the day preceding the birth

of the Fifth Republic. It still remains to be seen how the seemingly irresistible force of the patronat will fare if ever it meets the immovable object who now occupies the Elysée.

One of the main shortcomings of French employers, in Mr. Ehrmann's view, is the deplorable state of relations with the workers. A "proletarian" mentality prevails in most French workshops, and the author suggests that the conversion of the worker "from a proletarian into a citizen" might well be one of the foremost problems of French democracy. In an eloquent and prophetic passage he thus sums up the drift of French economic policy with the dead hand of the patronat on the tiller:

"In a world of permanent international conflict and crumbling empires the country will hardly be in a position to keep pace with its competitors if it does not adjust the political process to the demands of economic growth. While the need for change is widely recognised, there is lack of agreement on what such adjustments should be and how they might be effected. Which of the presently practised rules of the game must be discarded? Which cannot be abandoned without endangering democratic liberties? Should it prove impossible to settle these questions by consent, either immobilism might continuously produce injustice, social cleavage and alienation from the community, or the unchecked impatience of the élite might produce some form of despotism."

"Immobilism" is hardly the word to apply to the France of the 1950s, and it is surprising to find both Mr. Ehrmann and Mr. Baum taking this view of France as a relatively stagnant economy; but of the new "despotism" it is still too early to speak. What it will do for the French dynamic remains to be seen. It can, at least, be claimed for it, that it has given France the promising start of a monetary reconstruction and of a return of confidence in the currency more solidly based than any similar revival since the franc set out on its course of depreciation more than forty years ago.

P. BAREAU

London.

Investment in Innovation. By C. F. CARTER and B. R. WILLIAMS. (London: Oxford University Press, 1958. Pp. ix + 167. 15s.)

In the course of their inquiry into the factors that influence the rate of adoption of new economic and technical ideas by British industry (an inquiry which led to the publication of *Industry and Technical Progress*), Professors Carter and Williams collected data with a bearing on "the background of investment decisions." In writing this second book, they drew upon these data for the purpose of throwing light on the factors that promote or retard investment in innovation. It was not to be expected that they would produce a systematic theory of investment decisions, nor that a set

of closely related and assured conclusions would emerge from their inquiries and analysis. Indeed, they are at pains to emphasise that broad generalisations on this subject are suspect, "that a 'general theory' of investment decisions stands little chance of coping with the complexity of the real world." But, of course, by no means all their conclusions are negative. They make many useful, though perhaps not startling, contributions to knowledge in this field. They present significant facts about the process of investment and they comment shrewdly on business motives.

In their discussion of the "irreducible uncertainty" that attends investment in innovation, the authors have interesting things to say about how firms bring themselves to take decisions to innovate despite the fog that surrounds the future. We are shown that a firm "which is quick in adopting new scientific and technical ideas" is not necessarily more seriously affected by uncertainty than a routinier. The former has usually equipped itself, technically and commercially, to handle these risks, whereas the latter, when it is obliged to take an investment decision, is often unprepared. Some firms, we are told, seek to reduce risks in this field by "creeping up" on a problem. Others make their investment decisions within a "framework of a master decision governing the total amount to be spent." This decision may, for instance, take the form of deciding to produce two or three new products every year. The authors might usefully have linked this discussion with their examination of profit-maximisation as a motive for innovation. It is known, for instance, that some firms pursue a price and profit policy designed to provide resources for financing a rate of expansion on which they have previously decided. However, such a policy raises wider issues than those considered here and cannot, of course, be judged solely in the light of its effects on the rate of innovation.

There is a valuable analysis of the motives that lead firms to refrain from investment as well as of those that impel them to undertake it. For instance, family firms are sometimes inhibited in innovating activities, because the expansion of "their assets to grasp opportunities of profit . . . would endanger family control by introducing outside capital." In their review of the reasons why some promising projects are not proceeded with, the authors reach the familiar conclusion that "in Britain there is an unreasonably high ratio of unconsidered projects, due to poor organisation of research and development, to there being too little of it in private industry, and to a shortage of trained men and women."

It is inevitable that in so slender a book particular arguments should at times be insufficiently sustained and that important generalisations should appear as *obiter dicta* rather than as conclusions based firmly on evidence. On p. 142, where the authors list the influences that favour investment in innovation, they declare that for some industries keener home or foreign competition would be advantageous, for other industries "greater protection or less competition." This treatment of the complex relation-

ship between market structure and innovation is hardly satisfactory or helpful, and the subject certainly demands a far deeper analysis than it receives anywhere in the book. Moreover, inquiries of this character should surely be concerned not only with the rate but also with the type of innovation. If the structure of an industry is oligopolistic or monopolistic, then it is likely that the new projects selected for development will be those from which oligopolists or monopolists can derive special advantages. The merits of the book would probably have been enhanced if the authors had been able to approach their problem by a variety of routes. Their evidence, in fact, is drawn, mainly though not exclusively, from investigations of business experience and opinion during the middle fifties. There are dangers, of which the authors are conscious, in reliance on such data. If one is to reach a deep understanding of the process of innovation and of the motives that determine investment decisions, it is desirable to investigate also, in considerable detail, the course of successive innovations in particular industries over a long period.

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World Without War. By J. D. BERNAL. (London: Routledge & Kegan Paul, 1958. Pp. xi + 308. 25s.)

THIS is a book about what science could do for mankind if "The Cold War" were replaced by "conditions of peaceful international economic competition or, still better, of co-operation." The under-developed parts of the world could then develop, in a generation or two, "a standard of living at least equal to that enjoyed in the older industrial countries today" (pp. ix and x).

After an account of the destructive powers of nuclear weapons and a critique of the policy of "nuclear deterrent," Bernal attempts to justify his claims about the possibility of ending poverty. He first deals in a general way with the potential impact of science on supplies of food, raw materials and final outputs. A full application of science in industry would involve a revolution both in education and in the organisation of science—at any rate in "capitalist countries." Judged from the present Russian output of scientists and technologists in the appropriate age-groups, Britain would need a five-fold increase in the numbers trained (p. 201), and the supply of those directly or indirectly involved in scientific tasks would rise gradually to 20% of the working population (p. 88). As "in practice it has never been difficult to find enough people providing the community is willing to pay for them" (p. 102), there could be a sufficient supply of science students. Britain would also need more research and a more centralised system of scientific research organisation: to give an efficient information service and to concentrate scientific effort at the points "where advances are easy and abundantly rewarding." But finding the right problems is often more diffi-

cult than finding the solutions, and the efficient application of science to industry requires a close interaction of science and industry. To get this the scientist must be on top and not just on tap: "The key position which science is acquiring in our society will impose such a relation, but it can impose it usefully only if the administrators working with the professed scientists are also sufficiently scientists themselves to understand what the matter is about" (p. 204).

In a key chapter "The Time Table of Transformation," Professor Bernal gets to the detail of his forecast of productive possibilities. International grants or interest-free loans equal to one-quarter of the present amount of military expenditure would provide all the funds necessary to industrialise all the under-developed countries within the world within ten to twenty years (p. 244 and Table XXI). The preparatory phase would take ten years. The second phase of constructing industrial centres—enlarging eleven industrial centres and creating fourteen new major and thirty minor industrial centres (Map 1)—would take ten to fifteen years (p. 257), after which the new centres would be able to finance their own further development.

How Utopian is this approach? His starting-point is the rather speculative estimate given in *Measures for the Economic Development of Under-developed Countries*, of foreign capital needed for an annual 2% increase in income per head in poor countries. Bernal adds 50% to this estimate and then asserts from the experience of China that since "much more can be done for the same money by investing only in essential equipment" (p. 245), a 10% increase in real income (and not just of industrial output as the United Nations report supposed) could be achieved. Poverty is not eliminated by compounding statistical guestimates in this way, but even if real incomes per head in poor countries were raised 10% per annum, Egypt would take twenty-five years to reach the present British level, Burma, Ethiopia, Ecuador, Korea, Philippines, Saudi Arabia, thirty-five years, and Indonesia forty years.

It would seem that Professor Bernal has in mind the pattern of Soviet financial and technical aid to China. Is this a likely model for the rest of the under-developed world? In China a strong Communist party gained power by its own efforts, there are strong political reasons for aid (not least being tensions between Russia and the West), and it is a sufficiently large and self-sufficient nation to be suitable for a Russian-type industrialisation. In this sense China is a very different proposition from Bernal's other potential major industrial centres—the Middle East, North, West and Central Africa, Central and South America. There not even the appropriate *nations* exist. Who, then, would receive the monies equal to one-quarter of present military expenditure if the Cold War countries were prepared to give it? Without a very strong state in a viable economic unit Professor Bernal's crash programme of industrialisation would not be feasible. He sometimes writes

as if the end of "colonialism" or of "indirect colonialism" (which appears to be a state of suffering from private foreign investment) is sufficient to create the necessary conditions of rapid economic growth. As he argues that "really rapid economic advance is possible for an undeveloped country only when it is free from both foreign and native capitalism" (p. 229), he seemingly envisages the end of colonialism as a facet of the rise of Socialism. But even the rise of world Socialism would not quickly create those new nations and internal conditions required for Bernal's crash programme. It is impossible for foreign Governments to induce "10% growth" by offering capital grants and technical aid. Any attempt to force the pace could produce new forms of "colonialism" inconsistent with the requisite enthusiasm for education, invention and reconstruction. It is no accident that in this book China is the show-piece and not the East European countries.

Although Professor Bernal's book is over-optimistic about ending "the crime of poverty," too unaware of the political and human problems of creating new nations and transforming them very quickly, and insensitive to Western fears of the strength and objectives of Soviet military power, there is a sense of purpose, of the possibilities of transforming methods of production, of the power and potentialities of education, that I found most stimulating and instructive. My impression when reading the book was much more favourable than when for reviewing I looked again at the detail, and this I think is due to higher quality of his writing on war and science and education than on economic programming. A very quotable passage, which sums up Bernal's attitude, can be used to end this review: "What we have lost, particularly in the old capitalist countries, and what really gives salt to life is purpose. What purpose we conserve is for trivial or negative ends."

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Economic Elements in the Pax Britannica: Studies in British Foreign Trade in the Nineteenth Century. By ALBERT H. IMLAH. (Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1958. Pp. xiv + 224. 48s.)

IN spite of the fact that much of the material in this volume consists of estimates already published in journal articles, and frequently quoted by other authors, it remains a good buy for students of modern economic history. For Professor Imlah's estimates are still the best available for the crucial missing links in the statistical record of British foreign trade, and it is an obvious convenience to have them in one place instead of dispersed in different volumes and on different library shelves.

These methodical calculations of the annual values of imports and re-exports during the first half of the nineteenth century (when the trade accounts still enter most of them at the prices of 1697), the more daring, but still painstaking estimates of the invisible items in the nineteenth-

century trade balance, and the various indices of the terms of trade for the period 1796-1913, are basic data for the economic historian. They will be criticised and no doubt partially revised (though criticism is easier than improvement) by those who dispute the heroic assumptions underlying the estimates of the invisibles or who find new data with which to reconstruct some of the price and quantity series. But it is safe to assume that Professor Imlah's near-monopoly in the field of nineteenth-century British trade statistics will long be protected by the formidable quantity of labour and know-how which his estimates embody.

The familiar series, carefully revised, described and justified, and supported by important new breakdowns and additional estimates take up about half of this volume. The other half contains the author's own interpretation of the significance of his results.

His theme is a simple one. It is that Britain's liberal commercial policies, culminating in the free-trade system of the second half of the nineteenth century, made a powerful contribution to her own rapid economic growth and to the expansion of the international economy. Professor Imlah examines the evidence for the period after the Napoleonic Wars and finds that high protection constituted a positive hindrance to British exports and, by inference, a genuine drag on an economic progress based on the extension of mass-production methods. During the twenty-five years following the lifting of the barriers in the early 1840s the market value of exports grew twenty times as rapidly as in the preceding quarter century. "What is surprising is that net imports increased at a less rapid rate than British exports in the first period (1842-73) and less rapidly in amount than total British income throughout the (free trade) era as a whole." The relatively rapid growth in exports was reflected in the British contribution—by capital exports—to the development of the world economy.

Professor Imlah presents his case for the liberal economic policies of nineteenth-century Britain with clarity and conviction. It is not often that economic historians use quantitative material so skilfully and with so little loss of literary quality. The result is the kind of book that can be happily recommended to all students of economic history from the undergraduate onwards.

Specialist users of historical statistics will wish for more explicit recognition and discussion of rival estimates (for example, some of the official estimates of real values of imports which were made at various dates in the first half of the century). But when so many economic histories are grossly overweight it seems ungracious to dwell on the omissions of an author who has managed to keep within such elegant proportions.

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Manpower. By H. M. D. PARKER. History of the Second World War, United Kingdom Civil Series, edited by Sir Keith Hancock. (London: H.M.S.O. and Longmans, 1957. Pp. xviii + 535. 40s.)

Labour in the Munitions Industries. By P. INMAN. History of the Second World War, United Kingdom Civil Series, edited by Sir Keith Hancock. (London: H.M.S.O. and Longmans, 1957. Pp. xv + 461. 35s.)

THESE two volumes in the Civil Series of the History of the Second World War are complementary; as an Editor's Note states, Mr. Parker "deals with the total resources of man-power in the United Kingdom and their use . . . the responsibility under War Cabinet direction of the Ministry of Labour and National Service," while Mrs. Inman deals "with man-power as a factor of production within the industries controlled by the Supply Ministries." Although some overlapping is inevitable, Mr. Parker's general survey often gains considerably when supplemented and illuminated by the detailed study of the problems of the selected group of industries of key importance provided by Mrs. Inman, while many of those problems, and the measures taken to deal with them, cannot be properly understood except in a framework of broader problems and policies.

Mr. Parker's volume is in two parts, the first concerned with the general problems of mobilisation and use of man-power, the second with a series of particular topics. Part I opens with brief reviews of the problems of the First World War, of the employment situation between the wars and of (exiguous) man-power planning for the Second World War, all "essential to an understanding of the difficulties with which man-power planners were faced in the autumn of 1939."

The treatment of mobilisation and use of man-power in the war years is partly chronological and partly by topic (call-up for the Forces, the application of controls to civilian man-power, the development of central planning and of the administrative machine and so on). One can only admire the skill with which Mr. Parker has reduced to order a mass of complicated material to provide a lucid and absorbing account, in which there is sufficient detail to make the problems and policies comprehensible and susceptible to the reader's judgment, but not so much as to obscure the main picture.

The main theme is, of course, the development of stringency and the methods of overcoming it. Two major turning-points can be distinguished. The first was Dunkirk and the coming of Ernest Bevin to the Ministry of Labour. In so far as an Official History can have a hero, it is here the Minister of Labour. As Mr. Bevin himself said, "I came in at 2.30 on the afternoon of Tuesday and on Wednesday at 11 o'clock I produced at least the outline of the basis of my scheme" (as Mr. Parker shows, there was more than simple egotism in statements of this kind which came not infrequently from the Minister). It was this scheme which was the basis for

the mobilisation of man-power and its allocation for the next eighteen months, which saw the Essential Work Order, registrations for civilian employment, concentration of industry, and so on.

By the autumn of 1951, stringency was becoming greater, and the next few months saw the second great turning-point. In December 1941 there was legislation putting an obligation on all men and women between the ages of eighteen and sixty to carry out some form of National Service, and providing for conscription of women to the Auxiliary Services. This was followed not only by the application of closer measures of control but also by further development of the techniques of man-power budgeting, reaching their full fruition in the Ministry's survey of the autumn of 1942 and the War Cabinet's decisions based thereon which set the pattern for the peak of mobilisation by the autumn of 1943. To show the general order of the problem, in June 1942 the employed population consisted of about 22 million men and women, of whom about 9.5 million were in the Forces, Civil Defence and the munitions industries (in June 1939 there had been about 3.7 millions in these categories), with a further 5 millions in "essential" industries (such as coal, agriculture, transport) which could hardly give up much man-power. The needs of intake of the Forces and munitions over the eighteen months to the end of 1943 were put at about 2.7 million people, against about 1.6 million who could be made available. Re-assessment of programmes was therefore inevitable. These, however, were broad statistics; although the man-power "budget" was an essential instrument in planning, it was necessarily limited to broad decisions. The man-power needs for industry (and to some extent for the Forces) were for a multiplicity of different skills, changing from time to time, and in various parts of the country; they were by no means exactly matched by the people who could be made available from other employments. Thus the process of labour supply was usually complex, with an increase of one worker in a Royal Ordnance Factory involving possibly the movement of several others in a sort of general post. The problems which were thus posed, and the means of dealing with them are, of course, described in detail by Mr. Parker and Mrs. Inman.

Although it could be argued that the peak was reached too soon, the degree of mobilisation achieved surpassed that of any of the other belligerents, and in spite of the wastes which are inevitable in the changing conditions of war, the resources thus mobilised were in the main effectively used. There were many critics of the Ministry's policy, especially in the earlier stages, but Mr. Parker's judgment supports the Minister:

"It may also be claimed, with some justice, that Mr. Bevin was sometimes unnecessarily hesitant in introducing measures which he recognised would sooner or later be required. . . . But, although there is substance in these criticisms, there is another and better criterion by

which both the direction of policy and its timing may be judged. In retrospect the most impressive feature of the progressive mobilisation of the country was the willingness of men and women to co-operate with the Government. . . . This was the strength and vindication of Mr. Bevin's policy."

To this pragmatic justification, there can be added that Mr. Bevin also helped to preserve human values in which he himself profoundly believed.

The second part of the volume covers a wide range of topics, such as the contribution made by women to the war effort, the employment of juveniles, training, welfare, wages and industrial disputes. These chapters suffer at some points from compression, and at times are little more than summary accounts of administrative measures. There is, however, much of more general interest, and on some matters Mrs. Inman provides the flesh for what is otherwise little more than a skeleton. To the present reviewer her account of wages and industrial relations, in industries in which these matters were not only important in themselves and in their influence on other trades but also particularly intractable, is particularly valuable and illuminating.

One moral which neither author explicitly draws is the degree to which many of the accepted features of the present industrial scene had their origin as war-time expedients in the process of total mobilisation. The increased employment of women, continuing after marriage and often part-time, is well known, and is in some degree paralleled after the First World War. Less well known are such matters as the juvenile employment service, various training and educational schemes, decasualisation of labour in certain trades, "welfare" in the factory and the growth of "personnel management." These books are thus not only the standard works on the vital aspect of the war: they are also essential for understanding the development of many features of the present industrial economy.

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Factories and Plant. By W. HORNBY. History of the Second World War United Kingdom Civil Series, edited by SIR KEITH HANCOCK. (London: H.M. Stationery Office and Longmans, 1958. Pp. xiii + 423s. 6d.)

MR. HORNBY has a hard task in this volume of the Second World War History because so much of the ground has been trespassed on already by other volumes on War Production, Labour in the Munition Industries, The Control of Raw Materials and so on. But he contrives to give us a skilfully coherent and comprehensive story, based on the history of the munition industries right back to the Napoleonic wars. Those who were engaged—

necessity sectionally—in the war-production struggle in 1940–45 will find a great deal here they didn't know as well as recognise a faithful record of what they did.

It used to be said in the Ministry of Supply during the War that the Royal Ordnance Factories produced everything the British Forces needed up to the Crimean War, but that new-fangled weapons invented since, such as tanks and aeroplanes, had unfortunately to be bought from private industry. Mr. Hornby shows how the dilemma of policy between reliance on the original R.O.F.s—Woolwich, Enfield and Waltham Abbey, already operating in the Napoleonic Wars—and a private armament industry, caused much argument long before 1914. Convenience tended to dictate a great expansion of R.O.F. production in the wars, and the maintenance of capacity between wars by private industry producing for foreign countries.

"Although every war in the nineteenth century in which Britain took part brought a significant increase in the resources of the Royal factories, the inquest on every war after 1850 resulted in a policy in favour of extending the armament industry and limiting, to some degree, expansion of the Government factories. Thus it was true to say that the Government factories thrived in war but the armament industry thrived in peace or on rumours of war."

So when the First World War came, a vast expansion of the three R.O.F.s into 250 National Factories bore the real brunt of the war-production effort. Woolwich became the largest factory in the world, with 70,000 employees. After 1918, back we went again to Woolwich, Enfield and Waltham—with only an embryo filling factory at Hereford, an engineering R.O.F. at Birtley and an explosive R.O.F. at Irvine. The Nazi threat, therefore, found our State munition capacity grievously run down; but with an even worse deficiency than in 1914. Tanks and aircraft had never been produced in R.O.F.s, partly because these weapons had at least their civil counterparts in civil aircraft and road and rail vehicles.

For these reasons, the 1939–45 production effort was tackled by a huge expansion of the R.O.F.s for ammunition, filling, explosives, guns and small arms; but the "agency factory" principle (State-owned factories managed by private firms) was relied on for aircraft and tanks. The agency factory was very convenient for the now powerful aircraft and vehicle industries, because the Government took the financial risk and management stayed in the hands of private firms. Mr. Hornby rightly pays tribute to the remarkable achievement of the R.O.F. expansion between 1939 and 1943 (from six factories to over forty and from 50,000 workers to over 300,000). All this was secret at the time and not much publicised afterwards, because private industry was naturally more publicity-minded. But Mr. Hornby says justifiably that in 1943 "with more than forty factories and an employment

of over 300,000, the R.O.F. organisation was the largest munition undertaking in history . . . this expansion was the equivalent of the creation of a new major industrial sector. It was a transformation which would normally be achieved only over a long period of years."

Probably the greatest failure of the British Munitions effort was tank production. Really mass output of a wholly successful tank was hardly achieved, even by 1944. Some thought in the later war years that this was due to lack of R.O.F. production, and to 100% reliance on private industry. At any rate, Leeds R.O.F. was brought into tank production in the later war years, and was soon successful.

Was not the huge volume of aircraft production, however, wholly achieved by private industry, though mainly in agency factories, a proof that private firms could do the job? Partly, but only partly, because the aircraft expansion was to a great extent achieved at the expense of other (and perhaps more essential) forms of war production. This can be seen again from Mr. Hornby's excellent chapter on location of factories, though he does not himself draw the moral explicitly. The R.O.F. organisation pretty continuously located its huge new plants in areas of readily available labour, which also happened to be less strategically vulnerable (Wales, Scotland, Cumberland and the north-east coast). The aircraft factories, under short-sighted pressure from private managements, were ludicrously concentrated in Coventry, Birmingham and Bristol; until the Ministry of Aircraft Production by 1942 or so (as it used to be said in the Ministry of Supply) had at last heard of Manchester and went in a rush to Lancashire. In 1940 Mr. Hornby says, "the greater proportion of aircraft production was still in highly vulnerable areas." (Rolls-Royce at Hillingdon, Glasgow, was an honourable and highly successful exception.)

As a result, the Ministry of Supply was usually ahead of the M.A.P. in the race for labour; there was a persistent aircraft production crisis; and in the end all sorts of artificial "super priorities" had to be given to aircraft programmes to get near the target. It may be that if the R.O.F.s had produced aircraft before the War and had expanded in the right areas, these crises would have been avoided, and the War finished earlier.

One of Mr. Hornby's most interesting comparisons is between the production record in the First and Second World Wars. It is not nearly as favourable to the Second World War as many would expect. Taking four and a quarter years in each case, from August 1914 to November 1918, and from September 1939 to the end of 1943, output of rifles in the First World War was double that in the second, of small-arms ammunition greater, of filled shell about the same, of explosives and propellant about the same, of the larger guns greater, of merchant shipping tonnage not very much less, of machine-guns nearly one-third and of aircraft nearly two-thirds.

True, tank and vehicle production was far higher in the Second World War; and some weapons, such as bombs, were hardly produced at all in the

First World War. Also aircraft and many other weapons of the Second World War were, of course, far more complex and costly. Nevertheless, the comparison is surprising and requires explanation. Part of the answer is no doubt the much greater dependence on U.S. supplies in the Second World War, and partly the absence until mid-1944 of the non-stop murderous land battles which raged in 1916, 1917 and 1918.

Even so, those who have congratulated themselves on the "total mobilisation" of 1940-45 must, I think, in the face of Mr. Hornby's figures, pay a very heartfelt tribute in retrospect to the astonishing munition effort of 1914-18.

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Social Economics. By WALTER HAGENBUCH. Cambridge Economic Handbooks Series. (London: Nisbet and Cambridge University Press, 1958. Pp. 320. 12s. 6d.)

SINCE economics is essentially a study of man in society, one might doubt the logic of selecting particular topics for treatment under the label of "social economics." The intellectual justification is that the topics chosen are all near the borderlines between economics, politics and sociology. A more practical reason is that they are all of particular interest to those occupied in or training for what is commonly called "social work." This is probably the market for which this book is mainly designed, though there is much in it that deserves the attention of a wider public.

The first half of the book is concerned with the social problems of population, housing, working conditions, unemployment and poverty, and the second with the social services—the voluntary services, National Insurance, National Assistance, the National Health Service, the welfare and child care services, and food and housing subsidies. Education is included in the estimate of social-service expenditure, but is not one of the topics discussed in detail. On each of these subjects there is a brief historical sketch, an account of the present position, generally with some useful figures and guidance on their sources and an analysis of present and future economic problems.

To cover all this ground in a small book inevitably means that some aspects are treated only slightly, but Mr. Hagenbuch, aided by an easy and economical style, has done an excellent job of compression. A short review can only draw attention to a few of the many good things in the book. The population chapter includes a beautifully clear and concise account of the various measures of population change, while its estimate of the increase in productivity that will be needed to take care of an ageing population without a fall in living standards is a useful corrective to some popular fallacies. The

chapter on housing contains a good discussion of the uses and limitations of the price mechanism in the housing market and of the effects of rent control; and that on the voluntary social services includes some very wise remarks about the importance of these services in a free society and about their probable future. A further point that might perhaps have been made here is the effect of declining leisure among business and professional people on the supply of voluntary effort; it is much harder to find time and energy for voluntary social work if you have to do your own domestic chores.

In the sections on the public social services, the best features are the clear distinction between services involving transfer payments, services in kind and subsidies, and the analysis of the economic effects of each; the comparison of each type of expenditure with the relevant item in the national-income accounts; and the penetrating analysis of changing aims and methods in the social services. Here, Mr. Hagenbuch emphasises the extent to which the subsistence element in the original (Beveridge) National Insurance plan has been abandoned under the stress of inflation, and the degree to which the humane and efficient work of the National Assistance Board has broken down traditional hostility to the means test. These changes could open the way to a substantial raising of the "poverty-line" by selective aid and, when they can be seen in retrospect, they may appear as among the most important developments in the social services during the past ten years.

There are, however, several important points about which little or nothing is said. The whole question of the usefulness of the insurance concept in a universal service is discussed only briefly, and there is no mention of the possibility of graduated contributions and no attempt to estimate how far the social services redistribute income. Another very fundamental point which could have been discussed more fully is the distinction between objective standards and value judgments. Mr. Hagenbuch gives a good account of the uses and difficulties of objective standards in relation to poverty, but there are times when he seems to accept them rather too easily in relation to housing and medical services. These are fields in which the professional is under great temptation to impose his own value judgements on others, and it is important that both he and we should know when this is happening.

The limitations of this book are largely inherent in its scope. Its virtues, which are many, are those of the author—lucidity and penetration in analysis; clarity and ease of style; scholarly care in the handling of facts; and a humane and liberal outlook. These are qualities which make the book a worthy member of a famous series.

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The Structure of Competitive Industry. By E. A. G. ROBINSON. Cambridge Economic Handbooks Series. (London: Nisbet and Cambridge University Press, 1958. Pp. ix + 156. 8s. 6d.)

"LET us suppose that a meteoric shower of a few thousand large stones harder than diamonds fell all in one place . . . they would revolutionise many branches of industry; and the owners of them would have a differential advantage in production, that would afford a large producers' surplus." In contemplating their marmoreal resistance to time and decay, one is tempted to think that it was the Cambridge Economic Handbooks that Marshall had in mind when writing this passage.

Professor Robinson's handbook certainly gives proof of "being so hard as not to be affected by wear." This revised edition is, in all important respects, identical with what he first gave us nearly thirty years and 35,000 copies ago. When he published a revised version in 1953, he wrote, "I have not felt any strong desire to re-write it because things which I had originally written have been shown to be wrong." He is still of this mind. Essentially, what he has done in this latest revision is to integrate into the body of the book the epilogue which he added in 1953, to deal with ideas or changes of emphasis which research and the passage of events had suggested. Very little has been dropped, and the various changes of detail are largely in the illustrations and examples.

The book remains, therefore, what it has always been, flesh for the bare bones of competitive equilibrium analysis. In saying this, I do not wish to imply that there is no recognition of growth or change. Indeed, there is a chapter dealing specifically with the problems of growth. But this awareness expresses itself by way of qualification rather than in any modification of the conceptual framework.

Some will question whether the platonic ideal of the optimum firm is a fruitful analytical concept. But, for better or for worse, it remains one of the main tools in whose use the apprentice economist is instructed. It has been said that you cannot be taught economics, you can only learn by watching people do it. Professor Robinson's book remains a good introduction to workshop practice. If mastering the book will not entirely guarantee an honourable second-class degree, it is true nevertheless that the supplicant will neglect it at his peril.

J. DOWNIE

London.

Black Dyke Mills. By E. M. SIGSWORTH. (Liverpool: Liverpool University Press, 1958. Pp. xvii + 385. 35s.)

THIS book is a study of a large specialised worsted firm, Fosters' of Queensbury, from its foundation as a domestically organised concern in

1819, through a period of rapid expansion, through the "Great Depression," up to the time when it became a Limited Company in 1891. The value of business histories lies in the fact that they both explain the development of the industry and show the variety of the specific individual events which make up the general trend. The first and less successful half of this book is largely based on the former role; the second half achieves great success in the latter.

The three introductory chapters on the history of the worsted industry are highly selective and deal particularly with the concentration of the industry in the West Riding and the development of Bradford rather than Halifax as the centre of the trade and the technical developments. This is not altogether satisfactory; the worsted industry is considered in isolation (even the woollen industry is scarcely mentioned), and the depth of analysis varies very greatly.

The history of the Black Dyke Mills starts with a chapter based on the early account books; these form the basis of discussion of typical development. This is followed by detailed analysis of the expansion of the mills, the development of productive capacity and finance. This section is difficult to read, and constant reference to other sections is entailed, nevertheless, there is much information and a number of points of importance; the most striking is the accumulation of share investments. Between 1865 and 1867 (when these records end) the share investments constituted over 50% of the firm's assets, which were then £1.4 million. This great firm had become an investment trust as well as a worsted manufacturer.

The last four chapters, largely based upon private letters, are mainly concerned with "the techniques employed in the daily struggle with competitors." They give an excellent opportunity to assess the relative importance of different types of competition in the trade. The relationships between long-term strength, commercial acumen, strategy, quality and type of product are shown in their constantly changing patterns. The events, and the competitive methods which gave rise to them, emerge very clearly and give a vivid and fascinating picture of the techniques and customs of trade. The unmeasurable qualitative and strategic factors in competition are beautifully illustrated, and their great importance is shown clearly and forcibly. Information of this type is seldom available, and although generalisations about this aspect of competition are seldom illuminating, an understanding of the varied forms of competition is crucial. It is thus the final section of this history of Black Dyke which is particularly interesting and valuable.

LESLEY COOK

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Commonwealth Perspectives. By N. MANSERGH, R. R. WILSON, J. J. SPENGLER, J. L. GODFREY, B. U. RATCHFORD and BRINLEY THOMAS. (Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1958. Pp. viii + 214. 34s.)

In the spring of 1957 a Seminar was held at Duke University with the object of opening students' eyes to Commonwealth problems, and to the possibilities of research therein. These essays are the revised texts of the papers read at the seminar, and they form a very worth-while synopsis. There runs through the essays a feeling that the facts of Commonwealth relations and practices must be set out, as to an audience reared in a highly critical tradition; but although the essays are concerned with contemporary problems, they are academic in approach and almost entirely free from polemic. This virtue is, indeed, almost excessively cultivated, for emphasis is throughout on the statement of facts rather than on analysis of meanings and implications; and the facts are those of legal instruments, diplomatic discussions, abstracts of investment and emigration returns, of welfare expenditures and of export trades, rather than the more intimate realities of political strategy, personal loyalty, and individual standards of living and conduct, which count for so much in Commonwealth relations.

Such essays must, however, be judged in their appropriateness to the audience to which the papers were read, and their attitude of detached and impersonal inquiry is admirable for the purpose of revealing some elements in the complex Commonwealth situation. In the first two essays Professor Mansergh sets the tone when, lucidly and getting his emphasis from understatement, he takes his stand on the Balfour Report and its definition of Dominion status, and emphasises the equality between the Dominions, and between each of them and the United Kingdom. In his first essay he illustrates his point with reference to the Statute of Westminster, the royal succession and the abdication crisis, the status of India and Pakistan as republics within the Commonwealth, and the decision of Ceylon to continue within the Commonwealth. In his second essay he takes the issue into the particular realm of foreign policy. More might easily have been made here of the peculiar circumstances of the ANZUS pact, in which Australia and New Zealand entered into a defence commitment with the United States from which the United Kingdom was excluded; and the underlying significance of India's policy of non-alignment in relation to her membership of the Commonwealth could provoke much discussion. But the great bases of the Commonwealth relationship are clearly and attractively set out; certainly in their negative aspects, if less certainly in their positive binding forces.

Less attractive was the paper on Commonwealth citizenship, and the territory-by-territory treatment which is there adopted is also followed by

Professor Spengler in dealing with the implications of demographic dimensions. This, in effect, is a study of emigration movement within the Commonwealth. As an inclusive survey it is succinct and useful, but its conclusion—that diminution of British-bred emigrants must weaken the bonds of the Commonwealth—is raw and over-simple; perhaps purposely over-simple, since the economic, politico-military and constitutional principles involved in finding an alternative to membership of the Commonwealth are complex and difficult. The case-history of the emergence of Ghana and the study of the welfare programme of Australia might have provided opportunities for examining some of these factors. But the Ghana study maintains the level of a useful, sound narrative, slightly sceptical of the pace of development and of the predominance of cocoa in the economy of Ghana, but reluctant to analyse motive or to predict development; and the Australian study does not get outside of that Dominion to assess the relevance of the welfare system there to the rest of the Commonwealth. This is, however, a most interesting essay, from which two major conclusions emerge—that a welfare programme such as a white population requires (and white politicians demand) almost inevitably works towards a steady extension of federal power and a diminution of state-rights; and that it has brought Australia to a point at which her economy is less flexible than it should be, and absorbs a higher proportion of her national income than is compatible with her need for capital development.

Such conclusions are, however, not closely argued. They are rather tossed out as the basis for discussion at the end of the essay, and the main emphasis is here, as elsewhere, on the setting out of a factual narrative. Only in the last essay, by Professor Brinley Thomas on the Sterling Area, does something more analytical and disputatious emerge. Here a brief historical account of the pre-war Sterling Bloc and of the post-war Sterling Area leads direct to the refutation of the thesis that the Sterling Area is but the disguised last outpost of imperialism, to the emergence of Great Britain as the world's greatest debtor nation, the fundamental differences between Keynes and Vinson, and the problems of convertibility in the post-war period. Even here, although the analysis is economic (and the conclusion is that British resources ought to be able to provide some £350 million a year to finance purchases by the holders of sterling balances, to repay loans from North America, the International Monetary Fund and the European Payments Union, and for overseas investment) yet emphasis is on the constitutional aspects of the problem. Conflicts of interest between members of the Sterling Area, and between Great Britain and members of the "Outer Sterling Area," are therefore emphasised, with the informality of the arrangements and the difficulty of forming any diffused policy-making body. The crises of convertibility since 1947 admirably illustrate these points and lead to the conclusion that, although the members are likely to become more independent in disposing of their sterling balances and in

control of their currencies, abrupt changes are unlikely and the short-term advantages of seceding are by no means clear.

The volume ends, as the essays run throughout, on a gently-phrased fact which challenges but does not enforce speculation—against the deeply-rooted non-economic forces which give meaning to the Commonwealth, and the economic forces making for community of interest within the Sterling Area, must be set the "towering stature and bewildering dynamism of the American economy." Which must have made a very stimulating talking-point for a seminar!

E. E. RICH

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The National Income of Tanganyika, 1952-54. By ALAN T. PEACOCK and DOUGLAS M. DOSSER. Colonial Research Studies, No. 26. (London: H.M. Stationery Office for the Colonial Office, 1958, Pp. 78. 7s. 6d.)

THIS is the second national-income study to appear in the Colonial Research Series, and like its predecessor, the Prest and Stewart study for Nigeria, it is appropriately distinguished by qualities of common sense, imagination and scholarship. It is also distinguished by an unusual economy in text and table. This brevity is at times disconcerting and at time very illuminating. The table called "The Social Accounts of Tanganyika, 1954," for example, turns out to be one double-entry account with six items—imports and gross domestic product in market prices on one side and exports, government purchases, gross capital formation and consumption on the other. The empty boxes of a fourteen-industry input-output table, on the other hand, support more effectively than any amount of argument the authors' conclusion that input-output studies deserve a low priority in the statistical programme of an economy such as this.

The authors have shown that it is possible to be concise without indulging in the kind of false economy that occasionally characterises official national-income publications. They have defined their concepts, described their methods of estimate, evaluated their data and interpreted their results. Thus, although their gross national product aggregate differs conceptually from the Prest and Stewart total, the content of both is sufficiently explicit to permit the derivation of matching aggregates. Whatever the perils of basing welfare conclusions on such inadequate evidence, those who need an index of comparative poverty to inform policy decisions or analysis will continue to adapt national-income statistics to their purpose. It is not too much to ask that those who produce national-income statistics should give as much information as they can on the contents and weaknesses of their figures.

There is, however, an important cleavage of opinion on a matter of

principle which makes it difficult to derive conceptually comparable estimates for all African territories, however frankly annotated the basic estimates. This is the issue of the treatment of non-marketed production. At one extreme is the school of thought which holds that it is meaningless to impute a market value to activities and commodities not intended for the market. Its adherents would ignore subsistence production altogether in computing national product or, less defensibly, represent it by a token figure with no empirical basis at all. At the other extreme are those who choose to evaluate subsistence output at retail market prices, possibly even in a processed form (such as grain converted to meal), and so to include a value for the distributive and processing services which might be imputed to it. In the Peacock and Dosser table of gross domestic product the output which is largely subsistence (staple crops, livestock, forestry, hunting and fishing, brewing and the like) amounts to about half the total at factor prices. Widely differing estimates of the total could be reached by valuing subsistence output at less than local market prices or by including further processing imputations. To ignore subsistence output altogether might be to reduce the total by between a third and a half.

It is no accident then that those who prefer to value subsistence output at retail prices give a markedly more optimistic impression of standards of living in the more backward areas of Africa than those who ignore it or insert a low nominal figure. The authors of this report reject as a travesty of the truth the conclusion of a United Nations mission that millions of Africans in Tanganyika are existing on a starvation diet. In the light of their results the familiar vision of the desperate poverty of Africa, of peoples "poor because they are poor" is considerably dimmed. A people which can consume 10% of its income in beer, an economy whose gross capital formation can amount to between 13 and 14% of gross domestic product, is clearly not bumping along at starvation level.

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PHYLLIS DEANE

Colonial Planning: A Comparative Study. By BARBU NICULESCU. (London: Allen & Unwin, 1958. Pp. 188. 18s.)

"PLANNING," says Dr. Niculescu, "is mainly an administrative exercise," and his book, which is cast in terms of this definition, makes a useful review of development expenditure and its organisation in Colonial budgets of the last ten years. Most of the material relates to Africa, for the African colonies of each European Power heavily outweigh the Colonies outside Africa, both in area and in population.

The book is specially important because it breaks new ground in many ways: it attempts to cover the dependencies of all the European Colonial Powers; and it brings together the first collection upon this subject even for

British territories. Finally, it carries policy statements on Colonial planning back to an early date: in Britain to the Colonial Development Act of 1929; in France to the debates in the French Parliament of 1921 (though these were only translated into legislative policy in the 1930s); and in Belgium to the first Congo Development Plan of 1906. Thus it provides a guide to planning in each group of dependencies. It brings out the chief differences of planning organisation in each, the British and French being perhaps best described as decentralised (territorial) and centralised (metropolitan) planning respectively, while the Belgian Congo appears to have in it very little public planning at all, even in the limited sense in which the word is used in this context.

Unfortunately, the information presented on the different groups of territories is not of the same quality nor even quantity in all cases. As one might expect, British Colonial policy is particularly well documented, while there is next to nothing on the Portuguese or Spanish territories; every student of this field knows that it is very difficult to come by hard facts upon these latter colonial areas. It is more disappointing, on the other hand, to find relatively so little on the French territories and on the Congo. In this sense the book hardly bears out the promise of a comparative study. Dr. Niculescu does give the aggregate sums contributed by the metropolitan country to public expenditure in each group of colonies, but the global figures, interesting though they are, do not in themselves bring out the essential differences of British, French and Belgian colonial development policy without some guidance upon the fiscal and monetary framework within which each operates. For Dr. Niculescu assumes somewhat too readily that readers understand the differences in economic institutions and practices in the different colonial areas. Thus, while all English readers would understand, more or less, the framework within which to assess the impact of our Colonial development, they might well misunderstand the significance of the French and Belgian efforts, without guidance on Colonial budgetary and monetary mechanisms operating in these two other areas.

Even where comparative material exists, it is not always presented. It would, for example, have been interesting to have for British territories the breakdown of planned expenditure which is given for French colonial plans on p. 146. Similarly, the problem of priority allocation, which is admirably discussed for British territories, is dismissed somewhat summarily in the French case and not mentioned at all in the Congo. The same weakness shows in the historical analysis of the development of colonial policy in the different areas.

While the book does not come off fully as a comparative study, it provides an interesting and detailed critique of British colonial policy since 1945. In effect, it traces the growth of the idea of colonial "Plans" in Britain and gives a most sympathetic interpretation of its aims and purposes. It is specially good (within the terms of the initial premise already stated)

in its handling of certain criticisms which have been put forward in recent years, such as those arising from the continuing growth of colonial sterling balances. It also begins with a good review of basic concepts in the early chapters, such as that of underdevelopment, but thereafter it becomes largely a survey of administrative machinery and practice, satisfactory enough to those who define planning as an administrative exercise but rather less so to those who are interested in economic change.

For Dr. Niculescu gives no economic assessment of any of the plans even as between British colonies, though he says (p. 179), "it should be possible to discern some kind of outline, however blurred and shifting, of what plans may have succeeded in achieving, of what they have done but may have done wrongly, and of what they have omitted to do." To have attempted even the broadest of such assessments would certainly have greatly increased the size of the book and would probably have changed its scope. Some preliminary assessment, for example, would have been needed of the changed economic position of the colonial territories arising from the changed post-war terms of trade of all primary producers. Again, a preliminary comparison of British, French and Belgian Africa would have been needed to discount for the effects of Marketing Boards where these exist. Each of these preliminary investigations could, of course, produce books in themselves, so Dr. Niculescu may have been wise not to attempt any such assessment, on this occasion at least.

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Indonesia's Economic Stabilization and Development. By B. HIGGINS. (New York: Institute of Pacific Relations, 1957 (London: Allen & Unwin, 1959). Pp. xxii + 179. \$4.00.)

THIS book is an expansion of an Institute of Pacific Relations Conference paper (Kyoto 1954), enlarged by new factual material from the Massachusetts Institute of Technology's Indonesia Project. It bears the optimistic imprint of the work of the Centre for International Studies; and also suffers from being written by the Director of a continuing project in Indonesia, who must therefore criticise with discretion. Although well written and easy to read, its overall design shows insufficient co-ordination.

It falls into three parts, dealing with short-run stabilisation problems, long-run development and the political situation. The stabilisation chapter describes and analyses the trade regulations and monetary gadgets used from 1952 to 1956 to deal with balance-of-payment deficits and inflationary pressures after the collapse of the Korean War boom. It would have been improved by more information about the technical problems of budgeting and Treasury control in Indonesia, which certainly aggravate a difficult

situation. The account of the multiple exchange system and the different surcharges, advance payments and the like is clear and factual, and the interpretation, though sometimes rather too brief, is illuminating.

The chapter on long-run development mainly describes the development plan. The summary is clear and helpful, but there is not enough criticism or interpretation of the allocation of expenditure between objectives, and too much explanation of the reasons for delay in preparing a plan. The author mentions the plan's neglect of population policy and limited treatment of migration problems; but this hardly justifies his own very limited attention to demographic problems in this section. He recognises in principle that the growth and the distribution of population are key problems in any long-run view of Indonesia, but gives them no extended treatment or analysis.

British readers (browsing through the conclusions) may underestimate Professor Higgins's political judgment because of his insensitive comments on post-war Britain. His judgments of Indonesia seem (to a neighbour, not a resident) much better informed. He brings out clearly how ideas cut across party labels, but might have given us more information about the organisation of the parties and the economic basis of their membership. How vulnerable are they to manipulation by economic interests or infiltration by Communist agents? A tabular presentation of some of the facts about cabinets and parties would also have made a useful appendix.

Any economist's first comment on his analysis would surely be that it paints an over-optimistic picture. An Englishman in Singapore, with numerous Chinese contacts, may have a rather jaundiced view of the Indonesian economy, but why try to give the impression that illicit dealings in currency, smuggling and corruption are transient and peripheral phenomena in Indonesia? The climax is reached when the author describes as "history minded" those Indonesian intellectuals who, after ten years of independence, can still think of nothing for their country but endless sacrifice to "complete the revolution." Must inter-faculty rivalries be pushed as far as this?

Then, when Professor Higgins justifies multiple currency practices as instruments of taxation in underdeveloped economies, and even recommends a system of taxation through the Central Bureau of Statistics, one would expect some attention to be given to the question whether the damage this may do to the normal functioning of banks or of the collection of statistics offsets the advantage of greater present efficiency in collection. There is no real analysis, even in a static sense, of the economic gadgets for stabilisation as more or less efficient uses of Indonesia's very limited resources of administrative skill and integrity. But dynamic aspects are also important. Will the effectiveness of such resources as there are be still further reduced by throwing additional strains on them?

The discussion of the incremental capital-output ratio, where Professor

Higgins takes a more pessimistic view than the Indonesian authorities, will tantalise specialists in economic growth. Figures quoted for net investment range from Rp. 600 million to Rp. 5,000 million, and the estimates of incremental capital output ratio from under 2 for the whole economy to 4 for the agricultural sector and 4.5 for the industrial sector. On the basis of calculations which give, as he admits, "rough orders of magnitude at best" he estimates that Indonesia needs to invest at rather more than double its present rate of Rp. 5,000 million a year. Discussion follows on probable sources for these funds, a discussion adapted to American ears in 1957.

Is it not time to vary the approach? Surely Harrod and Domar must sometimes draw back in horror from some of the uses to which their apparatus is put! The statistics are wholly inadequate for this kind of thing; and it is doubtful policy even to use scarce resources of statistical talent to improve them in this direction. The trouble seems to have begun with the United Nations Report on *Measures for the Economic Development of Underdeveloped Countries*. Here the capital output ratio was used to indicate the order of magnitude of the capital sums that would be required to initiate a reasonable rate of growth in the underdeveloped countries. This report certainly helped to develop a climate of opinion favourable to economic aid, for which we may be grateful. It was followed up, however, by an attempt (*ex post* rather than *ex ante*) to dress up the first Indian Five Year Plan in terms of income growth and capital formation; and this seems to have set a fashion for vague estimates of the need for external aid to achieve defined rates of growth in income or income per head—such aid to be provided with a minimum of control over its use. Aid, of course, is not usually obtainable on these terms. But any encouragement to Asian planners to think that it should be merely heightens the suspicion in the United States and elsewhere of international funds for economic development and strengthens the present trend towards re-emphasising commercial investment.

In Asia, economic development is handicapped by a market structure riddled with monopolistic practices and rigidities, and a situation where even an efficient market would give wrong solutions to many problems of training and rural unemployment; a universal belief in economic planning, and great cultural difficulties in producing an efficient and impartial civil service; well-grounded suspicions of the political and monopolistic aspirations of foreign capital, and hostility to local capital markets by all those in a position to establish them; great shortage of practical scientific knowledge and simultaneous natural opposition to the social and cultural effects of education mainly conducted in foreign languages. Nowhere, perhaps, are these difficulties more crushing than in Indonesia. But Professor Higgins, like so many other foreign economic advisers who know these countries, has good reasons for failing to emphasise them.

Economists need the help of other social scientists (and of skilful international politicians) in offering solutions to these problems. But shortage of skills have economic implications also. On p. 83 Professor Higgins states that "it is not necessary to choose between increased production of foodstuffs and industrialisation, or between smallholders' agriculture and plantation agriculture, or between small industry and heavy industry in preparing a plan to raise overall productivity. The diversity of Indonesian resources permits an attack on all fronts at once." But even if the capital were available, there are scarce resources of skill and training ability that make it urgent to choose, and choose economically.

These criticisms are meant to suggest that we may expect (and shall doubtless soon receive) something better from Professor Higgins about Indonesia. But his deep knowledge of the country makes this book essential reading for any specialist on Indonesia. To the general student of Asian economics it will probably be intramarginal, but mainly because the field is not intensively cultivated. Students of economic development or stabilisation may find it disappointing.

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NOTES AND MEMORANDA

KEYNESIAN ECONOMICS REHABILITATED: A REJOINDER
TO PROFESSOR HICKS

PROFESSOR HICKS' recent "unorthodox" review¹ of my *Money, Interest and Prices*² is actually confined to the two chapters (XIII and XIV) of this book which present an interpretation of the Keynesian theory of employment and contrast it with the corresponding classical theory. Professor Hicks' article is essentially an extended criticism of this interpretation. The purpose of the present note is to show, however, that this criticism stems from a failure to understand the main proposition on which my interpretation is based.

Ironically enough, the point at which Professor Hicks goes astray is in the proper extension of his own well-known *IS-LL* diagram of the Keynesian system to a set of assumptions slightly more general than that considered by him in his original celebrated article.³ This diagram is presented in Fig. 1—with the difference that Y represents *real*, and not *money*, income. (Since we shall assume throughout the following analysis that wages and prices move proportionately, it makes no difference whether we measure real quantities in terms of price units or wage units.) *IS* represents all combinations of interest (r) and real national income (Y) for which savings equals investment. *LL* represents all combinations of r and Y —as of a given price level and quantity of money—for which the demand for money is equal to its supply. The equilibrium position of our economy is given by the intersection of these curves at P . If prices and wages are absolutely rigid—and if Y_1 represents the full-employment level of income—the economy will remain permanently at the level of "unemployment equilibrium" Y_2 .

Let us now drop the assumption of absolute price rigidity and assume instead that the pressure of unemployment at Y_2 causes wages and prices to fall simultaneously. This causes an increase in the real value of the public's money holdings. Let us first assume—in accordance with the usual Keynesian approach—that this increase affects the system only through its prior effect on the excess demand for money and thereby on the interest rate. Then the fall in the price level has no effect on the *IS* curve

¹ "A Rehabilitation of 'Classical' Economics?," *ECONOMIC JOURNAL*, Vol. LXVII (1957), 278–89; henceforth referred to as "Hicks (1957)." The adjective just quoted is from p. 278.

² Evanston, Illinois, 1956; henceforth referred to as "*Money*."

³ "Mr. Keynes and the 'Classics,'" *Econometrica*, Vol. V (1937), as reprinted in *Readings in Income Distribution* (ed. W. Fellner and B. F. Haley, Philadelphia, Pa., 1946), pp. 461–76; henceforth referred to as "Hicks (1937)."

I have explained elsewhere (*Money*, p. 229, footnote 21) why the *LL* curve should not be drawn with a left-hand horizontal segment. Nevertheless, it is drawn here as Hicks draws it in order not to complicate the present discussion with other issues.

in Fig. 1. On the other hand, it causes a downwards and rightwards shift in the *LL* curve. In Keynesian terminology, at any given level of real income, the real transactions demand is fixed; hence the public will agree

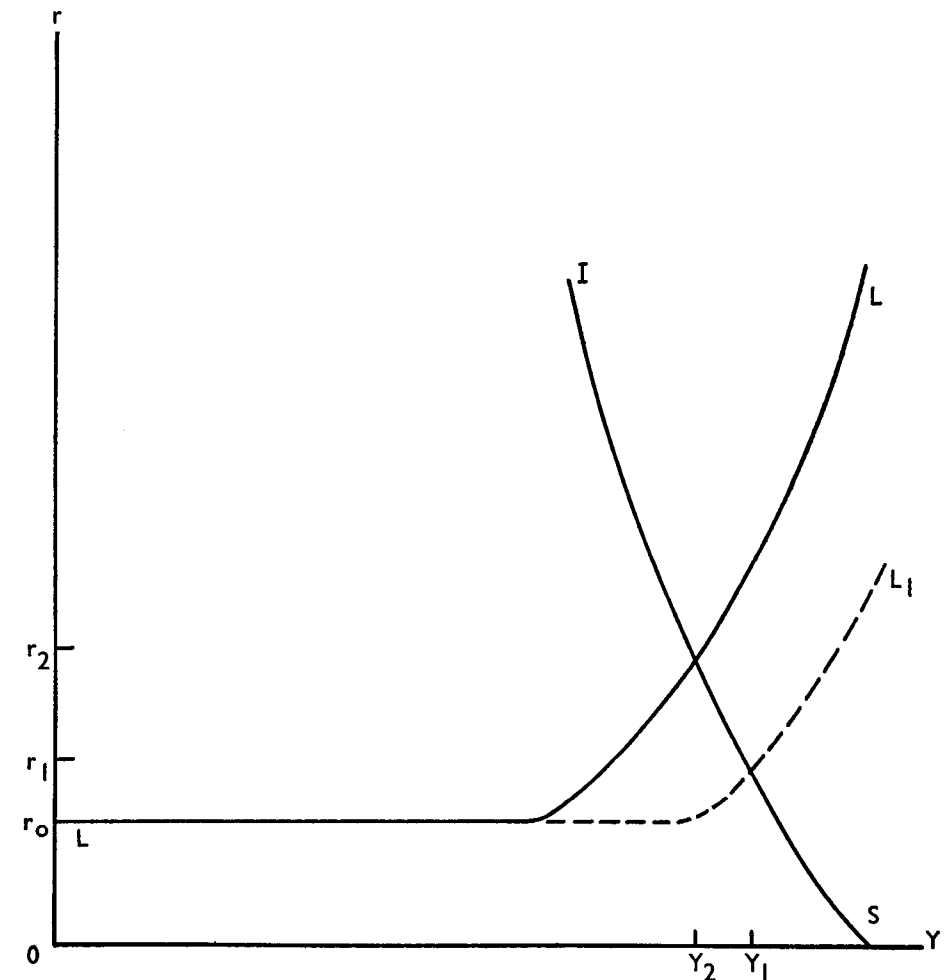


FIG. 1

to hold the increased real quantity of money only at a rate of interest sufficiently low to increase the speculative demand appropriately.¹

In Fig. 1 this process is assumed to bring the economy to its full-employment level: *LL* shifts rightwards to LL_1 so that it intersects *IS* at Y_1 . In economic terms the decline in the price level continues until interest has fallen sufficiently (from r_2 to r_1) to stimulate investment and discourage savings to such an extent that they are equal at the full-employment income Y_1 .

¹ Cf. Keynes, *General Theory*, pp. 232, 266–7 and 298.

In Fig. 2, on the other hand, we have the oft-cited case of the "liquidity trap." (For the moment, the dashed I_1S_1 curve here should be ignored.) Even when the rate of interest has fallen to its minimum level, r_0 , the level of demand in the commodity market (as represented by IS) is not high enough to generate full employment. Instead, unemployment to an extent measured by $Y_1 - Y_3$ remains in the system. It follows that in these circumstances no automatic force exists to extract the system from its state of unemployment.

So far we have remained more or less within the analytical framework of Hicks' 1937 article and its later elaboration at the hands of Modigliani.¹ But let us now depart from this framework and assume that an increase in the real quantity of money in the system has a *direct* effect on the level of demand for commodities—in addition to the indirect effect through the interest rate already noted above. In other words, let us investigate the implications for Keynesian economics of the "Pigou effect"—or, to use terminology which I now find preferable, of the "real-balance effect in the commodity market."² Then, in addition to the rightward shift it causes in the LL curve, the decrease in the price level generated by the unemployment which exists at the point T_3 will also cause a rightward shift in the IS curve: for the greater real quantity of money will *increase* the demand for consumption goods—which means that it will *decrease* the level of savings out of any given level of income, and thus require a higher rate of interest in order to bring it (savings) into equality with investment at that income level. Hence even in the "liquidity trap" case of Fig. 2 there is no reason for the system to remain stuck at the income level Y_3 . Instead, the continued fall in the price and wage levels can—in principle—shift the IS curve rightwards to I_1S_1 in Fig. 2, so that it intersects the LL curve at the full-employment level Y_1 .

This is the argument presented in detail in Chapters XIII–XIV of my book. Unfortunately, Professor Hicks misses its main point. For in his present article—just as in that of 1937—an increase in the real quantity of money is assumed to affect the workings of the economy only through its effect in shifting the LL curve rightwards; there is no recognition of its effect on the IS curve.³ Needless to say, this omission precludes Professor Hicks from correctly evaluating the argument he is attempting to review. Correspondingly, it leads him to the erroneous conclusion that the validity of the automatic full-employment mechanism depends on the shape of the LL

¹ F. Modigliani, "Liquidity Preference and the Theory of Interest and Money," *Econometrica*, Vol. XII (1944), pp. 45–88.

² *Money*, p. 21, footnote 11. In a recent article, E. J. Mishan has claimed that these two effects are different ("A Fallacy in the Interpretation of the Cash Balance Effect," *Economica*, Vol. XXV (1958)). It seems to me, however, that Mishan's argument is based on misunderstanding of the nature of the demand function. An attempt to derive such a function from microeconomic analysis (something Mishan does not do) would rapidly make clear that there is only one effect.

³ Hicks (1957), p. 281, especially footnote 1, where Hicks identifies the real-balance effect of my analysis with the shift in the LL curve!

curve—and that if this curve is horizontal in its left-hand segment, then the return to full employment cannot be assured.¹ As has been explained

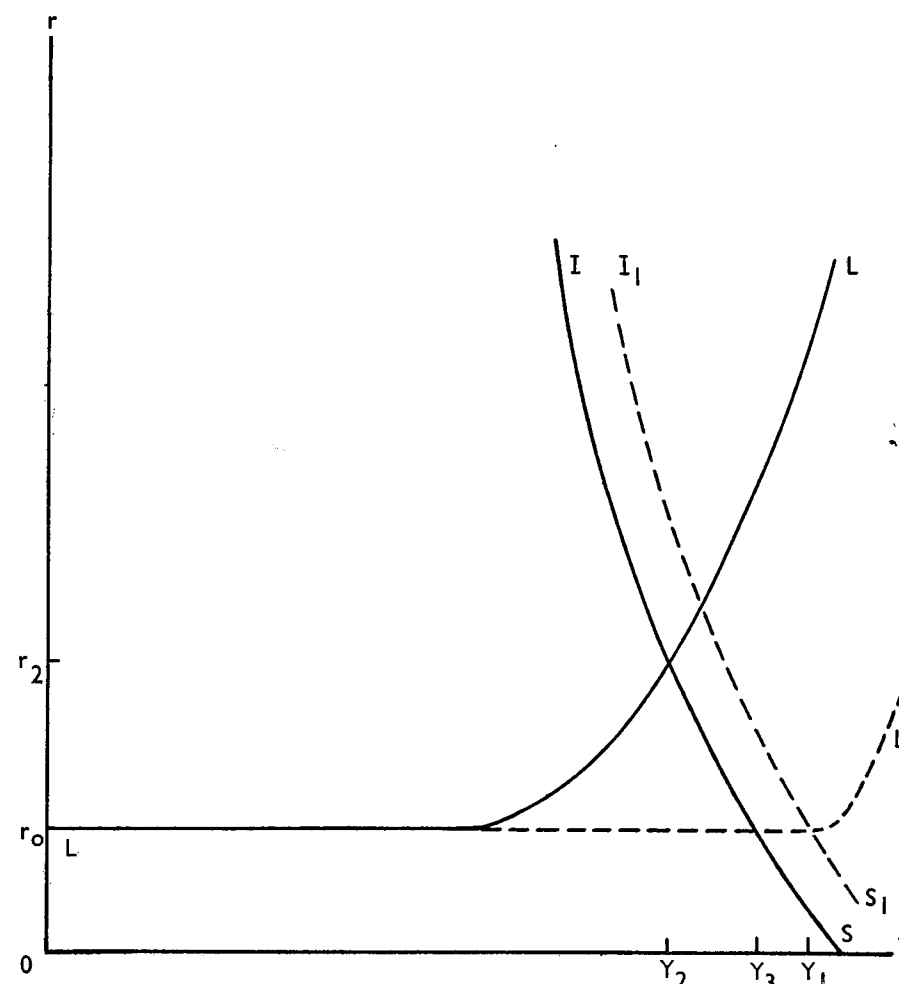


FIG. 2

again above—and as should have been clear from Chapters XIII–XIV of the book that was under review—this just is not so.^{2,3}

¹ Hicks (1957), pp. 287–8.

² Indeed—though this is unfortunately overlooked by Professor Hicks in his review article (pp. 279–80)—my original argument in these chapters makes use of his $IS-LL$ diagram—properly modified. This is explicitly pointed out in *Money*, pp. 227 and 229 (notes 19 and 21, respectively).

³ It should be emphasised that Hicks' position cannot be salvaged by assuming that the IS curve represents the "savings = investment" condition with the real-balance effect solved out by substitution from the liquidity-preference equation. For the IS curve corresponding to such a "solved-out" equation would not have the uniformly negative slope that is such a crucial element of Professor Hicks' argument. In particular, it can be shown that such an IS curve becomes horizontal at the minimum rate of interest. This is consistent—as it must be—with the shift of the IS curve to I_1S_1 in Fig. 2.

We now come to a related question: If the real-balance effect can—in principle—restore the economy to a full-employment position, what happens to the Keynesian Revolution? The answer—as I see it—is that the Revolution goes on—though on a somewhat different plane. In particular, the interest of Keynesian economics shifts from “unemployment equilibrium” (which is the concern of the Hicks 1937 article) to unemployment *disequilibrium*. And the main message of Keynesian economics becomes that the automatic adjustment process of the market (even with the real-balance effect—and even when supplemented by monetary policy) is too unreliable to serve as a practical basis of a full-employment policy. In other words, though the real-balance effect must be taken account of in our *theoretical* analysis, it is too weak—and, in some cases (due to adverse expectations) too perverse—to fulfil a significant role in our *policy* considerations.

In a way, this shift of interest actually strengthens the Revolution: for it makes it independent of such special assumptions as absolute wage rigidities and the existence of a “liquidity trap.” Both these assumptions aggravate Keynesian unemployment; but they are not necessary for the validity of Keynes’ argument as conceived here. Thus the foregoing interpretation really attributes much more general validity to Keynesian economics than that attributed by the Hicks–Modigliani interpretation.¹

There is a further point that should be mentioned with reference to the “liquidity trap.” This has frequently been presented as an economic phenomenon: as due to the shape of the demand curve for money. Actually, though, it is a political phenomenon. Monetary authorities cannot drive the rate of interest below the “minimum” rate corresponding to the “trap” because to do so would mean that they would become “the sole debt-holder—and, by that very fact, the sole lender—in the economy”: an outcome completely at variance with the political motivation of monetary policy in a free economy.²

As indicated, the three preceding paragraphs summarise views elaborated in the book that was under review by Professor Hicks. I repeat them here because I gather from Professor Hicks’ article that he would now be ready to subscribe to them.³ Unfortunately, however, he states this agreement

¹ This and the preceding paragraph summarise very briefly the discussions in *Money*, pp. 233–40. The main points of this discussion were already presented some years ago in my “Price Flexibility and Full Employment,” *American Economic Review*, Vol. XXXVIII (1948), pp. 562–4.

² *Money*, pp. 248–9. Keynes, of course, did not believe that the limitation of the “liquidity trap” had ever actually been confronted (*General Theory*, p. 207). In this context it might be noted that even during the British “cheap-money” episode of 1931–39 the monetary authorities never acquired more than 15% of outstanding government securities (see E. Nevin, *The Mechanism of Cheap Money* (Cardiff, 1955), p. 180). A similar statement holds for the Federal Reserve authorities in the United States (see *Banking and Monetary Statistics* (Washington, D.C.: Board of Governors of the Federal Reserve System, 1943), p. 512, Table 149). In view of this fact—and the general deflationary situation existing in the thirties—it is difficult to believe that the monetary authorities then could not have driven interest down even further by increasing their holdings of government securities. Cf. also Nevin, *op. cit.*, pp. 73–4, 96–8, and F. W. Paish, “Cheap Money Policy,” *Economica*, Vol. XIV (1947), pp. 167–8.

³ Hicks (1957), pp. 288–9.

in such a way as to create the impression that he is actually in disagreement!¹ This is a pity. For these issues have now been argued for so long that it would be a great help to all concerned (as well as a great saving of time and paper) if it were to be realised that in actual fact substantial agreement has been reached upon them.²

¹ As a case in point, the reader might find it instructive to compare the following two passages—the first of which has just been referred to in the preceding paragraph:

“... if the government is willing to pursue a sufficiently vigorous open-market policy—one that encompasses private as well as government bonds—there is no reason why it should not be able to drive interest down as low as it wants. But things are not quite so simple. By driving interest down below the “minimum” rate, the government also drives all private individuals out of the bond market. In brief, it succeeds in pushing interest down below the level at which “almost everyone prefers cash to holding a debt” only by itself becoming the sole debt-holder—and, by that very fact, the sole lender—in the economy. It thus negates the whole meaning of a policy designed to enable government to influence the overall level of activity in the economy with a minimum of direct intervention. Thus there is a limitation on the ability of monetary policy to reduce interest. But it is a limitation which originates in political, and not economic, factors. Furthermore, it is highly unlikely that this limitation has ever yet endangered the efficacy of monetary policy” (*Money*, pp. 248–9).

“When it comes to this crucial point [*i.e.*, the driving of interest below its ‘minimum’ level] it is a pity that Patinkin does not argue his case more fully; in particular, it is a pity that he does not test it by seeing how it looks when applied to less schematic models, which get a little closer to reality. If he had done so, I believe he would have seen that his ‘equilibrium’ required not only that the monetary authority should buy up all the ‘bonds’ which were being used as money substitutes, but that it should go to the point of superseding all the financial intermediaries—banks and other financial institutions—transactions between which regulate the whole system of interest rates which exist in reality. . . . The most that Patinkin can claim to have established is that the rate of interest might be forced still lower if that financial system were abolished, and the supply of funds to business were made directly by government. Whether or not that is the case—how far such a system is itself reconcilable with Patinkin’s other assumptions—does not, in my view, affect the main issue. The effective answer to Patinkin is that in the applications we want to make we do not normally desire to proceed to such extremes” (Hicks (1957), p. 288).

² Some other minor errors and misunderstandings in Professor Hicks’ article may be briefly indicated here. (1) His implications to the contrary notwithstanding, the dichotomy he refers to on pp. 283 and 284 (between the rate of interest and money national income?) has nothing at all to do with the invalid dichotomy between relative and absolute prices (in a full-employment model *without* a rate of interest!) described in my book (pp. 107–8). (2) Hicks’ assertion that the temporary lowering of the rate of interest (after a monetary expansion) was considered by classical economists to be the result of “forced savings” (p. 283) completely ignores the detailed references to the literature (assembled in Chapter XV: 1 and Note J of my book), which show that this temporary lowering is the direct implication of the loanable-funds theory with which classical economists worked—and has nothing at all to do with “forced savings.” (3) Furthermore, Hicks also ignores the citations reproduced in Note J, which show that such economists as Mill, Nicholson, Wicksell and Pigou were quite ready to concede that “forced savings” could have a *permanent* effect on the interest rate, and not only a temporary one. (4) Finally, Hicks implies that the transactions demand is independent of the rate of interest—and that the validity of the quantity theory depends on the demand for money being independent of the rate of interest (pp. 284 and 287 (line 11)). Both of these statements are incorrect (*Money*, pp. 78, 86–95 and 172–6).

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MR. LANCASTER ON WELFARE AND CHOICE

MR. K. LANCASTER has given ¹ an interesting account of how one could "consider welfare in terms of expanded choice and prove the various welfare propositions in terms which do not involve any indifference or utility apparatus at all." I would like to draw attention to a difficulty that arises in doing so.

Mr. Lancaster bases his exposition on the following definition: "An individual's welfare has *unambiguously* increased from situation I to situation II if his choice has expanded as a result of the change, that is, if, in situation II he can have

"(a) what he chose in situation I, and

"(b) at least one choice not available to him in situation I."

Now consider any two situations each of which permits some choices not available in the other (the situations shown in Mr. Lancaster's Fig. 1 would do) and suppose that an individual makes the same choice (possibly *S* in Mr. Lancaster's Fig. 1) in each situation. It follows that moving from I to II increases his welfare, and so does moving from II to I. Many strange consequences follow from this. For instance, this lucky individual can have his welfare continually increased (by continually changing from I to II and back again) without ever changing his consumption at all!

It all depends, of course, on his making the same choice in both situations, and he will do this only if he has an indifference curve with a kink (discontinuity of the first derivative) at the appropriate point. One could avoid this by demanding that he actually make a different choice before one judged his welfare to have increased; but this would be of no use in general welfare propositions, one would have to wait and see what he did before deciding whether he was better off. Alternatively, one could assume that his indifference curves were free of kinks; but that . . .

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MR. LANCASTER ON WELFARE AND CHOICE

MR. LANCASTER's recent article ² has shown that it is possible to give an account of some important propositions in welfare economics without using "indifference curves and preference orderings." It is not clear in his

¹ "Welfare Propositions in Terms of Consistency and Expanded Choice," *ECONOMIC JOURNAL*, September 1958.

² Kelvin Lancaster, "Welfare Propositions in Terms of Consistency and Expanded Choice," *ECONOMIC JOURNAL*, Vol. LXVIII, September 1958, pp. 464-70.

discussion whether he regards this as an improvement merely in elegance of exposition (about which opinions may legitimately differ), or whether he regards it as a reduction in the number of prior axioms required, which would be a gain in logical elegance. But it is difficult to escape the feeling that Mr. Lancaster believes that he has accomplished the latter. I shall argue that such a belief would not be entirely justified; there is an ordering, and there are equivalence curves, still lying about the place.

Mr. Lancaster's proof uses only the Samuelson consistency axiom of revealed preference (*RP*) to show that, in the two-commodity, two-person world, perfect competition results in the achievement of the optimum conditions for exchange, which is a classical result of welfare economics. Now I have recently shown ¹ that: (i) the extended axiom of *RP* (which postulates consistency of choice for *any* finite chain of situations); plus (ii) the budget identity; plus (iii) an assumption that all individual demand functions are continuous in prices and income, imply that the consumer does in fact *order* all points of the commodity space by the extended *RP* relation, and that this ordering is continuous.² In other words, there does exist a (revealed) preference ordering, and this ordering may be represented by a continuous function, unique up to the usual monotonic transformation. The contours of this function—the "equivalence curves"—will possess all the usual properties of indifference curves. Any two points *a* and *b* on a given curve are such that *a* is not (extended) *RP* to *b*, nor *b* (extended) *RP* to *a*.

So if we can show that Mr. Lancaster has in fact made the three assumptions above, we will have shown that there does exist an ordering, which is in some sense a preference ordering. To take the first assumption, we have said that Mr. Lancaster assumes Samuelson's consistency axiom. Hugh Rose has recently shown that, in the two-commodity case, the Samuelson and the extended consistency axioms become identical.³ So the first axiom has been assumed. Secondly, it is clear in Mr. Lancaster's exposition that he requires that choices only be made from points on the budget lines of the consumers, which is the second assumption.

The third assumption is not quite so easy to spot. If it is not satisfied, it means that for at least some rotation of some budget line, the chosen batch remains unchanged. Let us assume that one of the two consumers, say *A*, has continuous demand functions while the other, *B*, does not. Then it is not difficult to show that free exchange may *not* lead to a Pareto

¹ In a paper called "Complete Ordering and Revealed Preference," to appear in the *Review of Economic Studies*. This follows up Houthakker's pioneer article in *Economica*, 1950. For definitions of the extended *RP* relation and axiom, the reader is referred to my note in *Economica*, May 1959.

² I assume, as Mr. Lancaster implicitly does, that each batch will be chosen in some price-income situation.

³ Hugh Rose, "Consistency of Preference; The Two-Commodity Case," *Review of Economic Studies*, Vol. XXV, February 1958, pp. 124-5.

position. Consumer B can be "indifferent" between two price-ratios (in the sense of both leading him to the same chosen batch) while A cannot. There will be no inducement leading B to choose one such price-ratio rather than another; he may simply stick, out of pure cussedness, at a price-ratio which is optimal for him but not for A. This not to deny that the usual Pareto position will exist, a proposition which is not in doubt. The point is that free exchange under perfect competition may not lead to the welfare optimum in the discontinuous case. It appears necessary, then, to assume that the demand functions *are* continuous.

Hence all three assumptions are fulfilled, and a continuous *RP* ordering exists. What if there are more than two commodities? (The case of more than two consumers is obviously straightforward if there are only two commodities.) Mr. Lancaster leaves it "to the intuition to perceive that this [proof] can be generalised for any number of individuals and commodities by a progression of two by two comparisons." Maybe; but intuitions are notoriously tricky. *My* intuition, for what it is worth, tells me that there may be troublesome problems of inconsistency of choice in the general case if the *extended* axiom is not used; and this would imply that an *RP* ordering exists. But this conjecture may well be wrong. If Mr. Lancaster will provide a proof for the general case, we will soon know.

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SPECULATION, ARBITRAGE AND STERLING: A COMMENT

THE principal thesis of J. Spraos's article on "Speculation, Arbitrage and Sterling" in the March 1959 issue of this JOURNAL, namely that *rational* speculation against sterling must be held innocent of adverse effects on the reserves, seems to rest on an unusual view of rationality. Spraos correctly argues that if there are two means to the *same* end, the cheaper of the two will be chosen. *If* (and here his argument seems to slip) the "end" is a profit, or the avoidance of a loss, upon an expected devaluation of sterling, and the "means" are either a spot conversion of sterling into dollars or a forward sale of sterling for dollars, and if the monetary authorities see to it that the latter of the two means is less costly, *then* rational speculators will not convert sterling into dollars, but will sell it forward instead.

However, this does not follow from speculators being rational. For by choosing the cheaper of two speculative positions, a speculator can maximise his expected *rate* of profit per unit (pound) of the position. But if he is

rational in the usual sense his *maximand* will be not the rate of profit, but total profit. These are not, except possibly by coincidence, the *same* "ends." It is easy to show that, with given marginal cost and (expected) marginal revenue schedules, total profit is not normally maximised where the rate of profit *per unit* of output is highest; the size of the speculative position is analogous to *total* output.

In the specific case at issue, if the forward discount on sterling is kept narrower than the London-New York interest differential a speculator will expect to profit more per £1 of an open forward position than per £1 of an open spot position. Nevertheless, he will expect to profit more altogether if he opens both positions. This could fail to be true only if the size of the open forward position which the market allowed him to adopt were unlimited. But it cannot be unlimited if the market wants at least some assurance (formal or informal collateral) that a forward contract will in fact be honoured. A very bearish holder of external account sterling (or a "trader," or any non-resident with access to sterling borrowing facilities) might therefore well *both* move funds from London to New York *and* sell as much sterling forward as he could. Provided the cost of the dearer method still fell short of the expected profit from devaluation, this would be his optimum procedure whichever of the two methods was dearer.¹

This is not to say that judicious entry by the authorities into the forward market cannot regulate the reserves. It can do so within perhaps quite wide limits, not by diverting *all* speculation to the forward market, but by *offsetting* what spot speculation remains, plus what current-account balance-of-payments swings it is deemed appropriate to offset, through variations in the cost of cover against the exchange risks of short-period lending and borrowing.²

It also follows from the above argument that even under "one-way" exchange risk there is no logical contradiction between cheaper money in Britain than in America, on the one hand, and a covered arbitrage margin

¹ By analogy: if someone wishes to speculate on a fall of bond prices, he may sell bonds spot, or forward, or both. Suppose he can make credit arrangements enabling him to sell forward an amount up to *n* times his own net worth. He has the further choice of holding his net worth either in bonds or in cash. Spraos would require him to keep it in bonds, because holding cash involves forgone interest. Should, however, the speculator be deterred by the marginal opportunity cost (forgone yield) of holding cash? If he expects bonds to depreciate by more than the forgone yield, he will presumably sell bonds spot as well as forward, and hold cash. Thus he can reap a speculative profit on *n* times instead of merely *n* - 1 times his net worth, obtaining it at a marginal cost which, though rising, still falls short of the marginal profit. Substitute "sterling" for "bonds," "dollars" for cash and "arbitrage margin in favour of sterling" for "marginal opportunity cost," and we have the case treated by Spraos. Clearly, the claim that management of the forward exchange rate can stop all spot speculation against sterling is unfounded if speculators are rational, and *a fortiori* if they are not.

² By varying this cost (the discount on forward sterling) and keeping interest rates constant, the same mechanism could be worked which, in the classical gold standard world of complete faith in exchange stability, required actual variations in domestic interest rates before it could, in Goschen's picturesque phrase, "draw gold from the Moon."

in favour of London, on the other: for forward sterling *can* be lifted to a premium without the cost of doing so necessarily becoming "prohibitive."¹

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MARGINAL EFFICIENCY OF CAPITAL: COMMENT

IN the September 1958 issue of the *ECONOMIC JOURNAL* Messrs. J. D. Pitchford and A. J. Hagger have a "Note on the Marginal Efficiency of Capital" devoted to the point that the usually accepted measure of that concept may not have a unique value. They show that there may not be a unique solution for r in the following expression, where the Q_i represent the annual terms in a net return sequence, S is the cost of an asset and r is identified with the marginal efficiency of capital:

$$S = \frac{Q_1}{1+r} + \frac{Q_2}{(1+r)^2} + \dots + \frac{Q_n}{(1+r)^n}$$

Actually, this point has been recognised and extensively discussed for some years in the United States, though primarily in journals which were probably inaccessible to Messrs. Pitchford and Hagger in Australia. The discussion began with the well-known article by J. H. Lorie and L. J. Savage, "Three Problems in Rationing Capital," *Journal of Business* (University of Chicago), Vol. XXVIII (October 1955), especially pp. 236-8, which are devoted precisely to showing the possibility of and discussing the significance of dual solutions for r in the expression above. Even in that article, the point in question was a rediscovery rather than a new contribution, since the same phenomenon had been remarked (though its implications were not emphasised) by Samuelson in "Some Aspects of the Pure Theory of Capital," *Quarterly Journal of Economics*, Vol. LI (1936-37), at p. 475. A rather complete discussion of this and related difficulties of the concept of "marginal efficiency of capital" (or "internal rate of return") as an investment decision criterion will be found in my paper, "On the Theory of Optimal Investment Decision," *Journal of Political Economy*, Vol. LXIV (August 1958). Perhaps the final word will be the article by Martin J. Bailey.

¹ Both Spraos (*op. cit.*) and Bent Hansen ("Interest Policy and Foreign Exchange Policy," *Skandinaviska Banken Quarterly Review*, October 1958, p. 119) regard the contrary as almost axiomatic.

It is important to stress that the contrary view is due to an analytical oversight. Since collateral requirements, whether formal or informal, *can* limit the volume of forward sales of currency, they can also limit the cost of buying it at a premium. Under "one-way uncertainty," the sole buyers would *ex hypothesi* be the United Kingdom authorities. As monopsonists, they could largely control the cost *both* by varying the collateral margin (if need be calling for 100% and by demanding one type of collateral rather than another. *E.g.*, by insisting on sterling rather than dollar collateral, they could severely restrict, if not eliminate altogether, forward sales by non-residents who are staying out of sterling. Thus the privilege of selling forward sterling at a premium would be confined to inward arbitrageurs and others who *could* draw dollars from the reserves but refrain from doing so and in this way oblige by causing the reserves to be higher than they otherwise would be. This suggests that cheap money in this country is not only logically consistent with the maintenance of an inducement to hold convertible funds in London rather than elsewhere, but that in practice the cost to the current balance of payments of maintaining the inducement *via* a forward premium need not be much higher than *via* higher interest rates.

"Formal Criteria for Investment Decisions," forthcoming in the *Journal of Political Economy*.

The difficulty about dual (or, more generally, multiple) solutions for r in the equation above is no mere curiosity but rather the symptom of an underlying defect in the concept: investment alternatives cannot in general be evaluated (*i.e.*, adopted or rejected) or even simply ranked by a measure derived solely from their constituent cash-flows (the "internal" rate), but only in comparison with the market or the productive alternative opportunities available. Such a comparison is automatically made when investments are evaluated by present-worth computations performed with the appropriate discount rate derived from these alternative opportunities. Simple ranking of investments according to their "internal rates" will not, in general, lead to correct results, nor will evaluation of an investment by comparison of its "internal rate" with the rate of interest over project life. The "internal rate" concept can be used correctly, but only after a more sophisticated redefinition, as provided especially in Bailey's forthcoming work.

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CAPITAL EXPORTS AND GROWTH FOR ARGENTINA: A COMMENT

1. In his recent note on "Capital Exports and Growth for Argentina, 1880-1914,"¹ Mr. A. G. Ford sets himself the task of assessing the effects of foreign loans on the Argentine economy during that period, with special reference to the question of how far the Argentine experience involved "excess borrowing."

The purpose of this note is to supplement Mr. Ford's stimulating analysis in two ways: First, I make use of Professor J. H. Williams' estimates of the Argentine balance of payments, to present a measure of the amount of excess borrowing in Argentina during the formative period of 1880-1900. It appears that excess borrowing was rather large throughout this period. A second object of the note is to suggest that it is both useful and necessary on analytical grounds to distinguish explicitly between "excess borrowing"² and "unnecessary borrowing,"² and also between net capital imports,

¹ Cf. *ECONOMIC JOURNAL*, September 1958, pp. 589-93.

² It may be recalled here that, in an earlier article in this *JOURNAL*, I defined "excess borrowing" as "imports of capital which arise otherwise than in connection with a need to finance such supplies of goods and services from abroad as are, in a given state of tastes, techniques and available resources, and of a given amount and composition of output and growth, both indispensable and unrequitable" (cf. *ECONOMIC JOURNAL*, September 1957, p. 432). Thus, excess borrowing occurs when the rate of long-term borrowing or of asset-liquidation exceeds the difference between the actual rate of investment and either actual or *potential* domestic saving in the borrowing country, domestic thriftiness being given. Excess borrowing must be distinguished from "unnecessary borrowing," since the "rules of the game," or other factors in the situation, may make excess borrowing unavoidable (cf. *op. cit.*, pp. 436-7).

necessary borrowing and borrowing as a cause of growth, when considering the implications of the kind of material presented by Mr. Ford.

2. It is possible to document Mr. Ford's statements concerning the way in which "the habit of borrowing abroad" gave rise to excess borrowing in Argentina by means of the following table, which can be pieced together from Professor J. H. Williams' *Argentine International Trade under Inconvertible Paper Money*:

Estimated Balance of Payments of Argentina, 1881-1900
(All figures in million gold pesos)

Year.	(1) Exports (f.o.b.).	(2) Imports (c.i.f.).	(3) Balance of trade.	(4) Interest and amortisa- tion.	(5) Net gold exports (+) or imports (-).	(6) Sum of columns (3)-(5).*	(7) New borrow- ing.	(8) Apparent net short- term capital movement (export- import+).
1881	57.9	55.7	+ 2.2	-12.0	—	- 10.8	+ 14.1	- 3.3
1882	60.4	61.2	- 0.9	-15.7	—	- 16.6	+ 25.3	- 8.7
1883	60.2	80.4	-20.2	-19.5	—	- 39.7	+ 47.4	- 7.7
1884	68.0	94.1	-26.0	-27.6	- 0.4	- 54.0	+ 39.7	+ 14.3
1885	83.9	92.2	- 8.3	-22.6	+ 2.1	- 28.8	+ 38.7	- 9.9
1886	69.8	95.4	-25.6	-26.8	-12.4	- 64.8	+ 67.6	- 2.8
1887	84.4	117.4	-32.9	-37.3	+ 0.1	- 70.1	+153.5	- 83.4
1888	100.1	128.4	-28.3	-49.5	-36.1	-113.9	+247.8	-133.9
1889	90.1	164.6	-74.4	-59.8	+16.7	-117.5	+153.6	- 36.1
1890	100.8	142.2	-41.4	-60.2	- 1.9	-103.5	+ 45.4	+ 58.1
1891	103.2	67.2	+36.0	-31.6	- 7.6	- 3.2	+ 8.2	- 5.0
1892	113.4	91.5	+21.9	—†	- 4.5	—†	—†	- 1.5 †
1893	94.1	96.2	- 2.1	—†	- 3.9	—†	—†	+ 26.2 †
1894	101.7	92.8	+ 8.9	—†	- 2.9	—†	—†	+ 24.6 †
1895	120.1	95.1	+25.0	-38.1	- 4.5	- 17.6	+ 17.2	+ 0.4
1896	116.8	112.2	+ 4.6	-39.9	- 3.9	- 39.2	+ 37.1	+ 2.1
1897	101.2	98.3	+ 2.9	-44.0	+ 4.3	- 36.8	+ 38.3	- 1.5
1898	133.8	107.4	+26.4	-50.5	- 5.7	- 29.8	+ 46.1	- 16.3
1899	184.9	116.9	+68.1	-54.7	- 1.7	+ 11.7	+ 25.0	- 36.7
1900	154.6	113.5	+41.1	-58.6	- 6.7	- 24.2	+ 27.5	- 3.3

* Data on immigrants' remittances, tourists expenditures, etc., are not given by Williams. The net gold movement 1881-83 is assumed to have been negligible.

† Williams provides estimates only for the balance of new borrowing minus interest and amortisation payments for the years 1892-94.

Source: J. H. Williams, *op. cit.*, pp. 183, 45, 101, 136, 152, 49, 110, 111, 154.

Columns 4, 7 and 8¹ of this table afford *prima facie* evidence for the presence of large-scale excess borrowing in Argentina, particularly during the period of the 1880s, when borrowing and development activity were both at their peak. It can be calculated from the table that the proportion of new borrowing which was utilised to meet the Argentine trade and shipping deficit during the decade 1881-90 (inclusively) was no more than 30.7%.²

¹ This last column was calculated by myself from the data provided by Williams.

² While it is true that immigrants' remittances are not included in Column 6 of the table (they were not estimated by Williams), I in any case regard this exclusion as conceptually appropriate. They are best regarded as currently exported savings, which are thus appropriately added to column 8.

Interest payments, which, as can be seen, were dramatically swollen by the new borrowings themselves, accounted for 39.7% of new borrowings over the period, while gold imports (3.8%) and, most importantly, capital exports (including emigrants' remittances) accounted for the remaining 29.6%. These data suggest that a great deal of Argentine borrowing during the 1880s represented excess borrowing rather than an inability of the Argentine to match investment with domestic saving. It should be noted, also, that the comparatively modest liquidation of Argentine-owned foreign assets which occurred during the early 1890s took place at a time when Argentine savings were depressed by the effect of a world recession on her exports, while excess borrowing associated with Argentine short-term lending was apparently resumed in the closing years of the century, when world activity was reviving.

A complicating feature of the Argentine situation was that the period was one of inflation, with a domestic paper currency in use and with speculation in gold very active. This may account for the destination of a considerable part of Argentine capital exports in the late 1880s.¹

3. Mr. Ford's impression that savings in the Argentine were small relatively to the requirements of investment in railway building could also be tested if it were possible to construct annual estimates of the value of domestic investment in Argentina for this period, for use in conjunction with the above table, and other relevant data, *e.g.*, the size of the initial foreign-exchange reserves of Argentina in the early 1880s. Pending such a test, one should, perhaps, regard the question of the degree of adequacy of the overall rate of saving of the Argentine during this period as an open one.

4. It is important to grasp the point that at particular periods of history excess borrowing is *not necessarily* synonymous with unnecessary borrowing, because excess borrowing (*i.e.*, borrowing which *does not reflect a deficiency* in actual or potential domestic saving in the borrowing country) may be unavoidable for some reason or other.² The "rules of the game" enshrined in nineteenth-century canons of finance, and the degree of sophistication of the monetary authorities being what they were, it may be the case that *all* the Argentine borrowings of this period must be looked upon as having been necessary for financing the actual real outlays made in Argentina during the period studied by Mr. Ford, no matter how large the proportion of excess borrowing had been as a proportion of total borrowings, and no matter how small actual borrowings were in relation to either actual or potential Argentine domestic saving.

¹ Argentinians also acquired foreign assets in return for the sale of domestic assets, including a large part of their railway system, during this period. Cf. J. H. Williams, *op. cit.*, pp. 86-7.

² For a discussion of various ways in which this can happen, cf. my article on "Capital Exports and Growth," *ECONOMIC JOURNAL*, September 1957, pp. 436 *et seq.*

Nor does excess borrowing have anything to do with borrowing for unnecessary or wasteful purposes, on the definition I have proposed. Mr. Ford appears to have misinterpreted me on this point. Excess borrowing relates to the financing of *whatever* real outlays did, in fact, occur. Cf. *ibid.*, p. 432.

It is instructive to consider the significance of Mr. Ford's insistence on the aversion of Argentine wealth-owners to the shouldering of railway risks¹ in the light of the distinction just made. By themselves, the preferences of Argentine wealth-holders can provide no evidence for *absence of excess borrowing*, since such preferences could have been entirely compatible with a rate of actual or potential domestic saving in Argentina sufficient or more than sufficient to match the volume of investment that did, in fact, occur. Indeed, this aversion on the part of Argentine *rentiers* is actually suggestive of the *presence* of excess borrowing, since it must be regarded as a factor tending to drive Argentine borrowers to make use of capital markets abroad, irrespective of the degree of adequacy of the domestic rate of saving. Moreover, it has been shown in the foregoing that excess borrowing did, in fact, occur on a large scale in the Argentine during this period. But this same aversion *could* provide evidence of the *absence of unnecessary borrowing*, if it were legitimate to assume, *in addition* to the existence of this aversion, one or both of the two following things:

(i) that neither the Argentine Government, nor Argentine private entrepreneurs² were interested in promoting the construction of railways, so that entrepreneurial initiative had to come from abroad, and when it did come from abroad had to be imported jointly with foreign financing, and/or

(ii) that, whatever may have been the willingness of the Argentine Government and of private entrepreneurs to engage in railway construction, the effective financial "rules of the game" were such as to make the domestic provision of finance for such schemes impossible owing to the unwillingness of the Argentine *rentier* to provide it.

The realism of the foregoing assumptions should not be taken for granted. At some periods in the nineteenth century at least the Argentine Government clearly did take a great interest in railway development.³ Moreover, the "unsoundness," by orthodox standards, of the contemporary Argentine domestic currency arrangements suggests that the financial rules of the game were not taken too seriously in that country at that time. There is evidently scope for further research here. In any case, it is clear that we should interpret Mr. Ford as taking the view that it was *unnecessary borrowing* that was small in the Argentine in nineteenth-century conditions. This is a quite distinct proposition from saying that excess borrowing was small.

5. A further point of some general significance which appears as the corollary of this argument is that if we are interested in measuring the extent of *necessary borrowing*, the volume of *net* capital imports is, in itself, irrelevant. What matters is whether the observed rate of *gross* borrowing was necessary or not. Finance can be a bottleneck irrespective of the degree of adequacy

¹ A. G. Ford, *op. cit.*, pp. 590, 593.

² As distinct from *rentiers*.

³ Cf. J. H. Williams, *op. cit.*, pp. 38 *et seq.* and 87.

of actual or potential savings in the borrowing country. When this is the case it is the corresponding *gross* borrowings which should be regarded as the correct measure of the contribution made to domestic capital for formation by the action of foreign investors. But the *modus operandi* of this contribution has nothing to do with the transfer of borrowings in real terms and the supplementation of domestic saving by the thriftiness of investors in lending countries.

It is a characteristic of the classical literature of the history of international capital movements that it focuses attention on *net* capital movements, and equates such movements with contributions to growth in borrowing countries. Modern analysis does not permit us to make such a simple assessment. When *unnecessary* excess borrowing is taking place net capital imports overstate this contribution. When excess borrowing is made *necessary* by the rules of the game, net capital imports may, in some cases, be said to understate this contribution.¹

Historically speaking, the rules of the game have become increasingly so modified in practice, as sophistication in monetary matters has increased since the heyday of international investment in the nineteenth century, that the volume of gross borrowing made necessary by the rules of the game has increasingly tended to approximate to the *net* value of capital imports. Hence a given rate of *net* capital imports through time could, in a sense, be regarded as connoting *declining* foreign contributions to growth in borrowing countries. But this way of speaking seems rather unreasonable, because, from a modern point of view, the nineteenth-century rules of the game seem unreasonable. When unnecessary disciplinary methods cease to be used in order to ensure the good behaviour of children we do not say that they are receiving less help and attention.

JOHN KNAPP

University of Manchester.

IS FACTOR SUBSTITUTION A CRIME, AND IF SO, HOW BAD? REPLY TO PROFESSOR EISNER

PROFESSOR EISNER's broadside² seems to be aimed more at scoring debating points than at any substantial target. This puts me at a tactical disadvantage. If I merely assert that I have not said the things he attacks me for, I appear spinelessly to be yielding to pressure. Otherwise I am forced into defending a position I have never advocated. I propose to resolve the dilemma by doing both things. First, I show that Eisner has mis-

¹ Such understatement may be said to occur in cases when net capital imports are less than gross capital imports made necessary by the rules of the game, but it is possible for necessary excess borrowing to occur even if gross and net capital imports are equal.

² Robert Eisner, "On Growth Models and the Neo-Classical Resurgence," *ECONOMIC JOURNAL*, Vol. LXVIII (December 1958), 707-21. My paper was "A Contribution to the Theory of Economic Growth," *Quarterly Journal of Economics*, Vol. LXX (February 1956), 65-94.

interpreted my earlier paper. Then I suggest that there may in fact be a real issue between us, to whose solution Eisner has contributed only assertion.

An important part of Professor Eisner's refutation rests on his announcement that he will at once become "reasonable," "more classical" and "more Keynesian" than I (pp. 713-14). He accomplishes this turn by suggesting that the aggregate production function might be such that with any given amount of labor to be employed, the net marginal product of capital might fall to zero with some finite amount of capital. Or at least might fall below the minimal level to which the interest rate can be driven in the case of the "liquidity trap." As against this, I note that I have written at one point (pp. 75-6):

"At the very least one can imagine a production function such that if (the capital-labor ratio) exceeds a critical value $r_{\max}$, the marginal product of capital falls to zero. . . . For values of (the rate of growth of the labor force) below this zone the end result would be redundancy of capital, for values . . . above this zone, redundancy of labor. To the extent that in the long run factor proportions are widely variable the intermediate zone of growth rates will be wide."

Or at another point (p. 93):

"In the famous 'trap' case where the demand for idle balances becomes infinitely elastic at some positive rate of interest . . . the result will be underutilization of capital if the interest rate becomes rigid somewhere above the level corresponding to the equilibrium capital-labor ratio."

But this is mere technique. The heart of the matter is Eisner's assertion (p. 720) that I "reject . . . the *possibility* of the growth model problems" and that I believe "the economy will be free to adjust to whatever rate (of growth) is appropriate for full employment" (p. 708). He perhaps did not notice a section of my paper headed "Qualifications" which begins:

"Everything above is the neoclassical side of the coin. . . . All the difficulties and rigidities which go into modern Keynesian income analysis have been shunted aside. It is not my contention that these problems don't exist, nor that they are of no significance in the long run. My purpose was to examine what might be called the tight-rope view of economic growth and to see where more flexible assumptions about production would lead a simple model. Underemployment and excess capacity or their opposites can still be attributed to any of the old causes of deficient or excess aggregate demand, but less readily to any deviation from a narrow 'balance.'"

Is the whole tempest then merely a matter of verbal fireworks? Sometimes, as when Eisner's last paragraph expresses a view similar to mine, it seems that way. But in fact I think there is a deeper disagreement. Eisner *does* seem to believe in the "tight-rope" view. He seems to hold that the appropriate image of western capitalism is that of a system teetering in the

long run on the verge of capital saturation. (I emphasise the long run because the possibility of cyclical loss of momentum and collapse of the marginal efficiency schedule is denied by no party to the debate.) Even over long periods of time the system is incapable of teaching itself to hold just a little bit more real capital. I think that this view is incapable of explaining whole stretches of the evolution of capitalism. But that is in a way beside the point. The point is that this argument can never be settled by mere rhetoric and counter-rhetoric. It depends on such ultimately empirical questions as the nature of savings behaviour, the sources of technological progress, the formation of expectations, and others. On all these questions there is need for more evidence.

Massachusetts Institute of Technology

ROBERT SOLOW

REPLY TO PROFESSOR EISNER

I HAD thought that the indefinite article in the title of the paper of mine to which Professor Eisner takes exception¹ betokened sufficient modesty of purpose to protect me from the kind of debate he seeks to thrust upon me. Nowhere do I deny either the logical consistency of the models he defends and elaborates or the possibility that at times the economic impasse they suggest may have empirical relevance. "A rejection of the *possibility* of the [Harrod-Domar-Hicks] growth model problem" (Eisner, p. 720, his emphasis) was no part of my purpose. Nothing could be more misleading, moreover, than to attribute to my model the implication that it is always easy for the economy "to adjust to whatever rate [of growth] is appropriate for full employment" (Eisner, p. 708) without cyclical interruption or monetary-fiscal assistance.

Eisner is correct that, contrary to the impression given by my paper, constant coefficient technology is not the only source of the conclusion that the equilibrium growth path of the economy is a precarious knife-edge requiring a unique balance between the rate of saving and the growth of the labour force. The same conclusion can be obtained, given variable-proportion technology, from the assumption that the marginal product of capital stands at an irreducible minimum required to induce owners of wealth to hold capital. At this minimum, savers will absorb an ever-growing capital stock without protest; but let the rent on capital be pushed ever so little below it and they will shun capital no matter how saturated they may be with liquidity. One of the purposes of my paper was to challenge this tight-rope theory of portfolio election. As Eisner points out, this second alternative is the right interpretation of Harrod; Domar and Hicks, and most users of the hard-worked "Harrod-Domar model," seem to me to rely on constant-coefficient technology. In any event, my paper was clearly intended to apply

¹ Robert Eisner, "On Growth Models and the Neo-Classical Resurgence," this JOURNAL, Vol. LXVIII (December 1958), pp. 707-21. My paper was "A Dynamic Aggregative Model," *Journal of Political Economy*, Vol. LXIII (April 1955), pp. 103-15.

to situations where neither of these rigidities is met, but where capital is smoothly substitutable for labour in production and for monetary assets in portfolios.

There are other situations. "It is possible," I said early in my paper (p. 105),

"that there are levels of [the rent on capital] r (e.g., negative rates) so low that portfolio balance requires all wealth to be in the form of currency and that there is some level of r above which wealth-owners would wish to hold no currency. But the main argument to follow . . . concerns ranges of r between those extremes."

Eisner has examined the consequences of extreme low values of r and seems to regard this extension of my paper as a refutation. Not all the speculations of Eisner, Tobin and the whole cast of theorists in Eisner's footnotes can decide which case has the greater relevance. That requires estimates of strategic parameters of behaviour and technology. I don't have these any more than Eisner does. But I hold to the conjecture that at least some economic history and some economic future cannot be understood on his assumption that the slightest increase in the capital-labor ratio spells disaster. If I am correct, there is room for growth and cycle models based on different premises. Eisner's final paragraph indicates that he agrees with this conjecture. So what is all the shooting about?

Which economists, models and parameters are truly Keynesian? In common with others who attach great importance to this question, Eisner under-rates the neoclassical residues in the master himself. The liquidity-trap impasse, which Eisner chooses to regard as the "Keynesian heritage" to defend and build upon, is a special part and not the whole of what was, after all, billed as a *general* theory. Although Keynes expected the liquidity trap to prevent automatic recovery from deep depression, he surely did not intend it as a description of the typical state of affairs in full prosperity.

Yale University.

JAMES TOBIN

CURRENT TOPICS

THE Annual General Meeting of the Royal Economic Society was held on Thursday, July 9 at the London School of Economics. Sir Robert Hall was re-elected to the office of President. Professor C. F. Carter was elected to the Council in the place of Professor W. A. Lewis; with this exception the Council and Officers were elected without change for 1959-60. After the formal business the President addressed the Society on "Reflections on the Practical Application of Economics."

THE Secretary reported on the membership and finances as follows:

	1958.	1957.	1939.	1914.
New Fellows and Library Members elected	407	445	490	50
Fellows lost by death, resignation, or default	300	378	496	33
Number of Fellows and Library Members on December 31	6279	6172	4572	694
Total Compounders included in above total	1769	1747	1155	159
Surplus of Income over Expenditure	—	£1313	—	278
Surplus of Expenditure over Income	£56	—	38	—

WE apologise in advance for the almost certain late delivery of this issue of the ECONOMIC JOURNAL. We do not think that in this case our readers will expect an explanation.

MR. J. S. G. WILSON, Reader in Economics at the London School of Economics, has been appointed to the Chair of Economics and Commerce in the University of Hull, in succession to Professor I. Bowen, who has resigned to take up an appointment to the Chair of Economics in the University of Western Australia.

THE Trustees of the Houblon-Norman Fund, on the recommendation of their Advisory Committee, have made the following awards for 1959/60:

Research Grants

Dr. L. R. Amey, Lecturer in Industrial Economics, University of Nottingham.	The Movement of Firms between Industries.
Mr. K. F. Dixon, Westminster Bank Ltd. (Renewal.)	The Early Development of the London Money Market.
Miss A. G. Donnithorne, Lecturer in Political Economy, University College, London.	The Chinese System of Economic Planning, with special reference to International Trade.
Mr. Bernard Edwards, Lecturer in Economics and Statistics, Municipal College of Commerce, Newcastle.	The Structure of Banking in Venezuela.
Mr. J. K. Ghandhi, Research student, King's College, Cambridge.	The Extent and Impact of Hire-Purchase in the United Kingdom, 1948-1957.
Mr. C. H. P. Gifford.	The Problems of Economic Forecasting—a Case-Study in Timber.
Mr. C. D. B. Harbury, Lecturer in Economics, University of Birmingham.	The Distribution of Capital and the Laws of Succession.
Dr. G. D. Ramsay, Fellow of St. Edmund Hall, Oxford.	The Account Book of John Isham.
Mr. Peter Robson, Lecturer in Economics, Queen's University, Belfast.	The Economic Effects of Index Linking in the Money, Bond, and Insurance Markets.
Mr. Neil Runcie, Lecturer in Economics, New South Wales University of Technology. (Renewal.)	Hire-Purchase in the United Kingdom and the British Dominions.
Mr. R. M. Titmuss, Professor of Social Administration, University of London.	The History of Occupational Pension Schemes in Britain.

An offer of awards for 1960/61 will be made in due course. Further information may be obtained from the Secretary, Houblon-Norman Fund, c/o Bank of England, London, E.C.2.

THE following have been admitted to Life Fellowship of the Society:

Bainbridge, J. W.	Lerdau, E.	Peterson, K.
Barber, Prof. W. J.	Levitt, P. D.	Raj, J. S.
Eckstein, Prof. O.	Paul, Rev. T. F.	Seton, F.

THE following have been admitted to Fellowship of the Society:

Adebisi, C. O.	Gupta, Dr. V. P.	Muscatt, J.
Agama, F. H.	Harris, T. H.	Nwagbara, G. U. M.
Alabi, S. T.	Hay, E. A.	Obi, E. O.
Armstrong, R. E.	Holmes, M. A.	Opeodu, O.
Bakre, M. A.	Horne, E.	Oser, R. A.
Balassa, Prof. B. A.	Horne, Miss F.	Panduranga, V. N.
Battersby, P.	Horrigan, W.	Patel, G. M.
Bergin, F. J.	Horsten, F. A. M.	Patel, K. C.
Bloomfield, Prof. A. I.	Hurford, C. J.	Patterson, G. D.
Boan, J. A.	Ilori, S. O.	Phillips, L. A.
Boyer, R.	Ingle, G.	Portlock, N. D.
Bradlow, E. P.	Jamadar, K. M.	Pritchard, I.
Brown, A. S.	Jansen, K. F. R.	Rafi, M. N.
Catlin, E. R.	Jennings, V. E.	Rhodes, B.
Chakko, A. R.	Joshi, A. R.	Rowley, Dr. Sheila I.
Chaturvedi, S. K.	Kanabar, G. P.	Savaré, Dr. Ing. A.
Cheng Yew Kiew	Kaufman, G. A.	Saxena, P.
Cheung, L. H. F.	Kelaher, D.	Selisko, F.
Chow Chak Pui	King, D. T.	Sinclare, L. D.
Chu, J. H.	King, R. E.	Singer, Dr. L.
Collinson, J. F.	Klonarides, H.	Spiegelman, R. G.
Crawford, Miss F. A.	LeMarchant, R. C.	Stapleton, G. B.
Crotty, J. J.	Levitt, H.	Swingle, P. G.
Cummings, E. A.	Lewis, L. C.	Takeuchi, K.
Dada, P. O. A.	Liu, Dr. Tsun-yan	Tilley, V. H.
Davis, L. J.	Longfield-Jones, E.	Tobias, J. J.
Davis, O. A.	Lowe, J. R.	Truda, P. A.
Demou, Dr. F. J.	Mangan, L. J.	Victor-Smith, H.
Dutt, P. K.	Mathur, G. D.	Wan Sek Wah
Emmanuel, J. S.	Matthews, J. W.	Weinstein, M. E.
Feinstein, C. H.	McGovern, J. D.	Werner, Prof. R. O.
Frearson, K. S.	McNeil, A. L.	Weston, J. P.
Gibson, R. T.	Menzler, G.	Whitbread, T. A.
Green, R. K.	Mitra, P. T.	Yearwood, M.
Grove, D. K.	Mok Shu Wah	Zachariades, P. V.
Gupta, B.	Morgan, I.	Zapatero, D. V.

The following have been admitted to Library Membership of the Society:

Amalgamated Meat Cutters & Butcher Workmen, Chicago, 14, U.S.A.
 Balham & Tooting College of Commerce, London, S.W.17.
 Banque Centrale du Congo Belge & Ruanda Urundi, Léopoldville, Congo Belge.
 Communauté Economique Européenne, Direction Générale des Affaires Economiques et Financières, Brussels, Belgium.
 Compania Shell de Venezuela Ltd., Caracas, Venezuela.
 Consiglio Nazionale dell'Economia e Lavoro, Rome.
 Constantine Technical College, Middlesbrough, Yorks.
 Department of Political Economy, The University, Whiteknights Park, Reading.
 Drexel Institute of Technology, Philadelphia 4, Pennsylvania, U.S.A.
 Federal Reserve Bank of Boston, Boston 6, Massachusetts, U.S.A.
 Government of Pakistan, Planning Commission, Karachi, Pakistan.
 Institute of Administrative Affairs, c/o American Embassy, Tehran, Iran.
 International Cultural Research Centre, Seoul, Korea.
 International Executive, Riverdale Station, New York 71.
 Ministero delle Partecipazione Statali, Rome.
 Nanyang University, Jurong Road, Singapore 22.
 National Union of Railwaymen, London, N.W.1.
 Office des Marchés Agricoles, Montreal, P.Q., Canada.
 Rural and Industrial Development Authority, Kuala Lumpur, Malaya.
 Seccao Estudos Economiã Applicada, Associacao Industrial Portuguesa, Lisbon, Portugal.
 Vidyalankara University of Ceylon, Kelaniya, Ceylon.
 West of Scotland Agricultural College, Glasgow, C.2.

RECENT PERIODICALS AND NEW BOOKS

Journal of the Royal Statistical Society

VOL. 122, PART 1, 1959. *The Long-term Trend in the Size Distribution of Income*: H. F. LYDALL. *Report of the Committee on the Supply of and Demand for Statisticians*, introduced by E. S. PEARSON. *Size of Company and Other Factors in Dividend Policy*: P. SARGANT FLORENCE.

Applied Statistics

VOL. VIII, No. 1, MARCH 1959. *Some Aspects of Quality Control in Cotton Textiles*: G. NEWBERRY. *The Use of the Same Experimental Material for more than One Set of Treatments*: G. B. FREEMAN. *Elimination of Bias in the Preliminary Examination of Data*: D. H. WARD. *The Pattern of Consumer Purchases*: A. S. C. EHRENBURG. *Choice of Conversion Factor in the Derivation of an Index of a Discontinued Series*: K. S. BANERJEE. *A Significance Chart for Percentages*: S. ROSENBAUM.

Economica

VOL. XXVI, No. 102, MAY 1959. *The Politics of the Bullion Report*: F. W. FETTER. *Rational Choice Functions and Orderings*: K. J. ARROW. *On the Sales Maximization Hypothesis*: M. H. PESTON. *Factor Endowments and Factor Prices*: A. H. LAND. *Mathematical Appendix*: H. W. KUHN. *Inflation and Long-run Growth*: J. BLACK. *Productivity-gearred Wage Policies: A Comment*: W. PETERS; *A Further Note*: K. LANCASTER. *A Further Note on Revealed Preference*: P. NEWMAN. *The Rehabilitation of Classical Debt Theory*: A. T. PEACOCK.

The Review of Economic Studies

VOL. XXVI, No. 70, FEBRUARY 1959. *A Statistical Summary of the Extent of Import Control in the United Kingdom since the War*: M. F. W. HEMMING, C. M. MILES and G. F. RAY. *The Balance of Payments in a Monetary Economy*: F. H. HAHN. *Whose Cost of Living?*: S. J. PRAIS. *Regional Disparities in the Incidence of Taxation in Uganda*: W. ELKAN. *A Problem Encountered in the Comparison of Technical Coefficients*: Z. S. WURTELE. *A Graphical Exposition of Bohm-Bawerk's Interest Theory*: R. DORFMAN.

The Manchester School of Economic and Social Studies

VOL. XXVII, No. 2, MAY 1959. *World Exports of Manufactures, 1956 vs. 1937*: S. SPIEGELGLAS. *The Periodicity and Stability of Inventory Cycles in the U.S.A. (I)*: D. J. COPPOCK. *Employment Fluctuations, Labour Supply and Bargaining Turnover*: H. A. TURNER. *Life Assurance Funds and the Capital Market*: G. L. REES and W. HERRIGAN. *A Monthly Index of Wage-rates by Industries*: J. R. CROSSLEY.

Scottish Journal of Political Economy

VOL. VI, No. 2, JUNE 1959. *Economic Growth in Scotland, 1720-70*: H. HAMILTON. *Reflections on the Growth of Capital and Income*: A. K. CAIRNCROSS. *The Restriction of Road Haulage*: P. HART. *Urban Renewal in Glasgow*: S. C. ORR. *Recent Economic Trends: Demand for Ships, April 1959*: J. R. PARKINSON; *A Note on the Composition of the Working Population*: D. J. ROBERTSON.

National Institute Economic Review

No. 3, MAY 1959. *The Economic Situation: Expansion in Britain: Signs of a World Recovery. Price Stability and the Policy of Deflation: Post-mortem on the Effect of Deflation; The Prospect for Prices.*

The Economic History Review

VOL. XI, No. 3, APRIL 1959. *Adam Smith and the Industrial Revolution*: R. KOEBNER. *Heriots and Prices on Winchester Manors*: M. M. POSTAN and J. TITOW; *Statistical Notes on Winchester Heriots*: J. LONGDEN. *The Early Growth of Steam Power*: A. E. MUSSON and E. ROBINSON. *The Financing of Japanese Economic Development*: G. RANIS.

British Tax Review

APRIL 1959. *If I May be Pardoned for Adding . . .*: W. PHILLIPS. *More Section 8 Cases on Rating Relief*: K. F. GOODFELLOW. *Share Premium Account and Pre-acquisition Profits*: J. G. MONROE. *Settlements on Wives and Widows*: J. E. VINELOTT. *European Fiscal Systems: Belgium*: E. B. NORTCLIFFE.

The British Journal of Sociology

VOL. X, No. 2, JUNE 1959. *Toynbee and Sociology*: R. BIERSTEDT. *Darwinism and the Concept of Social Evolution*: D. G. MACRAE. *The Plural Society in Sociological Theory*: J. REX. *Some Social Consequences of Grammar School Education in a Rural Area in Wales*: J. L. WILLIAMS. *The Relation Between Educational Achievement and School Size*: R. LYNN. *Financial Incentives as the Expression of a System of Beliefs*: H. BEHREND. *Some Differences Between Middle-class and Working-class Grammar School Boys in their Attitudes towards Education*: E. BENE. *Social Arithmetic*: K. KELSALL.

International Affairs

VOL. 35, No. 2, APRIL 1959. *"The Invisible College"*: B. F. J. SCHONLAND. *Technical Aid: The Role of the West*: M. IONIDES. *China and the World*: R. HARRIS. *The B.B.C.'s External Services*: B. CLARK. *Education and Economic Development in the Under-developed Countries*: F. BENHAM. *Source Materials made Available to Historical Research as a Result of World War II*: M. LAMBERT.

The Banker

VOL. 109, No. 400, MAY 1959. *Financing the Expansion. The Economy Surveyed. Labour's Economic Policy*: A. CROSLAND. *Switzerland's Surfeited Money Market*: G. GYGAX. *Building Societies in a New Age*: G. LEE. *Wales in the Recession*: E. V. MORGAN. VOL. 109, No. 401, JUNE 1959. *How Soft is the Dollar? Everyman an Investor. The Economics of West Berlin*: S. SCHATTMANN. *Germany's Interest Rate Puzzle*: R. E. LÜKE. *London's Money Brokers*: R. F. G. ALFORD.

The Bankers' Magazine

VOL. CLXXXVII, No. 1382, MAY 1959. *An Obituary of the Credit Squeeze*: H. SPEIGHT. *Budgetary Expansion. The British Balance of Payments, 1958-59. Survey of Economic Conditions in the U.S.A.: A Welcome Spring Revival. Centralized Accounting in the Bank of Scotland*: R. HINDLE. *The Yorkshire Penny Bank, 1859-1959*: W. HARTLEY. VOL. CLXXXVII, No. 1383, JUNE 1959. *U.K. Finance Companies: The Uses and Sources of Funds, 1949 to 1958*: N. RUNCIE. *Some Characteristics of Banking in Switzerland*: G. GYGAX. *Survey of Economic Conditions in the U.S.A.: A Resumption of the Gold Drain. A New Approach to the European Problem*: G. CARLI. *Bank Advances for Welsh Drovers in the Early Nineteenth Century*: R. O. ROBERTS. *A Problem of the Paying Banker*: M. J. L. RAJANA-YAGAM.

Barclays Bank Review

VOL. XXXIV, No. 2, MAY 1959. *Britain Turns to Expansion. A Generation Ago. The Changing Pattern of Capital Movements.*

District Bank Review

No. 130, JUNE 1959. *Policy for Expansion and the Monetary Weapon*: SIR R. HARROD. *Management in Europe*: T. E. CHESTER. *This Octopus, Taxation.*

Midland Bank Review

MAY 1959. *The Nuisance of Stamp Duties. Monetary Survey, 1958-9. Business Conditions in Scotland.*

National Provincial Bank Review

No. 46, MAY 1959. *Building Societies*: A. STEWART. *Research on Building*: F. M. LEA.

The Three Banks Review

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- 2, 1959. *Law on Accelerating the Rate of Growth of the Economy, Improving the Material and Cultural Level of the People and Reorganising the Administrative and Economic Management of the State. Law on the 1959 Plan. The Total Mobilisation of Forces for the Accelerated Development of the National Economy. Ten Years of the Council for Mutual Economic Aid*: R. KHRISTOV. *The Economic Leap Forward and the Improvement of the Workers' Welfare*: I. ILIEV. *On the Question of Net Output as a Basic Indicator of the Plan*: G. ZHELEV. *Moral Obsolescence and the Need to Modernise Existing Fixed Assets*: I. RUSIMOV. *Reserves and Measures for Realising the Five-year Plan for Textiles Ahead of Schedule*: P. VYTKOVA. *The Rational Use and Reclaiming of Lubricating Oils—a Source of Great Economies*: M. GERASIMOV and D. RUSCHEV. *Some Questions of the Cost and Price of Food in Restaurants*: KH. KHADZHINIKOLOV.

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- No. 2, 1959. The Creative Might of Marxist-Leninist Theory. Ten Years Since the March 1949 Resolution (on Farm Collectivisation) of the Rumanian Workers' Party: S. NICA and O. PARPALĂ. Problems of Expanded Socialist Reproduction in Collective Farms: D. DUMITRIU and V. TOPOR. For a Fuller Use of Reserves for Lowering Industrial Costs. The Priority Growth of the Output of Means of Production: A. NEGUCIOIU. The National Income and Its Distribution in the U.S.S.R.: D. ALAKHVERDYAN. The Unscientific Character of Revisionist Theories Concerning the Evolution of Capitalism into Socialism: I. ADAM. The Soviet Conference on the Economic Efficiency of Investments—II. On Some Lectures of Political Economy on the Period of Transition from Capitalism to Socialism: V. SPIRIDON. Aspects of Materials Saving in the Semăntătoarea Works: N. RISTEA. Aspects of Inter-Imperialist Contradictions in the Struggle for Oil: I. STOIAN.
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NEW BOOKS

British

Agricultural Policies in Western Europe. PEP Occasional Paper No. 3. London: Political and Economic Planning (16 Queen Anne's Gate, S.W.1), 1959. 8½". Pp. 31. s. 6d.

[This contains a brief survey of the ways in which national agricultural policies in Western Europe have affected international trade. It is suggested that the next step in the attempt to co-ordinate policies must be a close examination of the extent to which countries have acted upon the recommendations already made by the Ministerial Committee for Agriculture and Food of the E.E.C.]

ALLHUSEN (D.) and HOLLOWAY (E.). *Money: The Decisive Factor. Britain's handicap in the economic race with Russia*. London: Christopher Johnson (11/14 tanhope Mews West, S.W.7), 1959. 9". Pp. 109. 10s. 6d.

[This is a statement of the case for a form of commodity money, which it is held would cure all problems of inflation, deflation and the like. Professional economists are roughly treated for their failure to grasp the simple truths involved. There is a valid element in the authors' contention that it is a peculiar arrangement that the greater part of the working money balances held by the public should serve as the basis of loans through the banking system; but the large claims made for their own proposals overlook, amongst other things, that the velocity of circulation is liable to vary for reasons that are not monetary in their origins.]

BALDWIN (K. D. S.). *The Niger Agricultural Project*. Oxford: Basil Blackwell, 1957. 9". Pp. xvi + 221. 27s. 6d.

[Mr. Baldwin has written a most interesting and well-documented appraisal of the Colonial Development Corporation's attempt to imitate in Nigeria the Sudanese Gezira Scheme. The Niger Agricultural Project, which lasted from 1949 to 1954 and showed a loss of £½ million overall, aimed to produce at a profit oilseeds for export and cereals for West African consumption by settling peasant farmers in a sparsely populated area at Mokwa, near the road/rail bridge over the River

Niger. Mr. Baldwin, who was invited by the Corporation to set out the lessons of this failure "for future guidance and warning," brings out clearly the contrast between the inconsistent planning of the public bodies concerned and the native shrewdness of the Nupe farmer in refusing to be drawn into a comparatively unremunerative occupation. The student of tropical agriculture and economic development will find a great deal of helpful material here in this case study on the problems of land tenure and share-cropping, on the nature of the labour market, on the need to make haste slowly with mechanisation, and for more adequate research into the technical and economic conditions peculiar to West Africa. The only criticism that one might offer is that the author could with advantage have said rather more about the reasons for successful land settlement in other places than the Sudan, for instance, Israel, or South-Eastern Russia.]

BARNARD (C. S.) and SMITH (V. E.). *Resource Allocation on an East Anglian Dairy Farm*. An application of linear programming. Occasional Paper No. 6 of the Farm Economics Branch of the Cambridge University School of Agriculture. Cambridge: Farm Economics Branch, School of Agriculture, 1959. 9½". Pp. 22. 2s. 6d.

[This is believed to be the first attempt in this country to use linear programming to devise a cropping and livestock programme for a farm. A small arable-dairy farm of a common type was chosen for study. Available activities, prices and resource limitations were specified, and an "optimum" programme calculated. The result showed a production pattern similar in outline to that actually in use; but the differences were none the less such as to make the optimum programme yield a net income 25% higher than the actual programme, on the basis of the assumptions made. This improvement was solely the result of reallocating resources among existing opportunities, and did not depend on any increase in the resources available or any increase in yields. The authors recognise that the application of linear-programming techniques to the problems of individual farms is not feasible on a large scale, but they suggest that its application to modal farm types might provide information which would materially assist individual farmers in planning their work.]

BRIGGS (A.). Robert Owen in Retrospect. POLLARD (S.). Dr. William King: A Co-operative Pioneer. Co-operative College Papers, No. 6. Loughborough: Education Department, Co-operative Union, 1959. 9". Pp. 33. 1s.

[These two papers were delivered as addresses at recent gatherings of those active in the Co-operative movement. They provide reappraisals of the work and ideas of one famous and one less well-known pioneer of co-operation. Professor Briggs emphasises Owen's merits as a social thinker in insisting on the need to view society as a whole. Dr. William King was a physician who was active in encouraging co-operative trading in Brighton in the 1820s. He ultimately parted company with the Owenites because he did not share their militancy or their hostility to religion.]

BRITAIN (SIR H.). *The British Budgetary System*. London: Allen & Unwin, 1959. 9". Pp. 320. 25s.

[To be reviewed.]

BROWN (HILDA G.). *Some Effects of Shift Work on Social and Domestic Life*. Yorkshire Bulletin of Economic and Social Research, Occasional Paper No. 2. Hull: Yorkshire Bulletin of Economic and Social Research, 1959. (Copies obtainable from The Secretary, Department of Economics and Commerce, University of Hull.) 9½". Pp. 54. 10s.

[This report is based on interviews with 156 shift workers and members of their families in five factories in the Midlands and the North. Some interesting conclusions emerge. Systems involving night work—either alternate day- and night-shifts or three-shift working—were found to be quite clearly disliked on balance, both by the shift-workers themselves and by their wives. But the same did not apply to double-day shifts. A majority of the workers interviewed actually preferred double-day shifts to normal day working. This preference was markedly stronger among women (the great majority of whom were married) than among men. Double-day shifts were least popular with young single men. They were also less popular in firms where they were a recent innovation than in firms where they were long-established and part of an accepted way of life in the community. In this connection the author raises the interesting question whether the social problems associated with shift-working may not in the long run solve themselves by a process of self-selection through which shift working becomes confined to people whose family circumstances and temperament cause it to be acceptable. She points out, however, that this would be at best a very slow process and would operate only in regions with a well-diversified industrial structure. In so far as this process of selection has already operated, it would impart a bias to the sample of workers interviewed, leading to a more favourable picture of the attitude towards shift-working than would apply among workers as a whole. This would be relevant if really large-scale extension of shift-working were contemplated in industries where it is not at present customary.]

CAMPS (MIRIAM). *The Free Trade Area Negotiations*. PEP Occasional Paper No. 2. London: Political and Economic Planning (16 Queen Anne's Gate, S.W.1), 1959. 8½". Pp. 51. 5s. This is available in the United States as Policy Memorandum 18 of the Center for International Studies at Princeton University, price 50c.

[This memorandum describes the course of the Free Trade Area negotiations and analyses the reasons for their breakdown. Mrs. Camps believes that there is a reasonable prospect of a better relationship between the Six and the United Kingdom being established in the near future, but on the more general issue of regional groupings she suggests that the conflict of interests between members and non-members is bound to be such as to invite reprisals in the long run unless the general level of world tariffs is low.]

CRACKNELL (B. E.). *Canvey Island: The History of a Marshland Community*. Leicester University Department of English Local History, Occasional Paper No. 12. Leicester: Leicester University Press, 1959. 9½". Pp. 47. 12s.

[This essay gives an outline history of the island from the earliest times up to the present day. This history falls into three main phases. Before the seventeenth century the island was a sheep-run, subject to periodic inundation, and with little or no permanent human habitation. In the 1620s sea-walls were built by Dutch engineers and workmen on behalf of a Dutch merchant resident in London, he being granted in return one-third of the acreage owned by the chief existing proprietors. This permitted arable farming, and led to the establishment of a separate Dutch community on the island. Within a hundred years the Dutch had either gone away or disappeared as a separate group through intermarriage, but arable farming continued to be the mainstay of the island's economy till the agricultural depression of the 1880s. The consequent extreme depression of land values encouraged purchase by speculators with a view to urban development of the island as a seaside resort. The result of this development may be seen in the increase in the island's population from about 300 during most of the nineteenth century to 3,532 in 1931 and 11,258 in 1957. Dr. Cracknell includes in his interesting essay a number of maps to illustrate specific points, but it would have been of assistance to the reader had he also provided a general-purpose map from which the place-names could be identified.]

DU CANN (E.). *Investing Simplified*. A guide to unit trusts, investment clubs and other share-purchase schemes. London: Newman Neame, 1959. 7½". Pp. x + 154. 15s.

[This is an elementary practical guide to Unit Trusts. The author is Managing Director of Unicorn Trust.]

EASTERFIELD (T. E.). *Productivity Measurement in Great Britain*. A survey of recent work. London: Department of Scientific & Industrial Research, 1959. 8". Pp. 79.

[This is a most useful survey. It consists of a critical account of recent and current British studies of productivity measurement at the level of the individual plant, arranged by subject in such a way as to constitute a continuous and readable text. Dr. Easterfield describes the main results of the research concerned and discusses the relationship between the findings of the different enquiries, the possible objections they may be open to, and the lines along which further research is needed.]

EINZIG (P.). *The Control of the Purse*. Progress and decline of Parliament's financial control. London: Secker & Warburg, 1959. 9". Pp. 344. 35s.

[To be reviewed.]

GUILBAUD (G. T.). *What is Cybernetics?* Translated by VALERIE MACKAY. London: Heinemann, 1959. 7½". Pp. viii + 126. 10s. 6d.

[The author provides an explanation in simple terms of the main concepts involved in the two aspects of cybernetics: control systems and information theory. It is written in a light, not to say whimsical, style, very unlike most Anglo-Saxon writings on the subject, and the author is concerned to deflate some of the more exaggerated claims made for the subject by its popularisers.]

HARRIS (R.) and SELDON (A.). *Advertising in a Free Society*. London: Institute of Economic Affairs (7 Hobart Place, S.W.1), 1959. 10". Pp. xiii + 216. 18s.

[Books on the economics of advertising tend to be a ragbag of arguments, good, bad and indifferent, brought forward to support a prejudged position for or against; they are rather reminiscent of some of the old literature on free trade and protection. The present book is a superior piece of work in its field, but it is still of this general type. It soon becomes clear that the authors are out to defend advertising, and in the analytical section of their book there are few pages where they do not try to press their conclusion home. The merits of their work are that it brings together a great deal of valuable information on advertising, including some interesting case-studies, and

that it shows a wide acquaintance with the literature of the subject, which the authors discuss energetically and with a fair degree of sophistication. The weakness is the lack of a properly thought-out and consistent theoretical framework within which the issues concerned may be judged. The argument is always flying off at a tangent, and the assumptions are varied from one point to the next without warning. For example, the authors several times emphasise that what sells a product in the long-run is its real merits, and that advertising only serves to induce the consumer to become acquainted with these, so that there is no danger of an inferior product becoming a market leader through superior advertising; yet elsewhere they support the view that advertising as such contributes to the "brand-image" of the product and hence to the utility the consumer gets from it. The authors recognise that there are objectionable features in advertising, but they do not regard these as inherent, and they suggest that they might be checked by the establishment of an Advertising Council and the adoption of stricter codes of conduct within the industry.]

HAWKINS (E. K.). *Road Transport in Nigeria*. A study of African enterprise. London: Oxford University Press, 1958. 8½". Pp. viii + 100. 15s.

[The significance of the road-transport industry in Nigeria, as the author sees it in this interesting study, is that it is a form of economic activity, mainly in the hands of Africans, which requires the ownership and operation of capital equipment. As such it may serve as a stepping stone to African entrepreneurship in manufacturing industry. The author describes the pattern of traffic, the size of firms in the industry, the recruitment and duties of drivers (owners do not generally drive their own lorries), costs, rates and charges, and the activities of the unions into which both owners and drivers are organised.]

HELLER (F. A.) (Ed.). *New Developments in Training*. Five studies in the efficient communication of skills. London: Polytechnic Management Association (Department of Management Studies, The Polytechnic, Regent Street, W.1), 1959. 8½". Pp. 80. 5s.

[This contains five papers: "A Biologist's Contribution to Learning," by J. Z. Young; "New Methods of Training in Manual Skills," by D. W. Seymour; "Recent Survey Findings in the United Kingdom and other European Countries," by Winifred Raphael; "Training for Executive Skills," by F. A. Heller; and "The Integration of Training, Organisation and Policy," by D. King.]

HEYWORTH (LORD). *An International Business*. London: Unilever, 1959. 8". Pp. 16.

[The international aspect of Unilever was the subject discussed in the Chairman's speech at this year's annual general meeting, here reprinted. Topics treated include the international distribution of the capital of the business, the functions of the Boards of the parent companies, and recruitment and allocation of managerial personnel.]

LAMARTINE YATES (P.). *Forty Years of Foreign Trade*. A statistical handbook with special reference to primary products and under-developed countries. London: Allen & Unwin, 1959. 9½". Pp. 255. 40s.

[To be reviewed.]

LEWIS (J. PARRY). *An Introduction to Mathematics for Students of Economics*. London: Macmillan, 1959. 9". Pp. x + 394. 40s.

[To be reviewed.]

MATHIAS (P.). *The Brewing Industry in England, 1700-1830*. London: Cambridge University Press, 1959. 9". Pp. xxviii + 596. 85s.

[To be reviewed.]

MUNKMAN (C. A.). *American Aid to Greece*. A report on the first ten years. London: Methuen, 1959 (New York: Praeger, 1958). 8½". Pp. x + 306. 30s.

[The author is a British accountant who was formerly Chief of Audits and Surveys of the United States Economic Mission to Greece. His book is a detailed account of the working of the American aid programmes to Greece on the basis of his personal experience. It contains much interesting material and provides a vivid picture of the achievements and frustrations of those working for the Mission. Mr. Munkman's appraisal is not very favourable. While American aid has undoubtedly made a major contribution to the country's recovery, Mr. Munkman maintains that a large proportion of aid funds have been wasted. In the first place, the system of annual allotment of funds hampered efficient planning and led to many ill-considered schemes being rushed through so as to get into the year. In the second place, advance plans made for maintenance and administration projects after completion were quite inadequate. In the third place, the sharp and indiscriminate cut in aid in 1952, in addition to creating resentment, prevented or hampered the completion of many unfinished schemes. In a final chapter the author draws some general conclusions on the best ways of planning aid to underdeveloped countries.]

PARKER (H. R.). *Paying for Urban Development*. London: The Fabian Society (11 Dartmouth Street, S.W.1), 1959. 8½". Pp. 21. 2s.

[This pamphlet provides an unusually clear statement of the compensation-betterment problem. After describing the background set by the Barlow and Uthwatt reports, the author analyses the financial provisions of the 1947 and 1953-54 Town and Country Planning Acts and puts forward some proposals of his own. His chief criticism of the present situation is that the attempt to collect betterment for the state has been abandoned, while, on the other hand, compensation continues to be paid (in respect of 1947 development values) where planning permission for development has been refused. At the same time he recognises that the system of development charges embodied in the 1947 Act was unsatisfactory in that it destroyed the owner's inducement to make land available for development. The author's own proposal is to revert to the original idea of the Uthwatt Committee, and make compulsory acquisition by the local authority followed by a lease to the developer the normal means of development. The acquisition would be at existing use value, and the element of compulsion would mean that the loss of incentive would not matter. In certain cases, however, especially where the proposed development was by the existing owner, this method would be unsuitable, and a development charge of the 1947 type could be adopted, but the charge should be at 75% of the extra development value, not 100%, so as to retain some incentive. Mr. Parker's argument is open to criticism at a number of points, mostly in his too ready acceptance of the doctrine of "floating value," but his discussion of the subject is interesting and well-informed.]

The Poor Man's Guide to the Stock Exchange. London: Labour Research Department (161 Drury Lane, W.C.2), 1959. 7½". Pp. 80. 6s.

[The bulk of this booklet consists of a description of the mechanics of the working of the Stock Exchange and a discussion of the distribution of share ownership. A chapter is also included on speculation. A final chapter states some conclusions. These are not argued at a high level, and it is not made clear whether the ultimate disappearance of the Stock Exchange, which is looked forward to, is or is not to be accompanied by the disappearance of the private ownership of industry. The main specific recommendations are the imposition of a capital-gains tax and the publication of fuller information in company accounts.]

ROBERTSON (SIR DENNIS). *Lectures on Economic Principles, Volume III*. London: Staples, 1959. 8". Pp. 164. 16s.

[To be reviewed.]

SALVADORI (M.). *The Economics of Freedom. American capitalism today*. London: The Pall Mall Press (Rood End, Dunmow, Essex), 1959. 8½". Pp. xxii + 242. 30s.

[This is a discussion in extremely broad terms of the social and political background of the working of the American economy. The author's liberal standpoint is unexceptionable, and he has some good points to make, but the unceasing parade of exalted sentiments is a little wearying.]

SHACKLE (G. L. S.). *Economics for Pleasure*. London: Cambridge University Press, 1959. 9". Pp. ix + 269. 21s.

[To be reviewed.]

STONE (R.) and CROFT-MURRAY (GIOVANNA). *Social Accounting and Economic Models*. London: Bowes & Bowes, 1959. 7½". Pp. 88. 9s. 6d.

[To be reviewed.]

THISTLETHWAITE (F.). *The Anglo-American Connection in the Early Nineteenth Century*. Philadelphia: University of Pennsylvania Press (London: Oxford University Press), 1959. 8½". Pp. x + 222. 36s.

[To be reviewed.]

WARD (J. T.). *Farm Rents and Tenure*. London: The Estates Gazette (28 Denmark Street, W.C.2), 1959. 9". Pp. xii + 95. 15s.

[This is an interesting and sensible essay. The author describes the present state of farm rents and land ownership in this country and considers the historical background and the case for reform. He regards the position as it has existed up till the Agriculture Act of 1958 as unsatisfactory in two respects: low rents and security of tenure have weakened the spur to farming efficiency, and the finance of agricultural investment has been found to need state assistance of various forms, in addition to that provided by price subsidies. Higher rents are in his view essential, and the Agriculture Act of 1958 should make them possible if the landlord is willing to press his claims—there are grounds for believing that in recent years he has often not pushed them even as far as the existing arbitration machinery would have permitted. In stating his case Mr. Ward brings together in useful form data on movements in rents since 1939, on the yield on investment in tenanted farms

and on the differential between prices of land with and without vacant possession. He also considers various proposals that have been made for reforming or altering the British system of land tenure, including nationalisation of the land. The latter he finds quite attractive in theory but more doubtful as a practical proposition.]

Canadian

BARBER (C. L.). *Inventories and the Business Cycle, with Special Reference to Canada*. Canadian Studies in Economics, No. 10. Toronto: University of Toronto Press, 1958 (London: Oxford University Press, 1959). 10". Pp. xii + 132. 28s.

[The first part of this book, which is a revision of a doctoral dissertation submitted at the University of Minnesota, is a review of the theory of inventory fluctuations, along lines made familiar by the work of Metzler, Abramovitz, Nurkse and others. The second part is a study of data on inventory behaviour in Canada, first for ten manufacturing industries considered separately, then for the economy as a whole. Some support is found for the doctrine that inventory investment plays a causal role at cyclical turning-points, but over the cycle as a whole the relationship between inventory investment and general business activity is less regular than might have been expected. This is largely but not exclusively due to variations in agricultural output. The author puts forward reasons for believing that previous writers have tended to put too much stress on the volume of inventories and not enough on value in their studies of the role of inventories in business fluctuations.]

Indian

AGARWALA (A. N.). *Some Aspects of Economic Advancement of Under-developed Economies*. Allahabad: Kitab Mahal, 1958. 8". Pp. v + 124. Rs. 3.0.

[This slight book contains articles on cottage industries, deficit financing, a comparison of economic progress in India and China and the social insurance system in China. It is too superficial to be of much value.]

BALAKRISHNA (R.). *Measurement of Productivity in Indian Industry*. Madras: University of Madras, 1958. 10". Pp. xii + 466. Rs. 20.

[This is the second edition of the book noted in this JOURNAL, December 1954. The methods and topics covered are the same as in the first edition, but the data have been brought more up-to-date. It is a well-written, well-produced work with a wealth of useful statistics on the major Indian industries.]

BANSIL (P. C.). *India's Food Resources and Population*. Bombay: Vora, 1958. 9". Pp. 252. Rs. 10.

[In Part I there is a historical survey of India's food-grain position from 1920 and a very brief analysis of Government food policy in recent years. Part II describes, *inter alia*, some of the requirements for increased agricultural output and lays particular emphasis on fertilisers. In Part III the recent past and future of population and food consumption are considered. The author is hopeful that it will be possible "to hold the Malthusian devil in chains," but the reasons given for his optimism are not altogether convincing.]

DHAR (P. N.). *Small-Scale Industries in Delhi*. Bombay: Asia Publishing House, 1958. 10". Pp. 277. Rs. 10.

[Discussions on the role of small industries in India's past and future are usually bedevilled by emotional statements and lack of statistical information. This useful quantitative survey avoids the former and goes some way to remedy the latter. Thirteen industries, ranging from flour milling and leather to light engineering and electroplating, were studied in 1954-55. Figures, some of doubtful accuracy, are presented on capital-output ratios, employment, productivity, sources of finance, costs and wages. The great diversity in size, use of power and efficiency is well demonstrated.]

GUPTA (R.) and MATHUR (R. N.). *Development and Working of the Indian Constitution*. Allahabad: Kitab Mahal, 1958. 9". Pp. 504. Rs. 7.50.

[This is one of many books on the Indian Constitution. It is aimed at undergraduates in political science. Part I gives a descriptive outline of constitutional developments from the beginning of British rule to 1947. Part II gives full details of the present constitution and its operation. There are chapters on the Presidency, the States, political parties, the courts and elections. The pre-1947 story is marred by undisguised bias and too much repetition. An index and fuller documentation are needed.]

GUPTA (S. C.). *Problems and Processes of Economic Planning in Underdeveloped Economies* (with special reference to India). Allahabad: Kitab Mahal, 1959. 8". Pp. xii + 228. Rs. 5.00.

[The author discusses the meaning of planning, the unsuitability of the capitalistic system for underdeveloped countries and the "need for a special theory of growth." Although he is a convinced "planner," he is severely critical of India's first and second five-year plans. The book is badly organised, the author frequently indulges in inaccurate generalisations and there are numerous mis-spellings.]

KHARE (G. P.). *Planning in India*. Allahabad: Kitab Mahal, 1958. 8". Pp. vii + 148. Rs. 3.75.

[It is apparently becoming obligatory for all Indian economists to write a book on planning. This is another in the usual mould which provides a short description and analysis of the two plans. The author uses linear programming techniques to test the success of the plans' allocation of resources to different sectors.]

LALWANI (K. C.). *The Indian Capital Market*. Calcutta: Artha Vanijya Gabesana Mandir, 1959. 7½". Pp. 159. Rs. 5.00.

[This is a short, badly-produced survey of the capital market in India, presumably designed for elementary students.]

MAMORIA (C. B.). *Tribal Demography in India*. Allahabad: Kitab Mahal, 1958. 7½". Pp. xiv + 151. Rs. 5.00.

[The population of the so-called "scheduled tribes" in India numbers about 19 millions. This slight monograph gives a superficial survey of their language, culture, employment, health and general social structure. The Government is taking some steps to improve the conditions of the aboriginals, but the long-run solution of the tribal problem remains uncertain.]

MISRA (B. R.). *Economic Aspects of the Indian Constitution*. Calcutta: Orient Longmans, 1958. 8½". Pp. x + 140. Rs. 7.00.

[This is the second edition of a work first published in 1952. An outline is given of the Constitution and the Directive Principles of State Policy. The bulk of the book is concerned with the financial aspects of the Constitution. There are chapters, for example, on the distribution of revenues between the Federation and the States, on financial administration and on the Finance Commissions. This is generally a well-written and useful survey of the subject.]

MISRA (B. R.). *Report on Socio-Economic Survey of Jamshedpur City*. Patna: Patna University Press, 1959. 10". Pp. v + 134. Rs. 10.0.

[The main purpose of this statistical survey was to study the problems of rapid urbanisation. Jamshedpur is the centre of the Tata Iron and Steel Company, and what was a village fifty years ago is now a city of 300,000 people. A vast amount of data is presented here on population, literacy, employment, migration and housing. Part of the study deals with conditions in shops and other small unorganised establishments.]

MUKHERJEE (P. K.). *Economic Surveys in Underdeveloped Countries*. A study in methodology. Bombay: Asia Publishing House, 1959. 8¼". Pp. xii + 263. 34s.

[This successful Oxford D.Phil. dissertation by the head of the Agricultural Economics Section at Delhi University attempts to provide "a new approach to research in agricultural economics." The author is rightly dissatisfied with existing statistical surveys in India and believes that these fail to give an accurate picture of economic conditions in village communities. Here he presents his suggested improvements. There are three parts. The first is descriptive and includes a brief assessment of major village surveys in India, although some of the more recent ones are neglected. Part II is entitled "Economic Concepts and their Measurability." He argues that "concepts of income, prices, wages, cost, employment, marginal holdings, etc. have to be redefined in the context of a self-subsisting barter and under-employed economy." For example, the meaning of "farm cost" in the Indian context and the problem of measuring it are discussed. The practical aspects of statistical measurement, especially problems in the design and execution of sample surveys, form the subject of Part III. The author suggests the use of a Leontief-type model to demonstrate the incoming/outgoing transactions of the village and a Keynesian-type multiplier to estimate non-agricultural incomes.]

NATIONAL COUNCIL OF APPLIED ECONOMIC RESEARCH. *Criteria for Fixation of Water Rates and Selection of Irrigation Projects*. Bombay: Asia Publishing House, 1959. 9½". Pp. xi + 156. 28s.

[The methods hitherto used in India are described and alternatives proposed. The old financial productivity criterion for planning irrigation works, as used under British rule, is rejected as altogether too restrictive in its effects through failure to allow for external economies. The alternative proposed amounts to the classical welfare criterion of equating social cost and social benefit at the margin. This unexceptionable principle is, of course, open to the objection that it is too vague for practical application, but examples are given of how it might work out in concrete terms. Traditional methods of fixing water rates are also judged unsatisfactory and insufficiently uniform. It is proposed that the rates charged should instead be such as to extract a percentage of the net benefit accruing from irrigation, the percentage varying according to regional conditions. An example is given of how this might be applied in the case of a particular project.]

RAMANADHAM (V. V.). *Indian Railway Finance*. Delhi: Atam Ram, 1956. 8½". Pp. vii + 184. Rs. 7.8.

[Indian railways are still the largest industry in the country and are unusual in that they consistently make profits. This competent study has three main parts. Part I analyses secular and cyclical changes in railway prosperity since 1900. In Part II there is a discussion of railway costs, and in Part III the relation between rates policy and profits is investigated.]

Report of the Third Indian Co-operative Congress. New Delhi: All India Co-operative Union, 1958. 10". Pp. vii + 138.

[The main interest of this report lies in the speech of the Prime Minister and in the criticism by almost every delegate of the increasing officialdom which has accompanied State participation in the movement.]

Pakistan

Report on the Survey of Rural Credit and Rural Unemployment in East Pakistan, 1956. Dacca: Dacca University Socio-Economic Research Board, 1958. 10". Pp. x + 205. Rs. 7.8.

[This report is the outcome of interviewing over 3000 households in four administrative sub-districts. The accuracy of the results is admittedly dubious, but data have been assembled on size of holdings and fragmentation, standards of living and unemployment. The main part of the report is concerned with the extent of indebtedness, sources of credit, causes of debt and methods and rates of interest payment. About three-quarters of families were in debt, more than half the loans being supplied by "friends and relatives" and very few by government agencies or professional money-lenders.]

Ghanaian

GARLICK (P. C.). *African Traders in Kumasi*. African Business Series No. 1. Accra: Economic Research Division, University College of Ghana, 1959. 10". Pp. 116. 5s.

[This study is based on interviews with African retail and wholesale traders. The object of the research is to discover the problems facing those traders. The research is continuing, and the present report is more concerned with presenting material than drawing conclusions. Information given covers the personal background of the traders, the structure of business, practices in buying and selling, and social aspects. The latter includes an interesting discussion of the possible obstacles to the growth of a business which are created by the matrilineal inheritance principle.]

American

AITKEN (H. G. J.) (Ed.). *The State and Economic Growth*. Papers of a conference held in October 1956 under the Auspices of the Committee on Economic Growth. New York: Social Science Research Council, 1959. Pp. x + 389. \$3.75.

[This contains essays by different authors on the role played by the state in economic development in a number of countries at certain particular stages of their growth. Among the cases studied are those of the United States, 1820-90, Australia, 1860-1900, Russia, 1890-1939, Manchuria, 1860-1940, Turkey, 1933-50, and Canada, Germany, Switzerland and Eastern Europe more generally. Three more general essays are contributed by Richard Hartshorne, Bert F. Hoselitz and Joseph J. Spengler.]

All-Bank Statistics, United States 1896-1955. Washington, D.C.: Board of Governors of the Federal Reserve System, 1959. 10". Pp. 1229.

[This volume is the result of a large-scale programme of work undertaken by the Federal Reserve System involving a comprehensive revision of statistics for all banks by class of bank and by state for the period 1896-1955. It contains balance-sheet data for banks in the United States as a whole and in each separate state. These data are broken down into major subdivisions on a comparable basis.]

ANDERSON (T. J.), GITLOW (A. L.) and DIAMOND (D. E.) (Eds.). General Economics. A book of readings. Homewood, Illinois: Irwin, 1959. 9". Pp. xiv + 487. \$3.50.

[This is another collection of readings designed to supplement, for the benefit of American undergraduate students, what they read in their main text-books. It contains sixty-four items, mainly drawn from the writings of recent American academic economists but with some representation of older classical writers as well.]

BAUMOL (W. J.). Economic Dynamics. An introduction. Second edition. New York and London: The Macmillan Company, 1959. 8½". Pp. xv + 396. 47s.

[The first edition of this book was reviewed by Professor C. F. Carter in the ECONOMIC JOURNAL of September 1953. In addition to a brief survey of the history of macro-economic dynamic models the book provides one of the best available introductions to the mathematics of difference equations. In the present edition the main additions have been in the mathematical material, and new chapters are included on certain mathematical techniques which have gained in importance in the literature in recent years.]

BERLINER (J. S.). Soviet Economic Aid. The new aid and trade policy in underdeveloped countries. New York: Praeger for the Council on Foreign Relations, 1958 (London: Oxford University Press, 1959). 8½". Pp. xv + 232. 35s.

[This study describes the nature and extent of recent Soviet economic dealings with underdeveloped countries and how they fit into the working of the Soviet economy. The author's main interest is in the political implications.]

BRAZER (H. E.). City Expenditures in the United States. National Bureau of Economic Research, Occasional Paper 66. New York: National Bureau of Economic Research, 1959. 9". Pp. 82. \$1.50.

[This contains a multiple regression analysis of factors associated with variations in municipal expenditure per head, in total and by categories, in 462 cities in the United States in 1951. Among the chief factors identified are location, classification as central city of metropolitan area, residential suburb, independent city, etc., and median family income. Population size as such does not appear to be important, though per head expenditure is affected by certain factors associated with size, namely density and the ratio of the city's population to that of the metropolitan area in which it is situated.]

CAIRNCROSS (A.). The International Bank for Reconstruction and Development. Essays in International Finance No. 33. Princeton, New Jersey: International Finance Section, Princeton University, 1959. 9". Pp. 36.

[Professor Cairncross here provides an interesting critical survey, in some detail, of the work of the Bank in its various aspects. He discusses the ways in which it has raised funds and spent them, its relation to other sources of international capital, and its advisory functions. His general verdict is a very favourable one, and few international institutions have, in his view, been so successful. He concedes that there is an element of truth in some of the criticisms that have been made of its policies. In particular, the principle of confining loans to the direct foreign-exchange component of specific projects is difficult to defend theoretically and could lead to distortion. Professor Cairncross believes, however, that it has been operated in a sufficiently flexible way to avoid serious harm. The Bank's unwillingness to finance manufacturing industries in government ownership is also admitted to be open to criticism, but it is suggested that the device of giving loans to publicly owned Development Banks has provided a way of getting round the obstacles that might otherwise have prevented sufficient assistance being given to the manufacturing sector. On the more general criticism that the Bank's loans have been quantitatively inadequate in relation to the needs of underdeveloped countries, Professor Cairncross's conclusion is coloured by his well-known views on the role of capital in economic development, which he considers to be overrated by many writers.]

CARTTER (A. M.). Theory of Wages and Employment. Homewood, Illinois: Irwin, 1959. 9½". Pp. xii + 192. \$5.00.

[To be reviewed.]

CHERINGTON (P. W.). Airline Price Policy. A study of domestic airline passenger fares. Boston, Mass.: Harvard University Graduate School of Business Administration, 1958 (London: Bailey & Swinfen, 1959). 8½". Pp. xx + 471. 60s.

[This is one of a series of studies into air transport undertaken at the Harvard Business School in recent years. It analyses the price policy pursued by domestic airlines and the effects of the control exercised by the Civil Aeronautics Board. It is based on a series of case-studies. In broad terms the author thinks that price policies have been reasonably successful, but he finds reason to believe that pricing is an aspect of management that airlines have given less attention to than might be desirable.]

DEWEY (D.). Monopoly in Economics and Law. Chicago, Illinois: Rand McNally, 1959. 9½". Pp. 328. \$5.75.

[This is a general treatise. It surveys in outline the theory of monopoly and describes briefly the results of empirical research on monopoly and competition. It then goes on to give rather fuller treatment of the law relating to monopoly in the United States. A chapter is also included on monopoly policy in Britain since 1890. The author describes his position as that of "a bourgeois liberal who esteems Frank Knight as the best of economists and Learned Hand as the best of lawyers".]

DIVINE (T. F.). Interest. An historical and analytical study in economics and modern ethics. Milwaukee, Wisconsin: Marquette University Press, 1959. 9". Pp. xvi + 254. \$7.00.

[This is based on a doctoral thesis written by the author at the University of London in 1938. Part I is a historical survey of debates on the morality of interest. Part II describes the classical, Keynesian and Robertsonian theories of the determination of the rate of interest; and Part III puts forward the author's own conclusions on the morality of interest as a problem of justice. Fr. Divine was first President of the Catholic Economic Association and is Editor of its journal *The Review of Social Economy*.]

DUE (J. F.). Government Finance. An economic analysis. Revised edition. Homewood, Illinois: Irwin, 1959. 9½". Pp. xxi + 627. \$6.75.

[The first edition of this was reviewed by Dr. A. R. Prest in the ECONOMIC JOURNAL of March 1955. The present edition takes account of contributions to the literature that have appeared since that time.]

DURKHEIM (E.). Socialism and Saint-Simon. Edited by A. W. GOULDNER and translated by CHARLOTTE SATTLER. Yellow Springs, Ohio: The Antioch Press, 1958. 9½". Pp. xxx + 240. \$5.00.

[This is the first English translation of an incomplete work of Durkheim's. It is a sociological analysis of the development of socialism in the eighteenth century and of the doctrines of Sismondi and Saint-Simon. It is based on lectures delivered by Durkheim in the 1890s. Durkheim originally intended to complete it with a treatment of Marx and German socialists, but was prevented by other work from returning to it.]

EPSTEIN (R. C.). Making Money in Today's Market. Economica Books. New York: Doubleday, 1959. 8½". Pp. vii + 215. \$1.75.

[This is a revised paper-backed edition, under a different title, of a book that first appeared in 1955 as *How to Invest Your Money*. It is a sensible elementary guide to the investor who seeks a good return on his money without trying to make a fortune. The main emphasis is placed on the advisability of diversifying the portfolio and also of diversifying the time of purchase.]

The European Common Market and Its Meaning to the United States. A statement on national policy by the Research and Policy Committee of the Committee for Economic Development. New York: Committee for Economic Development, 1959. 11". Pp. 152. \$2.00.

[An outline of the plans of the European Economic Community and their likely effects is followed by recommendations for action by the United States. These recommendations are of a generally liberal character and stress the redirection of business and trade.]

FABRICANT (S.). Basic Facts on Productivity Change. National Bureau of Economic Research Occasional Paper 63. New York: National Bureau of Economic Research, 1959. 9". Pp. viii + 49. \$1.00.

[This is a most useful brief compendium of findings on movements of productivity over time, as revealed by research conducted by the National Bureau. Some of the findings described are familiar, some are new. The average rate of increase of physical output per man hour in the

private sector of the U.S. economy since 1889 has been 2.3%. Estimates are also given of the rates of growth of output per unit of physical capital: these naturally turn out much lower. Productivity has increased distinctly faster after the First World War than it did before. Within the cycle, the main check to productivity increase comes in the first half of the downswing. Rises in productivity during the period studied have occurred in almost all industries, and they have tended to be most rapid in industries that have increased their total output most rapidly. As between industries, there is little systematic relation between rate of growth of hourly earnings and rate of growth of productivity. The rate of growth of productivity since 1945 has been high by historical standards, but not unprecedentedly so. What has been exceptional has been the rate of capital accumulation, which has been faster than in any preceding period of similar length.]

The Federal Funds Market. A study by a Federal Reserve System Committee. Washington, D.C.: Board of Governors of the Federal Reserve System, 1959. 9". Pp. 111. \$1.00.

[The Federal funds market refers to the borrowing or lending of deposit balances in the Federal Reserve Banks. Transactions of this description are carried out between commercial banks as a means of short-term adjustment of their cash and reserve positions. This study describes the growth and structure of the market in Federal funds and analyses the forces governing the rate of interest charged.]

FEIN (R.). Economics of Mental Illness. Joint Commission on Mental Illness and Health, Monograph No. 2. New York: Basic Books, 1958. 8½". Pp. xx + 164. \$3.0.

[This contains statistical estimates of the cost of mental illness in the United States, both in the form of direct costs (treatment, etc.) and in the form of indirect costs (foregone earnings of the patients). Estimates are given for different classes of illnesses, for patients of different ages and so on, as well as for aggregates. The author discusses the significance of his estimates and their limitations, and suggests ways in which research might be extended so as to assist in the formulation of public policy.]

GALENSON (W.) (Ed.). Labor and Economic Development. New York: John Wiley (London: Chapman & Hall), 1959. 9½". Pp. xii + 304. 54s.

[This contains essays on the labour movement in five countries, viewed against the general economic background of the countries concerned and the conditions of supply and demand in the labour market. The countries covered and the contributors are India, by C. A. Myers; Japan, by R. A. Scalapino; Egypt, by F. H. Harbison; French West Africa, by E. Berg; and the British West Indies, by W. H. Knowles.]

GARVY (G.). Debits and Clearings Statistics and Their Use. Revised Edition. Washington, D.C.: Board of Governors of the Federal Reserve System, 1959. 9". Pp. 144.

[This is a revised edition of a monograph which first appeared in 1952. The author discusses the technical issues involved in the interpretation of the statistics and their usefulness for purposes of economic analysis. The prompt availability of debits and clearings statistics gives them an advantage over any other comprehensive measure of economic activity, and, when taken in conjunction with data on deposits, they provide a measure of velocity of circulation. The revised edition takes account of changes which the Federal Reserve System introduced in the statistics in 1953. The discussion of the significance of velocity measures in monetary theory has not been brought up to date, and a separate study of this is in preparation.]

GOLDBERG (S.). Introduction to Difference Equations, with illustrative examples from Economics, Psychology, and Sociology. New York: John Wiley (London: Chapman and Hall), 1958. 9". Pp. xii + 260. 54s.

[This book presents a comprehensive and clear account of the elementary theory of difference equations. Although explicitly designed for readers with a comparatively narrow mathematical background, it will inevitably make heavy demands on them. Considerable effort has been made to see that the illustrative examples are up-to-date and of intrinsic interest in their own field. For example, there are discussions of the simple stochastic processes involved in learning models and in models of social mobility. Professor Goldberg has made a very valuable contribution to the series of mathematical texts for workers in the Social Sciences.]

HARRIS (C. LOWELL). The American Economy. Principles, practices and policies. Third Edition. Homewood, Illinois: Irwin, 1959. 9½". Pp. xv + 883. \$7.00.

[This massive volume is a general text-book covering theory as well as a descriptive account of the American economy. Previous editions appeared in 1953 and 1956. The present edition has been largely re-written. Its appearance is enlivened by the employment of colour in diagrams, but the use to which this is put does not appear to be very imaginative.]

HILL (S. E.) and HARBISON (F.). Manpower and Innovation in American Industry. Princeton, New Jersey: Industrial Relations Section, Princeton University, 1959. 8½". Pp. vi + 85. \$2.00.

[This is a case-study relating to the extent and causes of the increase in managerial, professional and other overhead personnel in fifty leading companies. The increase was found to be general, but greatest in the professional class. Experience in the firms studied suggests that the main reason for increases in overhead personnel is innovation in products, processes and administrative arrangements. Broadly speaking, the increase in overhead personnel was greatest in the firms whose innovation was fastest. Bureaucratic expansion and empire-building are not considered by the authors to be more than a minor cause, although the policy of decentralisation pursued by a number of the companies undoubtedly served to increase overhead personnel. A concluding chapter considers some implications regarding future man-power needs.]

HOLMES (A. R.). The New York Foreign Exchange Market. New York: Federal Reserve Bank of New York, 1959. 9". Pp. 54. 50c.

[This is one of a series of publications of the Federal Reserve Bank of New York designed to furnish information not easily available elsewhere on monetary institutions and techniques. It is mainly a description of the mechanics of dealing, but it includes also some illustrative examples of market behaviour drawn from recent experience. One of these relates to the margin between spot and forward rates for sterling, and the reasons why it has not behaved in the way that it would have done if arbitrage funds had been fully responsive to interest-rate differentials.]

KRIZ (M. A.). World Monetary Affairs Today. Essays in International Finance, No. 34. Princeton, New Jersey: International Finance Section, Princeton University, 1959. 9". Pp. 32.

[The first two parts of this essay discuss the main forms that international gold transactions have taken since the War and the place held by gold in comparison with other forms of international reserves. The author then goes on to consider some domestic implications. He believes that the elements of the old gold standard that have persisted into our present international monetary arrangements are salutary and provide an institutional framework within which monetary discipline is encouraged, without undue rigidity. Although Mr. Kriz is careful to introduce the appropriate qualifications where necessary, the reader who was guided by his general tone and approach might be inclined to forget that balance-of-payments disequilibrium is not solely a function of the level of domestic demand and that the level of domestic demand is not solely a function of the quantity of money.]

LEKACHMAN (R.). A History of Economic Ideas. New York: Harper & Brothers, 1959. 8½". Pp. xiii + 427.

[This is a lively outline text-book, describing the ideas of leading economic theorists from Plato to the present day. While the more difficult branches of theory are inevitably treated sketchily, a clear and vivid picture is given of the distinctive contributions of many thinkers and also of their personalities.]

LETWIN (W.). Sir Josiah Child, Merchant Economist. Kress Library of Business and Economics, Publication No. 14. Boston, Mass.: Baker Library, Harvard Graduate School of Business Administration, 1959. 10½". Pp. vi + 76. \$2.00.

[This is a debunking essay on Child's claims to be considered a precursor of analytical economics. Professor Letwin maintains that the objective use of logical reasoning is wholly absent from Child's work, and that in so far as his views appear to anticipate those of Adam Smith, it was because he found it expedient to support free trade in certain particular contexts, not from any general intellectual conviction. Appended to Professor Letwin's essay is a photographic reprint of Child's *Brief Observations Concerning Trade, and Interest of Money*.]

LI (CHOH-MING). Economic Development of Communist China. An appraisal of the first five years of industrialisation. Berkeley and Los Angeles, California: University of California Press (London: Cambridge University Press), 1959. 9½". Pp. xvi + 284. 56s. 6d.

[To be reviewed.]

LICHTENBERG (R. M.). The Role of Middleman Transactions in World Trade. National Bureau of Economic Research, Occasional Paper 64. New York: National Bureau of Economic Research, 1959. 9". Pp. xvi + 86. \$1.50.

[Middleman transactions are defined, broadly speaking, to mean transactions in which residents of the country of consumption make payment not to the country of production but rather to a third country. Estimates are prepared of the magnitude and nature of such trade, in total and for different

countries, largely on the basis of the trade returns of seven countries which record imports both by country of production and by country of purchase. Middleman trade is reckoned to have comprised about 13% of world trade in 1952. The relative extent and structure of three partially overlapping types of middleman trade are also studied—overseas territorial trade, trade between hard and soft currency countries, and entrepot trade. One of the author's chief concerns is the implications of middleman trade for the interpretation of trade statistics, and a long chapter is devoted to this.]

LINK (R. G.). *English Theories of Economic Fluctuations, 1815–1848*. New York: Columbia University Press (London: Oxford University Press), 1959. 9½". Pp. 226. 40s.

[This consists of essays on the theories of economic fluctuations embodied in the works of six British economists of the early nineteenth century, Attwood, Malthus, Joplin, James Wilson, Tooke and J. S. Mill. The essays are drawn together in brief introductory and concluding chapters. The views of each author are clearly and painstakingly set out, with plentiful quotations, and critically discussed in *a priori* terms in the light of modern theoretical concepts. Most of the authors concerned did not address themselves explicitly to the task of explaining fluctuations as such, far less to producing models of recurrent cycles. This naturally makes their work unsatisfactory by present-day standards, but is perhaps scarcely a fair ground of criticism. Some of the criticisms of Tooke, in particular, which Dr. Link makes seem rather wide of the mark for this reason. Dr. Link does not make clear that the whole object of Tooke's *History of Prices* was to combat the quantity theory of money and that his emphasis on harvest and factors affecting individual commodity markets was not a tiresome failure to see the wood for the trees but a quite deliberate attempt to emphasise the role of real forces in price determination and to play down the role of the money supply.]

MALLORY (W. H.) (Ed.). *Political Handbook of the World, 1959*. New York: Harper & Brothers for the Council on Foreign Relations (London: Oxford University Press), 1959. 10". Pp. 231. 35s.

[This handbook provides particulars in summary form of government, political parties, parliament and the Press in all countries for which data are available.]

MINTZ (I.). *Trade Balances during Business Cycles: U.S. and Britain since 1880*. National Bureau of Economic Research, Occasional Paper 67. New York: National Bureau of Economic Research, 1959. 9". Pp. x + 99. \$1.50.

[To be reviewed.]

NATIONAL BUREAU OF ECONOMIC RESEARCH. *The Study of Economic Growth*, by S. FABRICANT. 39th Annual Report. New York: National Bureau of Economic Research (261 Madison Avenue, New York 16), 1959. 10½". Pp. 90.

[The introductory essay by Solomon Fabricant surveys the various aspects of the research currently being done at the National Bureau on growth. Apart from a project on the U.S.S.R., this research is mainly concerned with growth in the United States. Aspects studied include long swings in growth generally, long swings in growth of population and labour force, trends in wages and productivity, and investment in education. Full particulars of these and other current research projects are given in the main body of the report.]

Readings in Industrial Organization and Public Policy, selected by a committee of the American Economic Association. Homewood, Illinois: Irwin for the American Economic Association, 1958. 9½". Pp. xi + 426. \$6.00.

[This is the eighth volume in the American Economic Association's series of readings. As the editors explain in their introduction, the subject is more diffuse than those covered in most of the previous volumes, and a certain lack of unity is unavoidable. Aspects included are industrial structure, pricing policy and behaviour, monopoly, competition and public policy regarding them. All the articles without a single exception are by American authors.]

ROBERTS (B. C.). *Unions in America. A British view*. Princeton, New Jersey: Industrial Relations Section, Princeton University, 1959. 8½". Pp. x + 136. \$2.00.

[This booklet was written by Mr. Roberts during a recent spell as a Visiting Professor in Princeton. It is an interpretative essay, not a descriptive survey, but British readers will find it a more interesting and intelligible account of current problems in the American labour scene than most accounts written by American authors, since it discusses explicitly many features of the scene which American authors take for granted but which do not by any means apply in other countries. The topics treated include both internal matters of union organisation and the role of unions in wage bargaining and industrial relations.]

ROBINSON (M. A.). *The National Debt Ceiling. An experiment in fiscal policy*. Washington, D.C.: The Brookings Institution (London: Faber), 1959. 9". Pp. 104. 11s.

[The statutory ceiling on the debt of the Federal Government was established in 1917, and although the ceiling has been successively raised, the principle of having a ceiling has not been abandoned despite the far-reaching changes in ideas on fiscal policy that have taken place since then. In this survey of the case for and against having such a ceiling the author finds that the disadvantages of the system as it has worked are overwhelming. Since the ceiling has been periodically raised it has not been effective in preventing the long-run increase in the debt, but it acts as a tiresome short-run restraint hampering efficient and consistent public-expenditure policy. Mr. Robinson suggests that the proper way to prevent the undue growth of the debt lies in more general measures of fiscal discipline.]

SPENCER (M. H.) and SIEGELMAN (L.). *Managerial Economics. Decision making and forward planning*. Homewood, Illinois: Irwin, 1959. 9½". Pp. xi + 454. \$6.75.

[This is a text-book of a relatively unusual kind. Its purpose is to explain the bearing of economics on practical problems of management, and the aspects covered include measurement of profit, demand analysis, costs and production management, pricing policy, investment planning and finance. The range of economic literature drawn on is extensive, and includes pure theory, applied studies and econometrics. Stress is placed throughout on "formulating problems in a conceptually quantitative manner capable of numerical solution." With such a wide range of subjects to be covered, the treatment given to many is inevitably no more than introductory, but the amount of material included is substantial, and not all of it is elementary. Good bibliographies are included.]

STUDENSKI (P.). *The Income of Nations. Theory, measurement and analysis, past and present. A study in applied economics and statistics*. New York: New York University Press, 1958. 10½". Pp. xxii + 554. \$25.00.

[To be reviewed.]

United States Monetary Policy. Addresses and papers of a regional assembly co-sponsored by Duke University and The American Assembly of Columbia University, and held in March 1959. Durham, North Carolina: Duke University Press, 1959. 9". Pp. vi + 60.

[This contains addresses on different aspects of monetary policy by Malcolm Bryan, President of the Federal Reserve Bank of Atlanta, Warren L. Smith, Professor of Economics at Michigan, and Henry C. Wallich, Professor of Economics at Yale. It also contains the final report of the Assembly. The participants were drawn from business, government, the Press and the universities.]

VILLARD (H. H.). *Economic Development*. New York: Rinehart, 1959. 9". Pp. xiv + 229. \$2.50.

[This is an elementary text-book designed for students with no previous knowledge of economics. About half of it is concerned with developed areas, the remaining half being divided with chapters on measurement, development under Communism and underdeveloped areas. In the brief space available to him the author chooses to give fuller treatment to the things he thinks most important rather than attempt a bird's-eye view of the field. These things are research and technical progress in developed countries and population in underdeveloped ones.]

WEINTRAUB (S.). *A General Theory of the Price Level, Output, Income Distribution and Economic Growth*. Philadelphia, Pennsylvania: Chilton Company, 1959. 8½" Pp. xi + 123. \$3.50.

[To be reviewed.]

WHELPTON (P. K.) and KISER (C. V.) (Eds.). *Social and Psychological Factors Affecting Fertility*. New York: Milbank Memorial Fund, 1958. 9". Pp. 6 × 1087–1372. \$1.00.

[The Indianapolis survey of social and psychological factors affecting fertility was initiated in 1938 and has provided the basis of a large number of studies. The present volume is the concluding instalment of these. It deals with such issues as the influence of siblings and friends on fertility, children's desires for siblings, the number of children desired at the time of marriage and so on. A concluding essay by the editors summarises the chief findings of the study.]

WILLIAMS (E. W.). *Freight Transportation in the Soviet Union: A Comparison with the United States*. National Bureau of Economic Research, Occasional Paper 65. New York: National Bureau of Economic Research, 1959. 9". Pp. ix + 38. 75c.

[This is based on a larger forthcoming study of Soviet transport. Comparative data for freight traffic in ton-miles are given for the U.S.A. and the U.S.S.R. Soviet railroad freight traffic exceeded that in America for the first time in 1957. However, whereas in the U.S.S.R. rail traffic comprised 90% of total freight traffic, the corresponding figure for inter-city freight traffic in the U.S.A. was only 41%. A striking feature of the Soviet railroad system is that it carries a volume of traffic roughly equal to that in America with only about half the line mileage and equipment. This is attributed by the author largely to the existence of a seller's market for rail transport, in which goods are carried at the carrier's convenience.]

Dutch

CHOW (G. C.). *Demand for Automobiles in the United States. A study in consumer durables*. Contributions to Economic Analysis, No. XIII. Amsterdam: North-Holland Publishing Company, 1957. 8½". Pp. 110.

[This is an econometric study in which the results of applying existing theories of demand to the case of automobiles are compared with the results of applying an alternative theory, of which the distinctive feature is the hypothesis that the individual aims to maintain a constant ratio between his stock of assets in the form of money and his stock of assets in the form of consumer durables. Most of the analysis is concerned with the demand for the total stock of automobiles. A separate chapter introduces the further consideration needed to yield a model of automobile purchases.]

Official

BRITISH

MEDLICOTT (W. N.). *The Economic Blockade, Volume II. History of the Second World War, United Kingdom Civil Series*, Edited by Sir Keith Hancock. London: H.M. Stationery Office and Longmans, 1959. 9½". Pp. xiv + 727. 50s.

[To be reviewed.]

NEW ZEALAND

FIRTH (R.). *Economics of the New Zealand Maori*. Wellington: The Government Printer, 1959. 10". Pp. 519. 50s.

[This is the second edition of Professor Firth's *Primitive Economics of the New Zealand Maori*, which originally appeared in 1929. The new edition is for the most part unchanged. Chapter I, however, which is a general discussion of the problems of primitive economics has been re-written in the light of the substantial literature which has been written on the subject since 1929. The chapter on economic aspects of cultural change has been added to so as to take account of the development of Maori economic institutions themselves over the last twenty-five years.]

CEYLONESE

NEWMAN (P.). *Studies in the Import Structure of Ceylon*. Colombo: Planning Secretariat, 1958. 9½". Pp. 108.

[This contains three parts. Part I presents data on Ceylon's imports since 1925, classified into consumption goods, intermediate goods and investment goods, together with separate data on re-exports and government imports so as to permit calculation of private retained imports within each of these classes. These data are designed partly as a preparation for the econometric work of Part III and partly as raw material that may be useful to other researchers. Part II derives estimates of the import function for consumption goods from cross-section budget study data. This is of some interest in itself, but a good deal of doubt may be felt about its usefulness as a guide to the import function for consumption goods of the country as a whole, which after all is what is mainly relevant for policy. A different type of approach to this problem is made in Part III, where some rough-and-ready econometric calculations of the determinants of imports are made on the basis of time-series data for the years 1948-56. The main explanatory variables used are export receipts, subdivided and lagged in various ways, and the level of Ceylon's external assets.]

IRISH

Economic Development. Dublin: Stationery Office (copies obtainable from Government Publications Sale Office, G.P.O. Arcade, Dublin), 1958. 9½". Pp. 253. 7s. 6d.

[This is a general survey of ways in which Ireland's economic development might be forwarded. It was prepared by the Secretary of the Department of Finance. The main part of the report is taken up by an industry-to-industry survey. An important feature of the recommendations made is that non-productive public investment should be reduced and public expenditure generally cut where possible so as to facilitate the expansion of productive investment in the private sector.]

INTERNATIONAL

BANK FOR INTERNATIONAL SETTLEMENTS. *Twenty-Ninth Annual Report*. Basle: Bank for International Settlements, 1959. 12". Pp. 244.

[This report contains, as usual, a survey of the economic developments of the year, with particular reference to events in the sphere of money and credit. A chapter is included on the liquidation of the European Payments Union and the transition to the European Monetary Agreement.]

EUROPEAN COAL AND STEEL COMMUNITY. *Seventh General Report on the Activities of the Community*. Luxemburg: High Authority, European Coal and Steel Community, 1959. 9". Pp. 410.

[This year's report contains, as usual, an informative account of the activities of the High Authority during the year and the situation of the coal and steel industries in the Community.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. *Third Report on the Agricultural Policies in Europe and North America*. Paris: O.E.E.C., 1958. 9½". Pp. 469. 24s.

[Previous reports in this series described agricultural policies in general in Member and associated countries and the particular problem of price- and income-support policies. The present report deals in greater detail with particular problems affecting individual countries. Structural problems are discussed in relation to Austria, Ireland, Luxemburg, Portugal and Yugoslavia; problems in the cereals sector in relation to Greece, Italy, Spain and Turkey; problems in the dairy sector in relation to Canada, Denmark, Norway, Switzerland and the United Kingdom; problems in the meat sector with reference to Belgium, Germany and Iceland, and price-support systems with reference to France, the Netherlands, Sweden and the United States.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. *Policies for Sound Economic Growth. 10th annual economic review*. Paris: O.E.E.C., 1959. 9½". Pp. 133. 9s.

[This contains a lucid brief survey of inflationary and deflationary tendencies in member countries since 1953, with special reference to the events of 1957 and 1958. Separate chapters are devoted to payments problems and policies and to trade and payments with North America and with third countries. The conclusions drawn as to the appropriate policies for the future are sensible but for the most part along familiar lines. Emphasis is placed on the need for concerted action between member countries to avoid balance-of-payments difficulties in the process of recovery from recession.]

UNITED NATIONS. *Agricultural Economics Research in Asia and the Far East*. Study prepared by the ECAFE/FAO Agriculture Division, Economic Commission for Asia and the Far East. Bangkok: United Nations, 1958. 9". Pp. viii + 100. 5s.

[Part I, written as a continuous text, is a description of the research work done in agricultural economics in universities and other institutions in the region. Part II is a list of research projects completed in 1956-57 or in progress.]

UNITED NATIONS. *Economic Survey of Asia and the Far East 1958*. Bangkok: United Nations, 1959. 11". Pp. viii + 225. 18s.

[In addition to a survey of developments during 1958, this report contains a general review of post-war industrialisation in the region. This brings together much national statistical material in conveniently comparable form.]

UNITED NATIONS. *Economic Survey of Europe in 1958*. Prepared by the Research and Planning Division, Economic Commission for Europe. Geneva: United Nations, 1959. 11". Pp. 216. 18s.

[As usual, this report contains much valuable and important material and many interesting ideas on a number of separate topics. The first two chapters deal with recent developments in Eastern and Western Europe respectively. The third chapter is related to this and discusses the

relation between external balance and economic expansion. The authors recognise that concern for external balance has led the governments of a number of West European countries to restrict activity below the level that would as such have been desirable, but they believe that the recent contractionary tendencies have not been due solely to government policies but have had deeper causes as well, in particular the slowing down in the rate of growth of the available labour supply and the exhaustion of the special post-war backlog of investment. Comparison of the experience of different West European countries yields some surprising conclusions: on the whole high rates of growth of output are associated with favourable foreign balances, and favourable foreign balances are associated with less-than-average rates of price increase. Possible explanations of this are considered. The two concluding chapters of the Survey deal with consumption patterns in Eastern and in Western Europe. Here again some of the conclusions are contrary to what might be expected. The broad consumption pattern in the advanced countries of Western Europe has been relatively stable over a long period and is expected to remain so. Expenditure on food has been prevented from falling as a fraction of the total by increased expenditure on processing, packaging, etc. The share of expenditures devoted to durables has increased considerably in the last few years, but comparison with American experience gives reason to doubt whether this increase will continue.]

UNITED NATIONS. *Economic Survey of Latin America 1957*. Prepared by the secretariat of the Economic Commission for Latin America. New York: United Nations, 1959. 10½". Pp. xx + 292. 22s. 6d.

[Part I surveys trends since the War in Latin America's external relations: its exports and capital receipts, and its capacity to import. In every year since the War up till 1956, Latin America's capacity to import grew, largely because of the rise in export prices. In 1957 the rise continued for the region as a whole, but it was entirely attributable to the increase in the earnings of Venezuela, and the position of the other countries became less favourable. Part II describes developments in 1957 in more detail, for the region as a whole and for Argentina, Brazil, Colombia, Cuba, Chile and Mexico. Part II consists of a single chapter on the balance of payments. Finally, there is a special study on trade in agricultural commodities in Latin America.]

UNITED NATIONS. *Food and Agricultural Price Policies in Asia and the Far East*. Study prepared by the ECAFE/FAO Agriculture Division, Economic Commission for Asia and the Far East. Bangkok: United Nations, 1958. 11". Pp. x + 110. 9s.

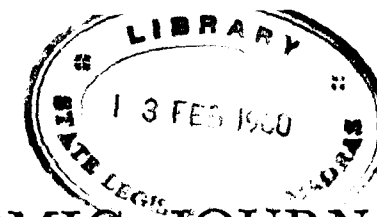
[Most of this report is taken up by country-by-country surveys. The policies pursued are described, and their effects are analysed in the light of data available on prices, consumption, production and imports. The main aim of policy in most cases has been the stabilisation of food prices for the benefit of non-farm consumers.]

UNITED NATIONS. *The Future Growth of World Population*. Population Studies No. 28. New York: United Nations, 1958. 11". Pp. 75. 5s. 6d.

[This contains new projections of world population up to 1980. These projections are materially higher than those that had been previously put forward. An important part of the explanation for this is the change in the picture of Chinese population prospects that recently available data have brought about; but revision of mortality and fertility forecasts more generally also work in the same direction. The calculation has been done by assimilating regional populations to one of a limited number of model types, and it is believed that the result is appreciably more reliable than previous projections have been.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. *Trade Yearbook, 1958*. Rome: F.A.O., 1959. 10½". Pp. xvii + 321. 17s. 6d.

[This is the new name given to the publication which formerly appeared under the title *Yearbook of Food and Agricultural Statistics, Part 2*. It provides statistical data on trade in primary products, given in value terms for the main categories of primary product, and in physical units for individual products.]



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REFLECTIONS ON THE PRACTICAL APPLICATION OF ECONOMICS¹

1. It is the privilege of the President of the Society to treat himself as an elder statesman and to make his address on some very general subject. I am the more led to follow this practice because of the nature of my own work in recent years. I must confess to feeling from time to time, as I read the pages of our own JOURNAL, that there is a considerable gulf between what I do and at any rate the more mathematical articles which seem to fill an increasing amount of our space. Doubtless there is a personal element in this. But I have been led to wonder whether there may not also be some features in the present conduct of our discipline which require re-emphasis. I am going to speak, then, on those very broad topics which involve the nature and usefulness of economics, about which we all think from time to time. And if there is any interest in a collection of things which have been said already, it must come from my own arrangement of pieces, most of which are already familiar.

2. The word "practical," of course, implies doing something, and those who make use of economics for purposes other than the pleasure of intellectual exercise obviously want to discuss things that are occurring or are likely to occur. This requires one to be able to take a view about what is going to happen, and if one does not like the prospect, to be able also to take a view as to whether anything can be done about it or whether one must put up with the inevitable. It may also be the case that where there is the possibility of action, one is not very clear as to what one ought to want and therefore to do. These are perfectly general formulations, applicable as well to other fields as to economics.

3. I shall confine myself mainly to economics in relation to public policy, and within that to the more general issues; and to speak also of Western economies and mainly of the United Kingdom. As Mr. Seers² has reminded

¹ Presidential address delivered to the annual meeting of the Royal Economic Society on July 9, 1959. I am much indebted to Mr. Bryan Hopkin and to Mr. J. Downie of the Economic Section of the Treasury for comments and suggestions. I was also helped by Professor Tress' lecture on "The Contribution of Economic Theory to Prognostication" (published in *Economica*) and by several papers by Professor Ely Devons (one of which was published in *Lloyds Bank Review*, July 1959).

² Dudley Seers, "An Approach to the Short-period Analysis of Primary Producing Economies," *Oxford Economic Papers*, February 1959.

us in a recent number of *Oxford Economic Papers*, a good deal of misunderstanding can arise in applying the lessons of one economy to another. My concentration on public policy is partly an occupational bias. But it is also because I suspect that one of the main uses of professional economists to business-men is in presenting them with the informed and coherent view of general economic developments which is also one of the chief preoccupations of the Government economist. I suspect that, in the determination of policy for a business, the irreducible area within which intuition is the best guide is rather wide. Even if computers enable us to act on what comes out of them, we will never get away from what we are to put in, and in business the intuitive input must be high.

4. I will first deal briefly with the third of the questions I posed a moment ago; the question of what you ought to want.

5. Many would say that the question of what we want or should want is not within the professional competence of economists. Within limits, I agree. In the practical world there is, strictly speaking, no such thing as an economic action any more than there is any other kind of action—there are only actions, with all the attendant circumstances. Thus if the economist is asked about what ought to be done, it must be either because he is being asked as a whole man rather than as an economist, or else the question implies agreement about the end, so that it is really about the means.

6. But it is an illusion to imagine that the dispassionate economist beloved of those who labour the distinction between ends and means, between normative and positivistic economics, can live in the practical world. It is an illusion which arises from forgetting to release from *ceteris paribus* at the end the multiplicity of interconnected objectives which exist in practice, but which are, quite rightly, assumed away at the beginning of the reasoning.

7. Any action has repercussions which are in principle unlimited. It is important in practice to hold to the strong shield of the doctrine of *de minimis*, lest we should end up by denying the possibility of knowledge, as some general equilibrium theorists have come very near to doing. Nevertheless, it is true that in practice what we want is always a *total* situation which we prefer to any other of the alternative total situations open.

8. And however much, in any particular instance of choice, our attention has to be concentrated on a particular objective, yet we shall be unhappy in the outcome if we neglect all the other objectives, and the fact that they are connected, in the sense that progress towards one will have implications for the others.

9. Put in this very general form, this is a commonplace. Everyone knows that you cannot have your cake and your penny too. But at the level of practical choice the inter-relations between choices are very complex. It is one of the merits of an economic training (indeed of scientific training generally, and social scientific training for our particular case) that it teaches us to consider always the inter-relationships of the particulars without

sing sight of the general. Thus, if I may be somewhat immodest on our behalf, the economist may sometimes know what his client "really" wants better than he does himself.

10. This doctrine that we can, so to speak, put ourselves into other people's minds is a dangerous one. It would be salutary for the practical economist to hang on his wall the forcible statement of Adam Smith¹ on this subject. Nevertheless, the idea that other people can express unaided what it is now fashionable to call their social-preference functions is also dangerous. The number of related objectives and the complexity of their inter-connections is often too great for the client to be able to formulate his objectives unaided. For example, it is only now ceasing to be a paradox to the layman that there are cases where you *can* have your cake and your penny too, *e.g.*, that you can increase consumption by increasing investment if you have unused resources. The idea that the best way to cure inflation may sometimes be to raise prices is still very shocking to the layman. And so on.

11. I suggest therefore that the economist can be of a great help as an advisor. There are, of course, limits to his approach. The economist, who is trained to measure quantities either in money or in terms of real costs compared by the possibility of substitution at the margin, is always in danger of minimising or neglecting the forces which cannot easily or accurately be expressed in these terms. The practical man, since he is dealing with reality from the beginning, is very unlikely to make this mistake. Take, for example, the dislike which is so prevalent among human beings of being subjected to the forces of economic change, especially when the short-term effects are financially adverse. This is at the root of the idea of what is "fair and reasonable" which is just as prevalent to-day as it has been at other periods of history. This dislike is very real to the politician, who has to evaluate it in the sense that he may have to take a decision as to whether to acquiesce in or promote or mitigate or prevent a change. Any help which he could get from the economist would clearly be extremely useful.

12. What this suggests to me is that the economist who is interested in these questions can make his characteristic or professional contribution, as I think many economists still tend to do, by condemning various practices out of hand, but by trying to identify all the forces at work and attempting to measure the cost of as many as possible in money terms. As I have said, this is unlikely in itself to yield a clear-cut answer. Thus if the objective is to keep a given number of people in a given area, to move them elsewhere is certainly not an exact alternative. But it would certainly be useful

¹ *Wealth of Nations*, Book IV, Chapter 2, "The statesman who should attempt to direct private industry in what manner they ought to employ their capitals . . . would . . . assume an authority which would nowhere be so dangerous as in the hands of a man who had folly and presumption enough to fancy himself fit to exercise it."

It is not so much that the statesman, or Civil Servant, would not be capable of making better decisions than many business-men, as that he has not time or information in the detail needed.

to know what this would cost, and also what it costs to keep them where they are. Another practical problem of this kind is presented on a large scale by the use of land, not only for agriculture but also in general. There is plenty of scope for economic analysis as well as for empirical research, since the problems of determining the costs involved—of getting the question into a shape where one can set about its measurement—are usually quite severe exercises in economic thinking. And anyone who is engaged in this kind of work can be fairly sure that he is doing something well worth while in the practical sense.

13. I come now to my central theme, namely to review the help which economics does and could give in predicting what is going to happen and saying how something different can be made to happen. I need scarce emphasise that the two problems are essentially the same. In one case the problem is to forecast the unrolling of events if certain strategic variables are left to work themselves out undisturbed. In the other case the task is to forecast how events will unroll if these strategic variables are changed, and how they may be changed.

14. Nor, in emphasising prediction, am I seeking to make a distinction between this and understanding. Indeed, if I understand them rightly, philosophers to-day maintain that all understanding and knowledge contain a predictive element. What we have to do, in order to understand and predict, is to impose some kind of structure on the undifferentiated flux of events. This involves two activities: the imposition of concepts by which we can classify the flow of events into a manageable number of classes, and the formulation of hypotheses about the empirical inter-relationships between these classes.

15. I must emphasise this distinction between concepts and hypotheses because, though quite elementary, it is also fundamental, and frequently neglected in practice. Hypotheses can be true or false, but concepts can only be useful or useless. The characteristics of a useful set of concepts are first, a minimum condition, that they should be internally consistent; second, that they should be capable of having quantitative statements made about them, cardinal or ordinal; thirdly, that it should be possible in practice to establish functional relationships between the concepts.

16. Let me take a very simple example; the simplest possible version of the Keynesian model in a closed system. The "value added" by the factor of production engaged in a business enterprise is defined as the excess receipts from current sales over current expenditure by the enterprise on goods and services purchased from other enterprises. The sum of the value added for all enterprises (broadly defined) is the national income Y . We then define C as current expenditure by non-business enterprises and I as $Y - C$. We so establish the fundamental definitional relationship $Y \equiv C + I$.

17. This expression is capable of mathematical manipulation in all sorts

of ways. In particular, without additional assumptions or definitions, we can write

$$\Delta Y \equiv \Delta I \frac{1}{1 - \frac{\Delta C}{\Delta Y}}$$

18. But, however much we manipulate the expression, it can tell us nothing about the nature or behaviour of reality, because the initial definitions contain no statements about the nature of reality.

19. But if we formulate the hypothesis $\frac{\Delta C}{\Delta Y}$ is constant we immediately have a most powerful instrument for predicting the behaviour of reality. If we go further, and postulate a second functional relationship between I and previous values of Y , we then have a member of the well-known, and now quite extensive, family of trend and cycle models.

20. I have quoted this example, not because I believe that these particular functional relationships are valid. Experience has shown that in fact, we do not know enough about the multiplier or the accelerator relationships to enable us to use them in any simple way in forecasting—even when the multiplier is formulated in complex matrix terms. I have used it, because the system of which it is the essence gave birth to one of the most fruitful developments in practical economics during the last quarter of a century—namely, national accounting.

21. I use this term in a wide sense, to mean not only the net accounts which appear in our Blue Book and in similar publications in other countries but also the various disaggregations of the net accounts, which belong to the same family—e.g., input/output tables or analyses of money flows.

22. I have already described the specifications which a set of concepts must satisfy if they are to be useful or fruitful; namely, consistency, measurability and that it should be possible to establish empirical relationships between them. How far does the conceptual structure of national accounting meet these requirements?

23. A great deal of work has gone into establishing the consistency of the definitions which are employed. Argument in detail still continues—I see that the proper treatment of import duties in the measurement of Gross Domestic Product is even now being hotly debated in the JOURNAL. But we now have a body of theoretical wisdom on the subject which rivals in thoroughness that of the medieval church. We have a structure of great flexibility, capable of disaggregation, in principle, right down to the individual household or firm.

24. As regards the measurability of the concepts, there is no longer much reason to complain. Sir Harry Campion, in his Presidential Address to the Royal Statistical Society this year, reviewed the present position on official statistics in this country. Thanks largely to his own efforts, and the

support of the statistical divisions in the Government Departments concerned, we can at least claim that we have a fairly full and accurate picture of where we were in the not too distant past, and work is going on all the time to make this more complete and nearer to the present. The user of statistics is, of course, never likely to be satisfied with what he has got; and there is a constant, though on the whole an amicable struggle, between the economist who wants everything now, the statistician who wants everything right and the raw material, the unfortunate supplier of the statistics who has to fill in the forms. We should all be grateful to the Federation of British Industries for the help which they give us: their members in turn, themselves engaged in forecasting, are, of course, helped by any improvements both in statistics and in their interpretation and application. We have at any rate something better than the "last year's *Bradshaw*" to which Mr. Macmillan referred when he was Chancellor.

25. But, as I have said, one set of concepts does not of itself say anything about what is going to happen. The only relations we can derive from a set of definitions are those logically implied by the definitions. Mathematical manipulation can suggest new and illuminating ways of bringing out these implications, but it cannot add any more content. We gain insight into reality and enable ourselves to pass from statistical material to something that will enable us to say with confidence what that material will be in the future, only by establishing empirically the relationship between our concepts.

26. It is when we come to consider the body of usable empirical functional relationships between the national-income categories that the gulf between theory and practice to which I referred at the beginning of my lecture yawns so wide. In his lecture on "The Contribution of Economic Theory to Economic Prognostication" Professor Tress has reviewed the field extensively and found it wanting. I have nothing to add in general except to say that my own experience in practical forecasting bears out his conclusion, that our models are very ill-clad with relationships.

27. As regards our own short-term forecasting, we do have some explicit empirical equations—tax functions naturally, price equations which do not simply contain a constant margin and so on. But, by and large, the models we use are extremely simple ones, and we find that for short-run forecasting it is much more reliable to collect forecasts of the components, where these are available, from people who will actually make the decisions or undergo the experiences, and to subject the other quantities to a simple process of extrapolation, modified as best one can by general knowledge, judgment and experience, than to use any more complex functional relationships. The process is well illustrated in the United Kingdom by the annual *Economic Surveys*, or the appraisals by the Chancellor of the Exchequer in annual Budget speeches. In these also a clear distinction can be seen between those quantities which can be reasonably got at in this way and those which

cannot, of which the most obvious example is stocks. "The last and most uncertain element in home demand is expenditure on industrial and commercial stocks." Our forecasts of export also, though based on detailed study, naturally involve some conjecture.

28. When it comes to long-term forecasts, which have become features of many countries, both those which are very rigidly controlled and those engaged on development, there are some theoretical reasons for expecting things to be better. The number of terms which can be handled in any equation is fairly strictly limited in practice. Hence, however cleverly our concepts are framed, we are bound to leave out some factors. But if we have chosen our concepts wisely, the excluded factors will be truly random, and so can safely be ignored in the long run, since their influence will be zero. But, in practice, I doubt whether economic science has yet contributed to long-term forecasting more than a few very general ideas, though these are important ones.

29. It may be thought that, in adopting this critical tone, I am finding fault with the nature of reality rather than with the practice of economics—a sin which it is peculiarly easy to commit.

30. So far as the long term is concerned, I think there is much truth in this. When we are dealing with the growth of output in physical terms, what is needed is essentially technical rather than economic information. An under-developed country needs to know how much steel a particular plant will turn out, how long it will take to train the labour and how efficiently it will operate; what sort of land it has, what sort of methods of cultivation could be applied to it, what are the social and technical problems involved in getting a change in methods; and so on. It is a very long and laborious process, first, to make a plan and, second, to put it into operation, and it is quite certain that at every stage things will be found to be going differently from what had been expected, and adjustments will have to be made. Thus the uncertainties inherent in any process of growth are likely to be so great that the price adjustments and the income adjustments which will have to be made as time goes on can be foreseen only as types of problem—or at any rate unless the original plans are constructed with so little relation to past experience as to make it probable that there will be a clear lack of balance between different components.

31. Indeed, I go so far as to wonder, where long-term projections are concerned, whether the main danger is not that under-developed countries will expect too much of economists, rather than too little; so that perhaps we ought actively to be engaged in reducing our image to more proper proportions. In particular, it seems to me that it is more necessary to be aware of the limitations than of the advantages of the capital/output ratio concept to which so much attention is now devoted. Economics was created in developed countries, and there is always a danger in applying its theories to under-developed countries without considering whether this is appro-

priate.¹ Even if we did understand the history of our own development, the initial circumstances of most of the now developed countries were very different from those now confronting the present under-developed ones.

32. As regards short-run forecasting, it may very properly be asked why, if one can ask people what they are going to do, one should hanker after functional relationships which would allow their action to be inferred from other given data. Moreover, I have already suggested that there are ineluctable uncertainties in the short run, for example, the weather, political uncertainties, labour disputes and so on. Indeed, the process of short-term adjustment of the economy, for these and other reasons, is often compared to other types of practical operation, driving a car or steering a ship. There is a general idea of the direction in which it is intended to move and of the conditions which will be encountered. But we do not know enough about the conditions either of the mechanism (the economic model) or the circumstances which will be encountered (the statistical background and the exogenous variables) to dispense with the driver or attendant, as may be done often with stationary machines for long periods. And if the driver has to be there he will do better to look out and adjust himself to the developing situation than to assume that his calculations will see him through.

33. Far be it from me to under-rate the virtues of the empirical approach to policy—the constant adjustment of the rudder. Still less do I want to decry the importance of general knowledge, judgment and experience—intuition. Indeed, if intuition were given the “scientific” name of “non-statistical inference” no one would look down his nose at it. Intuition may indeed be defined as the quality which, pragmatically speaking, sets men above machines.

34. There is much truth in all this. Indeed, since we are dealing with people, the basis of our discipline is obviously more akin to that of medicine than to the exact sciences. But we must not go too far in finding excuses for ourselves, or accept the present situation with complacency.

35. One might almost define scientific progress as the reduction of the area in which we have to rely on intuition or trial and error. The inevitable uncertainties with which we are faced in the short run are perhaps not so great as is often thought. While the weather in some countries produces uncertainties which upset all calculations, in Britain the availability of supplies from all over the world reduces the range of variation to quite small figures. Perhaps the most inconvenient effect is that of the vagaries of the potato crop on the retail price index, itself an important economic quantity. Because of the ratchet operation of wages, it could be argued that fluctuations in potato prices have an inflationary effect, and it would be an interesting study for Mr. Dow and Mr. Dicks-Mireaux to see if there is anything in it.

36. Other short-run uncertainties are mainly on the technical side, and

on exogenous shifts in demand, which again are much more difficult for less-diversified economies than our own; and the broad class of changes which could be labelled social and political. Wars and rumours of war, changes in political regimes and so on are things with which mankind has always had to deal so far, and with which it looks as if mankind will continue to have to struggle for some time to come. From the economist's point of view, everyone knows that they are uncertain, no one expects him to be able to predict them, and there is usually little of which he should feel ashamed if he gets them wrong. What at first sight seems to be a large political uncertainty—the incidence of labour disputes—is another field in which the economist as such is no better able to predict than anyone else. But in recent years they have in fact had very little effect on the general economic situation, and looking back at most economic series, it would be difficult to detect them apart from unevennesses in such figures as the monthly Trade Returns.

37. When it comes to asking, rather than predicting, what people are going to do, it is important to remember the limitations on the direct approach. In many cases, “don't know” would be the accurate answer to a question. But even when it is not, people often change their minds in the time elapsing between answer and action, because of changes in general economic circumstances. Some of them, and perhaps some of the “don't knows,” could be foreseen by those engaged in wider economic analysis. For example, economists in the United States began quite early to discount the rather gloomy forecasts of private investment intentions which businessmen were making in the later part of last year. As successive samples have been taken, showing a steady upward revision of intentions, the wisdom of this discounting has been justified. In the second place, great care needs to be taken in interpreting people's answers to questions, especially if they have an interest in the action to which their answers will lead. For example, I would not have great faith in the accuracy of answers to questions about what would be the effect of tax changes.

38. But perhaps the most compelling reason why I think we cannot rest at all content with the present position is the fact of the very wide differences of opinion which appear to exist between economists on quite fundamental aspects of policy. Such differences have long been a popular butt for the layman, and it is only fair to say that his impressions are sometimes based on misunderstanding. Nevertheless, there is a disturbing amount of truth in what he says.

39. Let me take only two simple and well-known examples. It is now a major problem of Western countries to find a method of reconciling a high level of employment with price stability. This is a practical problem in the sense that most Governments would very much like to be able to do it, as is quite obvious from reading the continuous official pronouncements on the subject. Economists have been, and to some extent still are, broadly

¹ Cf. Dudley Seers, *op. cit.*

divided into those who think that prices rise because of excess demand which creates conditions which pull up prices, and those who think that even there is not excess demand (unless this is defined by the existence of rising prices) prices will still rise because the modern economy offers no obstacle to their rising except in conditions of very severe unemployment. On the first view there ought to be a very close correlation between total demand in relation to resources and the movement of prices, while on the second there is an indeterminate element, and the problem has to be attacked partly by social or institutional changes.

40. The confusion which exists on this subject was well illustrated in a recent number of the *Scottish Journal of Political Economy*,¹ which was entirely devoted to it and where a number of reputable writers put forward very varied views, not only on the causes of price increases but even on the facts which they were trying to explain. If it is any comfort to economists in this country, a recent American symposium on the subject of *Economic Stability and Growth*, which contains no less than 712 pages and over forty contributions, contains in the General Summary at the end, in an article by Professor Ackley, the following passage. He is discussing the two schools of thought to which I have referred, the demand inflation and the wage/cost inflation

“ Economists have talked so much about these two kinds of inflation in recent years that it seems to me strange that they have not been able to say more about what has been happening. In particular, I would like to know to what extent the inflation in the United States since the war has been demand inflation and to what extent, if any, it has been wage cost inflation. I find very little discussion and no agreement on this question.”

41. Another example is in the question of the use of interest rates as a general economic regulator. I remember the symposium published by the Oxford University Institute of Statistics in 1952, soon after the revival of the active Bank Rate policy in the United Kingdom. It seemed to me then that there was a good deal of uncertainty, and this has really been going on among economists for a very long time. The main difficulty from a practical standpoint, in assessing the various theories on the subject, is that one is often left in uncertainty as to the various causal steps which are implied in a particular view, and it is thus impossible to subject them to any sort of empirical check. Indeed, there is still a tendency to speak in magical rather than scientific terms of the use of interest rates and of monetary control generally. The economy is to be given a potion or dose, and wonderful results will follow. Other examples will occur to everyone familiar with the subject.

42. What I have said so far may perhaps suggest that I think that the economics is of little practical value, apart from, so to speak, training the

¹ For June 1958.

drivers or the helmsmen. But this would clearly be going too far. Having been critical, I ought to say something in conclusion about the way in which things might be improved. I do so with some trepidation, and am uneasily conscious that much of what I have to say may seem very obvious. But unless the practical economist is prepared to tell his academic colleagues something of what he wants, he can scarcely claim the right to complain.

43. First, as to the problems to be worked on. It is not for me, or anyone else for that matter, to try to lay down a research programme for the profession. But it is useful to bear in mind that economics began as an attempt to clarify the issues of political dispute; it has been said that its proper function is metapolitical. While I do not suggest that the whole profession should henceforward devote itself to controversy on issues of current policy—my life would be even more difficult if it did—I do suggest that practical preoccupations can be a valuable guide to the direction of research. Logical precision for its own sake is a very beguiling siren—it took centuries before people gave up the attempt to square the circle—and one needs strong protection against this temptation.

44. I must confess that I am often surprised as much by the subjects which are not worked on as by those which are. I speak with diffidence here, for I do not pretend to have more than a very general acquaintance with work in progress. But I have been struck, for example, by the fact that, although price theory has always occupied a large part in the profession's attention, surprisingly little work seems to have been done so far on such questions as the variability of profit margins with the general level of activity, which modern statistics now make it possible to investigate. The multiplier was understandably out of fashion so long as the economy was largely supply-determined, as it was in the earlier post-war years. But, now that we have returned to more normal conditions, I would have expected it to recover its attractions as a subject for research. Indeed, much of the discussion about the probable consequences of the last Budget has turned on what value the multiplier has. The relation between exports and the level or rate of growth of home demand has been another very important practical issue, but again I have come across little academic work which bears on it. Indeed, the whole subject of the balance of payments seems to be suffering from some neglect, particularly in its dynamic aspects.

45. These are only particular examples, intended to illustrate and not to exhaust. I wonder also if we are not in danger of taking our conceptual apparatus too much for granted. The classes into which we divide economic events—be they commodities, classes of expenditure, types of output or of income, etc.—are all aggregates of individuals with differing characteristics. We tend to assume that totals or averages for the classes will summarise all the relevant characteristics of the frequency distributions within them. This is by no means self-evident. For example, to the best of my knowledge, those who have taken part in the discussion of wage drift in recent years

have all assumed implicitly that the differing rates of growth of average wages and average earnings which they are investigating result from differing rates of shift in the two frequency distributions whose characteristics are summarised by these averages, and not from mere changes in the shapes of the distributions. I would expect that the nature of the explanation of wage drift might differ considerably according to whether this assumption is true or not.

46. More generally, there continue to lie unused large banks of potentially valuable statistical data, in the shape of the primary material of which official statistics provide one form of summary. There are obvious practical difficulties about more intensive work on this material, and I do not wish to prejudice in any way the position of the official statisticians. But there seems to me to be here scope for valuable work by concept makers and concept users.

47. As to methods of work, there is first the old and simple rule that, if your facts or theories lead to conclusions which are at variance with your common sense, then you need to look again at your facts or theories. This recommendation may seem so simple as to be almost insulting. But, if I may make a personal confession, I am frequently struck by how much value I get from my academic colleagues in oral discussion compared with what I learn from their writings. Indeed, much of the profession seems to exist in a state of controlled schizophrenia—once paper is on the table, good sense and a sense of proportion seem to fly out of the window.

48. Narrowing so far as possible the limits within which there is disagreement between economists can be done with any prospect of success only by appeals to experience. It follows therefore that the most useful economists, from a practical point of view, are those who check their results against what has happened already.

49. This is a laborious and expensive task which is quickly brought up by limits of time and money. It seems to me, however, that we often make things more difficult for ourselves than we need to do, simply because we do not work in a standard form, which would at least make it possible to compare different pieces of work in the same field. Thus even if the resources are not available for the empirical research which would be needed to test any particular piece of theoretical work, much could be done by defining the terms in ways which would have a precise meaning to economic statisticians.

50. It hardly needs to be said that it is also an essential requisite that if forces are thought to be at work, the person writing about them should explain exactly how the changes which he is predicting would take place. For otherwise it becomes impossible for anyone else working on the same subject to speak the same language. To elaborate a little on a simple example to which I have already referred, if it is thought that monetary policy will produce a change, one must define the sectors which it is ex-

pected will be affected and how the processes will work. If monetary restrictions affect borrowers, will they be as Sir Ralph Hawtrey has argued for many years, in the first instance the holders of inventories? or will they include those who are borrowing for fixed investment? And if the monetary restrictions are supposed in turn to affect the movement of prices, what is the route through which changes in investment will affect the price level?

51. The explicit formulation of hypotheses in this way would help to guard against what I regard as a besetting sin among economists; that of enunciating purely definitional relationships when they purport to be making statements about reality. I have referred already to the difficulty of resolving disputes in the field of monetary policy arising from the truncated, not to say mystical form, in which theories are enunciated. The dangers can be illustrated from the report made by the staff of the International Monetary Fund last year on International Reserves and Liquidity. This is in many respects a most admirable document. But it is constantly on the edge of suggesting that because, *ex post*, changes in reserves are identically equal to the excess of saving over home and overseas investment, all that a country needs to do in order to build up its reserves is to raise slightly its rate of saving in relation to the rate of investment.

52. This sin is so pervasive that I almost wish that it were within our power to mark out in some way any economist who failed to use consistently the identity symbol when writing a definitional equation and to confine the use of the equals symbol to empirical relationships or hypotheses.

53. Proper formulation will also help with the problem of communication between different kinds of economists. It is becoming more and more difficult for the practical economist to learn from his academic colleagues or, I suspect, for any but the most industrious in the academic field to keep up with developments in other branches than his own. This is partly because of the gap between the mathematical and the non-mathematical economist. But it also results from the bad habit of economists of inventing (different) individual definitions for words in common use. In addition, there is a very real need for the occasional short and easily comprehensible review of the state of knowledge, including empirical knowledge, of the particular branches of the subject. The task of preparing them may seem to many academic economists to be so pedestrian as to be beneath them. I remember, however, that, before the War, the annual review of economic theory in *Econometrica* was much more than a mere summary; the stimulus of compiling it often produced new ideas. Since the War we have had two extremely valuable surveys of contemporary economics produced in America. Neither the time nor the expertise of the practical economist is unlimited, and it would certainly help him if he knew where he was, both in the progress of the subject and in its terminology.

54. All this sounds so extremely elementary that it may be asked if it is worth saying at all. And yet we come back to the large measures of disagree-

ment among economists about the objects and methods of practical policy, which can only arise either from the inability to get any facts with which to check the theories, in which case the theories cannot be said to have any practical application; or from the inability of economists to put their results in a form in which they can be checked empirically; or finally from the lack of resources to make the check.

55. There have always been economists who have sought to combine theory with practice. I think that their number is increasing. And in recent years we have seen the development of such bodies as the Institute of Statistics in Oxford, the Department of Applied Economics in Cambridge and the National Institute of Economic and Social Research in London. Similar bodies exist, here and in other countries—their names will be familiar to you all. If economics is to make further progress as a body of knowledge which can be applied to practical affairs it is to the collaboration of economists with bodies such as these that we must look.

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THE PROFITABILITY OF INVESTMENT

I do not propose in the course of these lectures¹ to advance any new model of economic growth—a model in which profitability of investments has been specifically elaborated. Unquestionably we need new and better theoretical formulations of the problems that I shall examine—I am quite aware of this—but in the international division of labour between economists I find that my own comparative costs for the skilful art of model-building are too high. Mathematical economists and econometricians can do this much better and much more easily. In my view an excess of our best skills and energies are directed to creating mathematically refined models of growth. Too little addition to knowledge is coming from the more or less intricate growth models. They give us alternatives for interpreting reality that are too numerous and too wide. And there are too few acceptable criteria for a good choice to be made as between them.

On the other hand, we have the statistical pictures of economic growth, presented more or less independently of the mathematical models. Here the definitions of variables and the basic assumptions as to interdependence are more or less hidden. The statistical time series of national incomes and capital stocks invite over-bold economists and econometricians to interesting and exciting interpretations of the process of economic growth. But the gaps between the statistical results and the interpretations are wide and often impossible to bridge.

SOME ISSUES OF ECONOMIC POLICY

The modest contribution that I have to offer is partly concerned with a few elements of a growth model and partly with the interpretation of corresponding elements of statistical knowledge. My problems refer to the notion of marginal efficiency of investment, and from the point of view of the statistical picture I am seeking to interpret the notion of profitability of investment.

My researches in this field arise out of current economic policy issues in Sweden. This is nothing to be ashamed of. There is an old tradition of close connection between economic policy and economic research. Its danger is that economists are tempted to be superficial and to base realistic policy conclusions on insecure foundations.

The issues that form the background to my research are not peculiar to Sweden. There has been a great scarcity of savings during the whole of the post-war period, and we expect that we shall continue to have great difficulty in providing for the growing volume of worth-while investment. The last

¹ This article represents a shortened version of two lectures delivered by the author as Marshall Lecturer in the University of Cambridge.

five-year survey of Sweden's future economic development, presented two years ago, predicted growing pressures. On top of a rapidly rising standard of private consumption, natural in a full-employment economy with an automatic and stable process of income inflation, very important social reforms were coming: a gradual shortening of the working-week, and a radical and general improvement of the pension system. The long-term investment programme emphasised the important sectors of the economy where investment had been neglected: housing, and various kinds of public investment in hospitals, schools, roads, water and sanitation, and so on. At the same time the investigation revealed rapidly growing investment plans in the private sector of the economy, which was adapting itself to great technical changes and to expanding markets at home and abroad. The method used in carrying out the survey of future investment needs and savings was very crude and failed to consider the implied relations between capital formation, production and prices. None the less, the recommendations for a higher gross investment and savings ratio in the Swedish economy and for more "rational" methods of determining priorities for investment projects provided plenty of material for debate on economic policy.

The questions I have in mind are thus: How much does it help, from the point of view of productivity, to raise the total gross savings ratio of the economy by so and so much? Or, how much lower will the rate at which production increases be if the savings ratio is forced down? Tendencies of this kind are more or less taken for granted, but there seems to be a great deal of variation in opinion as to how powerful the effect is and how quickly it operates. The answer is naturally affected by the composition of investment. To what extent does an increasing share of long-term public investment, including housing, affect the rate of increase of production as compared with a development more favourable to investment in private industry and trade? We need to investigate the effects of allocation as between branches of industry, types of investment and firms of different kinds within a branch. Looking at the question from another angle, we arrive at the intricate problem of how and by what criteria are gross savings allocated to the various channels of investment? In Sweden, the whole problem of the functioning of the credit and capital markets in relation to the productive use of savings has been brought very much to the fore in the heated debate on pension reform. The structure of the capital market will be radically changed by the creation of the huge pension fund proposed by the Government, ultimately reaching the order of one year's national income, to compensate for the expected decline in private savings and to meet the needs for increased savings. Is this change going to be for the better or for the worse? And from whose points of view?

What can economists say about such policy issues? What criteria can we use, not to solve impossible problems but to make more fruitful a sterile debate, in which the combatants are armed only with vague principles of

sound or progressive economic policy? My intention is modest: to try to use available theory and statistics to clarify problems of this kind.

THE LIMITATIONS OF SIMPLE GROWTH MODELS

I shall begin by examining some of the well-known growth models to see how far they can help us to understand such problems. I recognise that the models have not primarily been created to elucidate the particular problems I have in mind. Abstract theories of capital formation, such as Harrod, Domar, Mrs. Robinson, Kaldor, Champernowne and others have constructed, are of great importance in the development of economic thinking, not least for making precise sense or nonsense of vague statements in the discussion of economic growth. We enjoy or do not enjoy each other's models, and can give reasons for doing so. Sometimes an economist thinks that he can destroy a theory by showing that it is founded on an extremely abstract and unrealistic hypothesis that can never be tested. This is, of course, an illusion. The abstract hypothesis can be defended as being an accepted way of thought, belonging to an important line in the history of economic doctrines, and in the model these ideas are only brought out more clearly. Even if no empirical test can be applied, the features involved in the abstract hypothesis might be defended as essential to the understanding of economic growth. The omission of such features and the incorporation of other variables and relations, just possibly capable of translation into econometrics, might make the model still more unrealistic. The hypotheses underlying our use and interpretation of capital and production statistics sometimes seem to me to be more risky than the pure theories. Be that as it may, my criticism here of the models in question, with and without statistical applications, is from the limited viewpoint of how far they help us to clarify the type of economic-policy issues I have mentioned.

My scepticism about prevalent growth models refers especially to the high degree of aggregation and the concepts of capital and investment connected with this. Of course, all model builders are aware of these difficulties and perhaps fear that, if they did not simplify in one respect, they would be forced to over-simplify in another and more important respect. The prevailing modes of model building contribute little clarification to the problems of how changes in the total volume and allocation of investment may influence production, and of how to judge the functioning of the credit and capital markets with reference to marginal efficiency and profitability of investments. What, in my view, is very much needed is a sensible interpretation of capital as a factor of production and a disaggregation of the concept, or the statistics, of the volume of total capital into parts. A disaggregation is needed to clarify the problems of the *interdependence* of capital and capital investment in various sectors of the economy. We miss essential features of the process of capital formation in a developing economy *if* we aggregate too much and leave out completely the interdependence problems. Moreover,

an analysis of the interdependence problems presupposes an understanding of the profitability criteria of investment in the various sectors of the economy. No easy way out can be found by assuming some kind of profit maximisation rule. Account must be taken of administrative behaviour, of current rule of thumb, and of the permanent and characteristic imperfections of the capital market. I am going so far as to maintain that we overlook essential feature of a dynamic and expanding economy if we make the assumption that a capital market is perfect, or even that it has a tendency towards perfection. To illustrate these problems, I propose to make use of some results of an interview study of profitability of investment in Swedish industry—a research project that I have been working on recently.

Let me begin with the problem of the effect of the rate of growth of capital on the rate of growth of net output. I shall use rough overall figures, assumed to be representative for the Swedish economy. If the average rate of profit on all invested reproducible capital is about 5% and if, as many investigations seem to show, the profit rate tends to be reasonably constant over time, and if there is a net rate of investment of say 10% per year, then, at a rate of growth of total net production of 2% per year, the capital accumulation should only account for one-quarter of this rise of production. 5% on the additional capital corresponds to only 0.5% of national product. An economic policy successfully promoting savings thus brought up the average savings propensity so much as from 10 to 15% would with the same assumptions only augment the rate of progress by one-quarter of 1%. Very small reward indeed for this great effort!

Here I have the first and very simple answer to one of my questions: how great is the marginal importance of savings for the growth of the economy? This interpretation is obviously based on marginal productivity theory where you can measure the importance of the factors of production from their remuneration. For the sake of simplicity I shall call this kind of interpretation the Douglas approach.

As an alternative interpretation, at the other extreme, I can take the Cassel approach giving maximum importance to capital accumulation. This is the simple exponential-growth formula used so commonly in the literature of economic growth, where the exponent is the relation between the net saving ratio and the capital coefficient. In the case mentioned above—if the marginal capital coefficient is stable at 5—the Cassel theory would imply an increase in the rate of growth of the economy from 2 to 3% per year as a consequence of the higher savings ratio (from 10 to 15%). It is clear that it is very important for economic-policy decisions whether the rate of growth of production is influenced significantly or hardly at all by a given change in the savings ratio.

More sophisticated versions of the marginal-productivity theory have often been used to give quantitative precision to the relative importance of factors supposed to determine growth of production: on the one side, inpu

of labour and capital; on the other side, more or less unknown factors summarised as shifts of the production function. A great number of applications of the Cobb–Douglas function have been made, and in spite of all the serious criticism directed against the method new experiments are continuously being tried. I shall mention briefly the results recently published by Aukrust for Norway and Solow for the United States, since it gives me a good starting-point in the search for alternative methods.¹

Aukrust, using a Cobb–Douglas function, finds for the Norwegian growth of real national income 1900–56 (excluding war years) an exponent for real capital of 0.20 and for labour of 0.76 and a yearly shift of the production function of 1.8%. Applying the findings to the post-war period 1948–55, when capital accumulation in Norway indicated a Western record (5.6% per year), Aukrust finds that the capital coefficient was increasing rapidly and that the contribution of capital accumulation to the growth rate of 3.4% per year in spite of this was less than a one-third of this rate. Aukrust takes these results seriously and uses them to show that a lower rate of savings would mean comparatively little to the rate of progress. More attention should instead be directed to the factors determining the shifts in the production function, technical research, administration, investments in education.

Solow has the same problem of segregating variations in output per head due to technical change from those due to changes in availability of capital per head. The time series of private non-farm output per hour shows a doubling over the years 1909–49—in real terms. According to Solow's econometric manipulations, 80% of this growth rate is accounted for by the cumulative upward shift of the production function; the average rate of shift during the whole period was 1.5% per year, and thus close to Aukrust's results. An interesting result, according to Solow, is that the rate of shift was less than 1% on the average up to 1929 and as much as 2½% per year during the period 1930–49, when "employed" capital actually increased at a significantly slower rate than the number of hours worked. The elasticity of production with regard to capital is about 0.35 according to Solow's production function.

We all know the shaky foundations of the Cobb–Douglas technique, both statistically and as a theory of production. As was pointed out long ago, the intercorrelations of the time series in question are apt to give very unreliable results. My own very rough experiments with Swedish statistics show that the differences in the closeness of fit are often not great enough when using elasticity coefficients for capital varying between 0.20 and 0.50% for one to feel sure that the one interpretation is clearly preferable to the other. And the results as to the rate of shift in the production function would be entirely different.

¹ Odd Aukrust, "Vebst og Struktur," *Bedriftsøkonomen*, December 1957; Robert Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics*, August 1957.

To this very wide range of choice as to the interpretation of accepted statistical time series must be added the unavoidable uncertainties of definition and measurement, especially of the growth of the volume of capital. The capital-volume series we have used to measure the trend of capital coefficients in Swedish industry have in my view an indeterminate range of about ± 10 –20% over a ten-year period; that means that we do not really know whether the volume of capital in Swedish industry has increased by 90% or 50% over the period 1946–56 (that is, more or less rapidly than the volume of production). This uncertainty is due both to lack of clear definition and to the fact that the definitions stand in an unknown relation to available statistics.

Before I examine these problems let me deal with a simpler and, in a way, clearer approach to our problem of estimating the relative importance of real capital accumulation in explaining the growth of production. This consists of measuring the rate of increase of total productivity by relating an index of the volume of production to an index of the volume of input of production factors of all kinds, including both labour and capital. The weighting becomes a delicate problem, but it is an honest index-number problem and is not concealed in a correlation procedure. It is reasonable to argue—as Professor Kendrick has done—that the relative weights of different kinds of labour- and capital-input units should be determined by their base-period prices, hoping that these approximate the marginal contributions to output during that period.¹ Such a measure should tell us how output would have developed if productive efficiency of the various inputs had been constant and the same as in the base period. And the relation of the two index series of actual and hypothetical output gives us the rise in productivity that has to be explained by other factors than changes in the volumes of labour and capital input.

I regard this method as preferable from several points of view. Discussion depends too much on the very crude and much misused concept of “productivity of labour,” in which the output index is related to only one of the factors of production. One can argue that as a measure of productivity an index also taking into account the inputs of capital must be “better” in some way than the usual index neglecting the process of *substitution* of capital for labour. The results are, moreover, of interest in a discussion of economic policy; that has been my experience when it could be shown that the “true” rate of increase of productivity in Swedish industry during the post-war period has not been 3.5–4% as commonly maintained but only about 1% per year. It is especially interesting when comparing rates of productivity increase in agriculture and industry, as well as in different branches of industry. The results of such comparisons between branches seem more relevant when some account—however approximate—is taken

¹ John W. Kendrick, “Productivity Trends: Capital and Labour,” *Review of Economics and Statistics*, August 1956.

of the very wide differences in the rates of capital accumulation. The Kendrick approach, although in a way less ambitious than the Douglas approach, does not, of course, eliminate the fundamental questions of uncertainty as to the interpretation of the results. The questions are how shall we interpret the changes in volume of capital input; what sense there is in the choice of the relative weights for capital and labour inputs: the necessity or advantage of assuming a given production function that is implied equally in the Kendrick approach. These questions are obviously closely inter-related.

VOLUME OF CAPITAL CONCEPTS

Let me start with the statistical problem of measuring the development of the volume of capital for a branch of industry or for the economy as a whole. Goldsmith and other specialists in this field, who produce measurements over long stretches of time, apparently care little about the arbitrariness of the results and the uncertainties of their interpretation. Take the problem of the deflators—hardly even mentioned as a difficult problem in the account of the methods. When using an aggregate of machinery price-index numbers for deflating fire-insurance values of capital or accumulated values of machinery investment in Swedish industry I had a very uneasy feeling of the uncertain nature of the underlying machinery price statistics. Do the prices of the machines included in the index take account of changes in the efficiencies of new machines—and if so to what extent? My belief, based on some acquaintance with our machinery price-index numbers in Sweden, is that the collectors of the price material try to include only standard machines with constant efficiency in some sense, using chain links to make adjustment for necessary changes of types. But I am sure that they do not succeed very well in this endeavour. The index therefore gives not the price of an unchanging “volume” of machinery services, but rather prices of machines with a rising trend of efficiency. If the efficiency of the machines is rising on average by, say, 1 or 2% a year, the use of this type of deflator gives a lower rate of increase of the machinery capital interpreted in productive capacity terms than a “correct” index would give. I dare not raise the question of how effective the machinery price-formation process is from the point of view of difference in quality and efficiency. Generally there is a tendency for an increase in efficiency of a new model that is greater than the rise in price. There is the same type of bias in the measurement of the volume of construction. A construction-cost index with relatively fixed weights tends to over-estimate a rise in building costs, and a rising efficiency of the use of factory space is, of course, not accounted for. The sad thing is that the statisticians do not really know what they are doing when they deflate the capital-value figures, and the economists do not know which capital concept the volume estimates most closely approximate.

This brings me to the difficult problem of the relation between the interpretation of statistical time series and model builders’ conceptions of

essential features of economic reality. In the production function approach, the theorist usually likes to consider capital as input of some kind of original factor of production, measuring input as costs of labour and materials embodied in the machines or buildings in question. An assumed production function gives precision to the notion of how much an increased volume of capital should add to production under given technical and other conditions. The rate of increase of "total" productivity—the shifts in the production function—represents our *ignorance* of the causes of economic growth.

It is, however, possible—as Kuznets has maintained—to define the volume of capital with regard to the capacity to produce embodied in the machines and buildings.¹ Difficulties as to time dimension and degree of utilisation are the same in both cases and can be assumed away. This means that if a machine costs as much as in a base period, in terms of input of labour and other real costs, but at a given rate of utilisation renders 50% more in the form of flow of services (for example, in horse-power), then there is a corresponding increase in the volume of capital. From the point of view of the reproduction cost of an existing capital structure, old capital goods should then be imputed values with regard to the cost of replacing them with modern machines built with improved techniques but giving a constant volume of services. Capital consumption may according to this approach be defined as the scrapping of old equipment with no deduction for obsolescence as long as capital instruments are repaired and maintained to keep constant capacity. This means that the depreciation charges of old capital will be probably lower than according to the input-cost approach (for example, the perpetual inventory method) and also that the rate of growth of the volume of capital will be more rapid. It may be significant that Kuznets' index figure for the growth of reproducible wealth in the United States over the period 1900–50 lies 30% above Goldsmith's measure.

In pure models the one approach can logically be made as precise, attractive or persuasive as the other. And there are no *a priori* criteria as to which model is less realistic or more difficult to apply. What I am interested in is the shift in interpretation of the rôle of capital formation implied. In the first approach our attention is directed towards the substitution possibilities between labour and capital under given static conditions—and, as the statistical applications seem to show, a relatively small part of growth is explained by this method. In the second approach investments are somehow supposed to materialise in capital goods of rising technical efficiency and productive capacity, with the result that a Kendrick method of analysis would result in a lower rate of unexplained rise of productivity. There would be correspondingly fewer "unexplained" shifts in a production function. It is evident, however, that the difference between the two types of approach means a transfer of ignorance from the unexplained shifts of the production function to the hypothetical efficiency effects of investments in

¹ See the discussions in *Problems of Capital Formation*, Studies in Income and Wealth, Vol. 19, 1957.

new capital goods which are transposed to a capital-volume index. One important consequence of this latter approach could be that capital accumulation would be regarded as a more important factor of growth. This is on the condition that when the rising efficiency of capital goods is incorporated in the definition of capital volume, the weight of capital can be assumed to be the same, independent of the definition and measurement of capital growth.

SOME MORE SOPHISTICATED THEORIES

At this point I come very close to profound problems intensively discussed at a high theoretical level here in England. I am especially thinking of the theories and definitions developed by Mrs. Robinson, Mr. Kaldor and Mr. Champernowne.¹ I am not competent to judge the intricate details of their models—and to what extent the results may clarify my problems. As far as I have understood the issues as to capital formation involved in the discussion, a fundamental dilemma resulting from the application of classical tools of analysis is clearly unveiled. The production—or for that matter the productivity—function is a useful instrument for showing a substitution process of capital for labour under static conditions—where technical knowledge is given. The concepts of capital volume and marginal efficiency or productivity of capital can then be given a precise meaning. When the object of analysis is widened to include discontinuous or continuous changes in applied technical knowledge, the assumed or given shifts in the production function limit the explanatory value of the theory very much. Widening the system to include these dynamic changes will, on the other hand, upset the system of explanation a great deal and spoil the simple definitions of capital and marginal productivity. The growth of capital volume can strictly be regarded as an accumulation of net investments only during the substitution process when real wages are assumed to be constant. The introduction of important technical innovations upsets the equilibrium, causes serious changes in income distribution and profit expectations and makes revaluations of existing capital equipment necessary. But the rules of valuation must be rather arbitrary, if the disturbance means an upsetting of the equilibrium conditions. In any case this type of approach implies changes in capital values that are not regarded as consequences of net investments. The marginal efficiency of investments defined as the profitability of substituting capital for labour assuming given technical knowledge becomes something very different from the marginal productivity of capital. This concept should then refer to more or less arbitrarily determined changes under dynamic conditions in the values of capital and net output, both in real terms. Of course, energetic model builders overcome a dilemma of

¹ Joan Robinson, "The Production Function and the Theory of Capital," *Review of Economic Studies*, Vol. XXI (2), 1953–54; N. Kaldor, "A Model of Economic Growth," *Economic Journal*, December 1957; D. Champernowne, "Capital Accumulation and the Maintenance of Full Employment," *Economic Journal*, June 1958.

this kind by introducing new sets of assumptions and by finding consolation in points of dynamic equilibrium.

My objection to the type of model Mr. Champernowne has built with great skill refers to the unnecessary extra complications that arise because of the artificial position of the static production function in a system explaining dynamic economic change in the economy as a whole. The term "artificial" is used as a subjective-value judgment. You can defend the production function as an important tradition in economic thought; making use of it is thus good for pedagogical reasons. A model is apt to be accepted if it contains traditional relations such as consumption and production functions, and part of our profession consists of training our skill in handling these tools for nearly all purposes. It may be argued that the production function is not more unrealistic than the Keynesian consumption function for model-building purposes. My contention is merely that the way of posing the problems that is implied in the production function does not clarify the issues I wish to have clarified.

My simple premise is that the investment process can be understood only when the reference basis is an economy perpetually out of static equilibrium. There is a more or less continuous flow of disturbances brought about by technical change as well as by other changes in demand and supply conditions. The resulting disequilibrium positions create investment opportunities, and then the investment process is continually creating new disequilibria. Only a minor part of the investment problem can *a priori* be regarded as a static substitution problem. If this view is accepted as plausible, then an effort to use the production function as an instrument to explain long-term growth must be considered as very inadequate. Solutions along these lines will from this point of view seem artificial. But of course I cannot prove this.

In Mr. Kaldor's model of economic growth¹ the distinction between movements on a production function based on a given technical knowledge and shifts of such a function is abandoned. Instead, he introduces a postulate of a given relationship between capital accumulation and growth of productivity. Instead of the traditional assumption of the production function he makes the heroic assumption of a given curve—the technical progress function—presuming a constant flow in the rate of new technical ideas and possibilities—and lets the rate of capital accumulation determine to what extent the technical opportunities can be used. In this way investment is completely and inseparably combined with the dynamism of technical change. A mutual interdependence is pictured: the speed with which an economy can absorb capital growth depends on its technical dynamism at the same time as the speed at which technical innovations can be applied depends on the supply of savings.

¹ See reference above. I am sure that Mr. Kaldor has built many new growth models since 1957.

In spite of the high level of abstraction, I consider this approach based on the technical progress function as being more promising for an understanding of the rôle of capital formation than models built on the static production function. In this discussion I am not interested in Mr. Kaldor's ambition to find a solution to his system of long-run equilibrium growth. I am only a little sorry that it compels him to introduce a number of simplifying assumptions that make his model impossibly remote from the level at which I need help to understand reality.

What explanatory value can we find in the highly abstract technical progress function and Mr. Kaldor's method of relating it to capital accumulation? It shows that productivity would grow even at zero rate of capital accumulation. This constant in the technical-progress function corresponds somehow to the shifting of the static production function; productivity will rise for a number of reasons independently of capital accumulation. What is left unexplained should, however, be less than in the Douglas or Kendrick approaches. Again the position of the curve will depend on the definition and measurement of capital and its rate of growth. The more "gross" we define investment in order to include efficiency gains when replacing old capital goods, the smaller will be the increase of productivity explained by factors other than capital accumulation. But something should always be left to be accounted for by a multitude of factors, such as improved skill and organisation, cost reductions of various kinds, enlargement of markets; all these are factors contributing to more efficient methods of production without there being any need for capital investments of the type here discussed. The continuous process of growth of technical knowledge, shifts in demand functions, price and cost changes, must mean that firms are constantly out of equilibrium, that is, with a below-optimum allocation of resources. Lagging adjustments of production and sales policies—the lags being partly determined by costs of administration, lack of information, or absence of incentives—will mean a stream of efficiency gains at the various points in the economy, which may be assumed to sum up to a yearly increase in productivity for the economy as a whole.

Interview studies in Swedish industry give me a strong impression of this factor; managers and engineers know that all the time there are a great number of suboptimal disequilibrium positions within a firm. At an interview the management of a large engineering firm maintained that at nearly any stage in production costs could be reduced by about 10%, without there having to be any investment, after a close scrutinisation of the methods of production; but there was not sufficient time and administrative staff available to catch up with development. I have obtained some sort of empirical evidence of these possibilities from a steel-producing concern. In one of its plants no fixed investment at all was made during a ten-year period; there were only expenditures for repair and maintenance, while in the other plants of the concern new investment for expansion and modernisa-

tion was considerable. The rate of increase of production per man-hour for the concern as a whole varied between 4 and 5% per year, and was about 2% in the steel plant without investment. I have several similar instances of this phenomenon, but certainly not enough evidence to generalise.

The most interesting property of Mr. Kaldor's model of growth, from the point of view of my problems, is that the marginal social efficiency of investment, as it perhaps could be derived from the technical-progress function, has very little to do with the average level of the rate of profit. The effect on production of a change in investment depends upon the slope of this function at the point in question, and this slope is determined by the investment opportunities created and by the flow of technical innovations. The sad part of my interest in Mr. Kaldor's model is that this slope, which might give me a beginning to an answer to my question regarding the effect of an increase in the investment on the rate of growth, is not revealed in any outward manifestations of the working of the model: not in the rate of profits, nor in the rate of progress, nor in the inverted capital coefficient. Changes in profits act only as short-term regulators of savings—and even in the long-run the dynamic equilibrium rate of profits on capital has no clear singular relation to marginal efficiency of investments. Is this a peculiarity of Mr. Kaldor's model or something essential to be found in economic reality?

My contention is that this lack of relation between actual *ex-post* profitability of investments and social as well as private marginal efficiency of investments is an important feature of reality and should be demonstrated in a good model. You cannot regard actual profits on capital as even approximately measuring the effect on production of changes in capital volume as the applications of marginal-productivity theories try to do. It is always dangerous to pick out and isolate one relation within a system of interdependent variables and draw conclusions of causal significance. To the extent that profits are supposed to be determined in a set of equations, then the *ex-post* profits registered may have very little to do directly with a change in the volume of capital in a production function. The same holds, of course, with regard to changes in the degree of utilisation of the capital stock.

On the other hand, I feel very critical as to the *way* in which Mr. Kaldor deals with the interdependence of capital formation, profits and changes in the marginal efficiency of investments. Very many simplifying assumptions have, of course, to be made in order to make the model manageable. The entrepreneurs seem to be very short-sighted in his model, as they are not aware of the profitabilities of investments that the technical-progress function implies. Investments are determined mainly according to the acceleration principle—with some regard being paid to *ex-post* profitability of capital—and the entrepreneurs are continually being surprised by greater increases of productivity than they had expected (on the condition that they are to the left of the long-term equilibrium point, otherwise the surprises will be in

the opposite direction). In a way, it is strange that when the flow of investment opportunities is given as being very much dependent on the growth of technical knowledge—and this is a realistic feature of the model—the entrepreneur should be more or less blind to these opportunities and only react to the expansion of the market that has just occurred. In fact, the entrepreneurs and their staffs of technicians and engineers will really be more or less busy creating small pieces of the technical progress curve and they must therefore be aware—although, of course, in varying degrees—of the opportunities for investment arising from technical innovations. This is self-evident, or interesting, or startling, depending on the extent to which we have taken a model seriously and been prisoners in its cage, or have fallen in love with it.

SOME EMPIRICAL EVIDENCE ON CAPITAL GROWTH

My proposition, then, is to consider entrepreneurs as experts on the technical-progress function on the micro level for the individual firm. Many of them know a lot about it, although in varying degree in different firms. Imperfections and uncertainty in knowledge have to be examined. In a model we have to make precise assumptions about these things. My contention is that by studying the profitability calculations and expectations of returns from different kinds of investments in the business firms we might find out something about the slope of Mr. Kaldor's technical-progress function. We cannot avoid meeting on this roundabout route the insoluble problems of how to aggregate the micro relations of private marginal efficiency of investments to macro concepts of social marginal efficiency relation for the economy as a whole. My point is, however, that a break-up of the aggregates is needed in order to see what problems we are really assuming away.

In the study mentioned above of profitability of investments in the Swedish economy, I find it fruitful to pursue the analysis on the macro and micro level simultaneously. The aggregated statistics of capital, investments, profits and production in total industry and in the various branches of industry can be much better understood and interpreted if intensive studies of how corresponding things happen and how they are expressed in statistics are made on the level of the individual firm. In the aggregates important features of the story may be eliminated. Misinterpretation of the statistics may be avoided if we know how things occur on the firm level and not least how the statistics are reported and created. Of course, there are great risks and much waste of time with interview studies. The interviewing of something like 100 industrial firms is apt to give a bias. The selection of firms has in my case nothing to do with a well-structured sample. It is instead heavily influenced by such irrelevant considerations as that the firm is relatively big and the managers are open and interested in the problems in question. The interviews have been partly of a more extensive kind in order, for example, to get a general impression of the extent to which profitability

calculations have been made, and if so by what methods. More intensive personal interviews have been made of about twenty companies. On some occasions it has been possible for me to take part in the profitability discussions of the management of the firm when investment decisions were being made.

What can be the outcome of such small-scale interview research? I am not in a position to apply significance tests to the results. My intention is only to survey the types of problems that an economist meets when he tries to understand problems of marginal efficiency of investments from an *ex ante* point of view. For my own part I have been learning a great deal about criteria for investment decisions—and that is certainly an important element in this kind of direct observation. One clear outcome is that my propensity to generalise from official statistics and oversimplified macro-models is much diminished.

Let me begin with some comments on the measurement of the growth of capital volume and of capital coefficients in Swedish manufacturing industries. The official statistics of fire-insurance values, deflated by the special machinery price and construction-cost indices used by the insurance companies, give as a result capital coefficients varying from 1–2 up to 6 for the various branches of industry. Looking at individual firms inside the branches we also find great dispersion. If agriculture, power production and housing are included the dispersion is, of course, still wider—up to 15. All averages for the economy as a whole must therefore be closely watched with regard to changes in the composition of the national product. For manufacturing industry as a whole, as well as for the various branches, we generally find higher marginal than average capital–output relations.

Capital volume, according to our measurements, has tended to increase more rapidly than production volume during the post-war period, and indeed from the end of the 1930s if one may trust very rough estimates. Barna seems to have reached similar results for England.¹ How significant are such results? The problem is important from the point of view of estimating future need of savings for economic growth.

I have already mentioned the difficulties of price indices and definitions of capital volume. I shall only add that alternative measures of capital volume, namely the index of horse-power installed and the perpetual-inventory method, gave so much lower growth rates that the conclusion about rising capital coefficients is in any case rather doubtful. Regarded as an efficiency or capacity measure of capital growth, fire-insurance value may be more significant than as a measure of capital input. In our interview studies we tried to check the significance of the fire-insurance statistics. We drew graphs showing development of fire-insurance values, cumulative figures on gross investments in real terms, production, employment for firms

¹ T. Barna, "The Replacement Cost of Fixed Assets in British Manufacturing Industry in 1955," *Journal of the Royal Statistical Society, Series A*, Vol. 120, Part 1, 1957.

interviewed and inquired about strange deviations in the curves. It was a fruitful way of finding out sources of errors. The general impression from these discussions was that fire-insurance values tended to overstate reproduction costs of reproducible capital (after allowance for depreciation) but not by very much, but that the trends of growth could be treated as significant for capacity development. It was, however, disturbing that at intervals of five to ten years there was usually a complete revaluation of the fire-insurance values—and it cannot be judged to what extent these revaluations tend to average out in the aggregates.

One possible method of answering the question of whether capital coefficients tend to be rising in Swedish industry would be to try to compare capital–output relations derived from fire-insurance statistics with corresponding relations derived from expansion programmes planned or in process of realisation. But here we meet many interesting difficulties. Obviously one must not compare planned investment expenditures with expected value added; to make the figure comparable with *ex-post* relations based on fire-insurance and production statistics, the investment figure must be reduced to some kind of average value of real capital over the life of the investment. The arbitrariness of any such deduction leaves little confidence in a comparison with *ex-post* average and marginal relations based on fire-insurance values. The few comparisons we have been able to make seem to give low capital–output ratios for new expansion programmes as compared with existing structures.

One must be very careful in drawing conclusions, however. An expansion is usually based to a varying but sometimes quite important extent on the use of already existing facilities of the firm—power supply, transportation equipment, warehouses—where there is unused capacity. This is particularly the case when old buildings can be used and the expansion refers only to machinery and equipment. The need for new factory building may come later when higher profits at the expanded level of production make financing easier. To this factor is due a not uncommon tendency to underestimate *ex ante* the total cost of investment, not least because of an unexpected rise in the need for circulating capital.

The capital needs of an investment project are only one part, and certainly the easiest part, of a profitability estimate. And it is on this point, as I have said, that I like to use the entrepreneurial expert knowledge to obtain some basis on which to judge the slope of a Kaldorian technical progress curve, or of Keynes' marginal efficiency of capital. Let me start again with the *ex-post* values. On average for the past five years I find a level of gross return on capital of 8–10% before taxation in Swedish manufacturing industry, corresponding to perhaps 5–7% net return. This level of *ex-post* profitability in Swedish manufacturing industries is much lower than Barna's very similar estimate of about 15% gross profitability for British manufacturing industries—as presented in his address to the Royal Statis-

tical Society in 1957. But I think Barna tends to overestimate gross profits considerably through his method of making subtractions from value-added figures. Using Barna's method, I also get 15% gross profits in relation to total assets. The reason is that all items of cost are not accounted for in the production statistics. My results are based on official statistics of profits that can be made approximately comparable with the statistics of fire-insurance values, and I think this method is a little more reliable. A low *ex-post* profitability on invested capital is also a phenomenon I very often find in the interview studies of the individual firms. I have quite a few examples of rates of net return approximating 5% (before taxation) on reproduction values of existing capital, and this was during the very good years 1955–57, and refers to corporations which were probably among the more efficient in the respective branches.

The best way of valuing capital will always remain a puzzle. The book values of the companies' own capital (share capital and funds) were very much lower and yielded much higher returns. To the extent that the reproduction cost valuation measures costs of re-erecting the capital structure of the firm with to-day's costs of reproducing old capital goods, or substituting with new capital having the same efficiency as the old ones, it is quite natural that net *ex-post* profitability shall be relatively low. To what extent fire-insurance values or expert evaluation approach such an indeterminate principle of valuation can never be very well ascertained because of the lack of clear criteria. The unsatisfactory feature of this kind of measure of profitability is that we are again trying to introduce a pseudo-static reference scale in a world of dynamic change, in which essential characteristics of the conditions of capital formation cannot be properly understood if static notions are preserved.

PROFITABILITY ESTIMATES

I have dwelt on this notion of *ex-post* profitability of capital as it constantly arises in the discussion of economic policy. Representatives of private industry come back to calculations of this kind to show that Swedish industry cannot, after paying very high corporation taxes and dividends, even finance reinvestment of old capital. It is maintained that the very sound consolidation that is characteristic of the greater part of Swedish industry is a result of a long sequence of exceptionally good years of inflation. At prevailing profit margins and with the prospects of less inflation and more competition, determined among other things by the European free market, the outlook for profits is much less favourable. My own view is that industrialists in the political arena always tend to be pessimistic. As investment experts, planning for the future, they are apt to be more optimistic. I think that the presentation of some results of my interview studies will clarify the significance of such doubletalk.

I can summarise my impressions from the interview study under three

headings: first, the relatively low frequency of more exact profitability calculations for new investments; secondly, the wide differences in definitions, methods and basic assumptions in the calculations and the difficulties of translating them into some common "language" or norm of evaluation; thirdly, the high dispersion of yield expectations from the various kinds of investment within a firm, as between firms in the same branch of activity and as between different sectors of the economy.

Observations under these headings are, of course, closely related. Behind all the difficulties of comparison lies the subjective nature of every estimate of uncertain future costs and revenues. How and to what extent consciousness of the uncertainties of the future is included in the profitability estimates must be a very subjective judgment. Investment decisions have in any case to be made on uncertain and insufficient premises. Whether a more exact profit estimate is formulated or not may depend on such circumstances as the education and training of the entrepreneur and his men, the traditions in the firm, the effectiveness of an exactly formulated profitability calculation as an instrument of persuasion among the members of a board of directors and so on. The profitability estimate may in many cases have very little to do with the investment decision. Some kind of loose estimate and weighing up of possibilities is always done, although a quantification of the hunches may be regarded as unnecessary for decision making. In the interview studies I have tried to find out something about the frequency of profitability calculations with regard to various types of investment and branches of activity. An account of the results would, however, lead me too far from my main subject.

My own bias in favour of the view that more exactly formulated profitability calculations can be useful in a study of this kind should not be forgotten. My doubts and reservations will soon come. Let me first present some results. As I have said, the most surprising discovery to me—at least in the first stages—was the great dispersion in the expected internal rate of return (before tax) of planned investment projects. During the period covered, the good years of 1956–57, investment decisions were made on the basis of internal yields as low as 4–5% and as high as 70%. Yield expectations around 20–30% were quite common in manufacturing industry; but much higher and much lower returns were expected quite frequently. Investments in electric-power plants were generally expected to yield between 6 and 8%, while the figure for house-construction was around 6–7%.

There is not only a wide dispersion between branches of activity and between firms but also within a firm. In a big pulp- and paper-producing corporation the internal yield expected on investments actually being carried out varied from 3–4% up to 30–40%. Investments in forest plantation were being made on a very low profitability basis, while machinery purchases for modernisation and automation purposes showed very high expected yields. This observation of great variability in returns on actual investments within

one and the same firm was quite general. In a firm there always seem to exist many "golden" investment opportunities in the form of new labour-saving machines to replace old ones, of methods of saving fuel, investments to expand bottlenecks and the like, promising very high and quick returns. The responsible engineers have to be restrained by rules preventing them from buying machines that have a longer "pay-back-period" than one, two or up to five years—depending upon the firm's financial and administrative capacity. The yields will, of course, depend upon the expectations of the length of economic life of the machines in question, but mostly the returns will seem very high in comparison with the expected returns on investment in long-term expansion programmes. And these expansion programmes are given priority although they are only expected to yield a 10 or 20% rate of return, compared with a stock of "golden" investment opportunities yielding perhaps 30–50% on a comparable basis of estimate. These highly remunerative investments are squeezed out in order to make room for the financing of the expansion programme. Over a ten-year period companies have to make quite substantial investments in welfare or community projects of various kinds with no expected direct yield at all, but with opportunity costs corresponding to the postponed or neglected golden investment opportunities.

On this level of direct observation we obviously get a very chaotic picture of economic reality. This is nothing new; economists working on investment problems have always known that there were wide ranges of yield expectations. To present figures of doubtful significance may be considered irrelevant. But it is one thing that economists should know it. It is another that they should assume it away or forget it when they build their models. We may admire the courage of the economist in making order out of chaos by assuming a natural rate of interest or a schedule of marginal efficiency of investment, or a perfect capital market. I suggest that we should, to a greater degree than we usually do, accept this chaos on the capital market and, instead of abstracting from it, analyse the causes and effects of very high dispersion of expected investment returns. It is from this angle that we should try to understand the functioning of the capital market and the relatively low *ex-post* returns on capital.

THE INTERDEPENDENCE PROBLEM

There are many difficult problems connected with the interpretation of business-men's expectations of net yields of investment. I leave aside the niceties of financial mathematics as secondary. I would only remind you that to summarise the economic prospects of an investment by giving only the internal rate of return neglects a number of considerations. I also leave aside the question of the compatibility of subjective biases due to the temperament of the estimators: some estimate hopefully, others conservatively; we have here all the various methods of transforming doubt into certainty, subjective ways of including contingency allowances in price and cost fore-

casts, in estimates of average degrees of utilisation of capital, in estimates of time-lags between investment costs paid out and the full operation of the plant and the length of its economic life.

Allow me in spite of all this to assume the business-men, making profitability estimates, to be experts on the real rate of interest and the marginal efficiency of investment. Some correction for apparent differences in methods and assumptions can be made, but of course never on a perfectly objective basis. The results will, I am sure, still show a picture of very wide dispersion in expected net returns on investments. My intention when leaving out these, I admit, somewhat disturbing questions is to concentrate attention on three issues of great importance in the interpretation of profitability statistics: the interdependence of investments in various fields of activity and therefore of returns on investment, the dynamic effects of investment on income distribution, and the influence of costs of capital looked upon from the point of view of the capital market.

The question of interdependence explicitly or implicitly enters into every investment problem. In the simplest form the bank meets it when, for instance, scrutinising the credit-worthiness of a firm's machine-investment. It often happens that a very fine profitability estimate is presented, but that the bank manager has to point out that a considerable amount of supplementary investment is needed to finance increased circulating capital. It is not uncommon even in big corporations that the investment cost of an expansion programme is underestimated considerably because of an underestimation of the needs of increased circulating capital as well as of more of additional credits to customers and greater needs of liquidity. In the case of an iron and steel plant, to mention only one example, an expansion programme extending production capacity by about 50% implied needs of increased circulating capital corresponding to about 60% of planned expenditure for fixed investments in factory buildings, plant and machinery.

But the interdependence problem goes deeper than the mere accuracy of estimates. High profitability of particular machinery investments in a firm is often dependent on some surplus capacity of plant, buildings or transport equipment; this is, of course, most clearly the case when a bottleneck is eased. When ranking investment projects from a short-run point of view this is right. But from the point of view of the balanced growth of the firm in the longer-run this kind of direct profitability estimate is less valid. The golden opportunities of bottleneck investments are dependent on the general development of the firm and the profitability prospects of the firm's total capital. It may, therefore, be more profitable for the firm in a given situation to postpone investments which seem much more profitable than the long-term investment programme. In this case it can be said that the profitability estimates are not accurate guides, either for the investment policy of the firm or for the economist trying to understand something about the marginal efficiency of investment. The very low return on investment

in forest-growing in the pulp and paper companies mentioned cannot be analysed as an isolated factor. Current high expected returns on machinery investments are to a larger extent determined by long-term investments in the forests made thirty years ago.

The nexus of difficulties in estimating net returns that I am here approaching is evidently closely related to the classical problems of internal and external economies. When a big aluminium company was interviewed about expected returns on a planned expansion programme they appeared promising, giving something like 20% net internal yield. But this profitability estimate was based on unchanged costs of electric power. After revision of the estimate to take account of investment costs for the additional requirement of power, profitability turned out so much lower that, according to the manager of the company, the expansion programme would not have been considered on the basis of this latter estimate.

We meet here the strategic interdependence of public and private investments, which is especially important in Swedish economic policy discussions. There are many complaints from private business about the rapidly rising trend of public investment—including government-regulated house construction. The public sector, in a wide sense and including house building, now takes about 60% of total gross investment of the Swedish economy, squeezing private investment in industry, agriculture and trade to about 40% of the total. It is pointed out that, according to the national accounts, the private sector—taking 40% of total gross investment—makes a contribution in the form of value added corresponding to 80% of national income.

I need not point out the sources of errors and dangers in such a comparison. What I would like to stress in this connection is the dependence of the relatively high net returns (before tax) of private investment on the large public investment expenditures that generally yield low or no returns. I find it a little difficult sometimes, however, to persuade members of Chambers of Commerce and Bankers' Associations that they should be grateful to the Government and the municipalities for their generosity in taking over so much of the economy's fixed capital investments, having very high capital coefficients and low or no returns—thereby making private investments more profitable than they otherwise would be.

An interesting feature of this problem of interdependence of public and private investments is that this interdependence operates differently in various sectors of the economy. Quite a large part of Swedish industry is located in the country-side, in small towns, where often one or two industrial companies (iron and steel works, wood industries or engineering companies) completely dominate the community. The investment costs in an expansion programme of such a company must often include quite a considerable amount of community and social costs, which in a large city would form part of public investment. Our taxation system does nothing to correct any bias in profitability estimates as between companies situated in small towns and big cities.

The main point, however, is the varying difference between the estimated profitability of private investments taken as an average for branches of activity and the calculated social marginal return on the corresponding investments when account is taken of the need for complementary public investments. And exactly the same consideration comes in when studying the interdependence of investments within a firm and between branches of activity. One way out of these complications, which is really a retreat from the difficulties raised, is to aggregate investment and returns for the whole economy and look at the results *ex-post*. We are then back at the aggregate capital theories that I discussed earlier.

DISEQUILIBRIUM POSITIONS AND PROFITABILITY

My theme is, in fact, that if we wish to understand the dynamic processes resulting in the aggregates we have to pay attention to the wide dispersion in the expected profitability of investments at the various points and sectors of the economy. From this point of view, investment in capital goods normally occurs in partial disequilibrium situations, characterised as promising investment opportunities. The essential characteristics of these positions are determined by the possibilities of introducing new methods of production, new types of goods, the creation of new markets, replacing obsolescent machines by new and more efficient ones and so on. The relatively high profitability is to be explained as a result of partial disequilibrium. It may be assumed that equilibrium positions are in some way approached at the various points in the economy as a consequence of the increased production and productivity following the investments, but new disequilibrium positions are continually created, partly as a consequence of this very flow of investments. New demands arise in lagging private and public sectors of the economy as well as within the individual firms, and obsolescence of old capital is created. The flow of new technical knowledge being made applicable for use in production is more or less continually creating new disequilibrium positions and investment opportunities. This Schumpeterian way of looking at the process of economic development gives, of course, only a limited aspect of the conditions of capital formation—certainly very far from a complete model. There are a number of other conditions, relating, for example, to total demand and employment that have to be considered in a complete system of explanation. But for the moment I am concerned only with one element in the model.

I come now to the income-redistribution complication of capital formation that is in fact related to the interdependence problem. I have constantly met this problem in interviews on the micro level. The question arises about the price and cost estimates and the assumptions about future development that are built into or implied in the profitability estimates. Should the estimate be based on the price and wage cost relations ruling at the time of the investment decision, or should the estimated return include a

forecast of the probable changes during the economic life of the investment in question? I have come across very different attitudes to this question, partly depending on the purpose of the profitability estimate. If the purpose is one of ranking potential investment projects of similar type as to profitability at a given time, then it does not matter so much if to-day's or some future price—and wage—relations are used. The relative positions of the projects will usually not be much affected. When, however, the purpose is also to use the estimate of the returns on investment for forecasting the future financial positions of the firm, attention has, of course, to be paid to the probable trends of wages and prices.

I can again take as an illustration the profitability estimates of the investment programme of the iron and steel plants mentioned earlier. On the basis of wage costs and prices ruling in 1955, the gross profits before tax would in 1963, when the new capacity is expected to be in full production, have increased by about 40 million Swedish crowns. When, however, account had been taken of an expected rise in wages per hour, in relation to steel prices, of, on the average, 3% per year—this being the trend observed for the past years—the increase in gross profits would be reduced to 15 million crowns. This is 60% less than the hypothetical amount at given price and cost relations. Under reasonable assumptions as to the length of the investment period and the economic life of the new plant the internal rate of net return would be nearly 30% in the first case and only about 10% in the second case, when account is taken of the changed wage-price relation. If wages continue to rise in the same way during the whole expected lifetime of the investments the internal yield will ultimately be reduced to 1–2%. This is, of course, a very unrealistic example, being an *ex-ante* calculation of the *ex-post* profitability results under given productivity development. Unexpected golden investment opportunities may improve the profitability results and make the second alternative the more realistic. But you will notice the unavoidable arbitrariness in the methods of forecasting total results, when account has to be taken of the interdependence of yields of the firm's investments over time as well as of the impact on costs and revenues of general economic development.

These are problems which are discussed on a very high level of abstraction in Mrs. Robinson's and Mr. Champernowne's theories. The rise of wage costs in relation to capital costs during the course of development and the corresponding redistribution of incomes and the process of revaluation of capital give rise to serious and difficult problems in capital theory, especially when they are handled with the old tools of analysis. In terms of the division of labour that I have suggested, my limited task is to enlarge the inventory of different aspects of profitability in the light of my contacts with reality.

There are clearly other types of changes of income distribution than those that I have just mentioned which can be foreseen and built into a profitability estimate. Instead of wage earners directly absorbing the

benefits of the productivity increases consequent on investment, consumers in general may receive the benefits by way of price competition. Investments in new technical processes or in the production of new commodities create quasi-monopoly positions, promising high returns in the short-run before other producers enter the field. I only need to remind you here of how intimately profitability estimates of investment, both *ex-ante* and *ex-post*, are related to market structure, price policy and wage formation. In this connection, I have to ask how anticipations of market reactions enter into profitability estimates and how they should enter if they are to throw light on the marginal efficiency of investment.

I quite often find that these problems are in some degree taken into account in the sense that firms anticipate about the same gross profit margins on sales as the average that has been ruling during past years. This implies that entrepreneurs expect that exceptional investment opportunities, promising high profitability, will not usually last very long. Increased wages, lower prices and other forms of competition will sooner or later eliminate exceptional gains. And for the longer run there are also expectations that, with costs of capital and labour about the same for their competitors, lasting deviations from the anticipated relations will be impossible. From another point of view, this prospect of future changes in price-wage relations will be interpreted to imply bankruptcy in the longer run for a firm that fails to make investments to secure higher productivity. Some kind of alternative calculation showing the difference in net returns between making investments to secure higher productivity and failing to make such investments will again result in the same kind of high profitability that emerged under the assumption of given price-wage relations.

What I am trying to say here is closely related to the problem of *ex-post* profitability that I discussed earlier from an aggregate point of view. The iron and steel plant to which I have been referring as a micro model made a calculation of the costs or reproducing the whole plant at the 1957 price and cost level. The result was a capital coefficient of 5.5, in fact very close to *ex-post* estimates for the iron and steel industry as a whole, built on fire-insurance values. In the calculation the higher efficiency of a newly built capital structure was eliminated by assuming such a rise in wage costs that gross profits remained at a conservative level of 15% of the value of production (it is only a little more than 10% to-day). The result was a rate of yield on capital of only 3%. With a reasonable capital structure, a long-term interest rate of 5% and some normal consolidation after taxes at 57% on net profits, the corporation would be able to pay 1.3% yield on its share capital. I have examples of similar tendencies for other corporations interviewed.

SUMMARY

Let me now summarise the lines of thought that I have been illustrating. There is a series of alternate investment profitability concepts of varying

significance: first, profitability estimates of an investment in an expansion programme at given prices and wages; secondly, the same after correction for an autonomous rise in productivity, supposed to be possible without investment; thirdly, profitability of an investment with due regard paid to an expected rise in wages in relation to prices; fourthly, some kind of measure of *ex-post* profitability of the total reproduction cost of capital that is invested in a firm.

The fourth definition, usually giving a very low yield, is sometimes used in economic policy discussions to show that Swedish industry, at prevailing tax rates, labour costs and the rest, is doomed to ruin. After paying taxes and dividends, industry cannot afford even to replace worn-out capital; this is the tragic conclusion of this type of argument. And yet these entrepreneurs sitting at their desks are quite different people from what they are when they are putting their case in the political debate. And quite rightly so. The *ex-ante* expectations of profitability are often very high, as shown in the interviews. This is so partly because the investments are marginal in various ways and degrees. There is always some old capital waiting to be put to better use inside the firm, and this is also the case in relation to the public sector. This is the interdependence factor. Moreover, the income redistribution effect often seems to be excluded or to be underestimated. Is it not an essential part of the vitality and attractiveness of the capitalistic system that *ex-ante* profitability of investments is expected to be high and promising, while *ex-post* returns, when looking back at past capital costs, seem to be sadly low?

My conclusion from these scattered observations is that interview studies of the type on which I am engaged certainly do not bring much order into the chaos of direct observation. The one good thing about this kind of survey is that it may upset some of the over-simplified assumptions underlying our usual models of growth. In my view, marginal efficiency of investment should be closely related to the *ex-ante* profitability of investment at the various points in the economy and should be determined at given prices and costs. At these points, you may argue, there are micro-elements of Mr. Kaldor's technical progress function. The *ex-post* returns on reproduction costs of capital should not be taken as given parameters in the system of explanation as in the Douglas approach. But obviously the level and changes of *ex-post* profitability of capital, however defined, has to be explained. And one of the factors of explanation should be the flow of investment opportunities with expected high returns in private industry, with a given rate of expansion of the public sector. It may be, as the survey results suggest, that the first measure, that of *ex-ante* profitability in private industry, is often several times higher than the second, the *ex-post* results on an average at the micro points in private industry. But we also have a picture of great dispersion, and when analysing this, account must be taken of the interdependence complications, especially with regard to investment in the public sector. The need for disaggregation follows as a consequence, first and

foremost as between private and public sectors. Within the private sector there should not be complete chaos as to the level and dispersion of profitability expectations.

The third issue I have raised refers to costs of capital. There are capital and credit markets functioning and bringing about some kind of order which have to be studied. The rules of thumb, the rejection rates set up when scrutinising investment projects, the standards of profitability regarded as reasonable in the various firms and branches of activity are not arbitrary and independent of market conditions. The rates of interest on the credit and capital markets, the yields on stocks and the tax rates on net profits have an influence on the rates of net return demanded. In interviews, managers often refer to the costs of new capital as lying between 15 and 20%, and this view apparently determined directly or indirectly the responses to our question of expected returns.

But surely there is hardly a more imperfect market than the capital market. There was a particularly tight rationing of bank credit during the years in question. There are always many more or less water-tight compartments, not least because corporations refuse the market test by retaining their savings. Allocations of savings are distorted in a number of ways, including taxation, weakening any equilibrating mechanism that may exist.

From this point of view, and in the light of the wide dispersion of profitability rates expected from investments being currently made, one conclusion must be this: that more effectively functioning credit and capital markets, with less rationing, more mobility and higher flexibility of capital costs, should make possible a higher rate of increase in productivity.

I am back in the old Swedish habit of facile generalisation and policy recommendation based on very insufficient analysis and factual research. But what can an impatient economist do? I have, on the one hand, been critical of existing models of growth, because of the methods used in treating the essential problems of investment profitability. These methods largely consist in assuming away just those problems that I consider most important if answers are to be given to my economic-policy questions. On the other hand, I have not produced a clear alternative line of approach as a result of my examination of the problems of investment profitability.

I have not succeeded in synthesising all the various aspects of profitability problems that I have identified, but perhaps I have succeeded in giving some reasons why profitability estimates made by experts are interesting as indicators of the level of marginal efficiency of investments, and why return estimates are and must be so difficult to compare. I hope that I have also led you to the conclusion that there is much field work still to be done. If I have at the same time succeeded in disturbing and at best in provoking to new activities some of the skilful model builders among you, some good results may emerge in the longer run.

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THE NEW THEORIES OF THE CONSUMPTION FUNCTION

I. INTRODUCTION

THERE have recently been put forward—notably by Friedman (1957a) and Modigliani and Brumberg (1953 and 1954)¹—new theories of the consumption function which have startling implications for a wide range of economic problems. It is my belief that the acceptance of these theories has been prejudiced, primarily by the conjunction of valuable and highly controversial hypotheses, and secondarily by certain flaws in exposition. I have, therefore, attempted an exposition of the essentials of these theories which, I hope, avoids these disadvantages.²

I distinguish sharply the different hypotheses involved, and consider separately the evidence for, and implications of, each of them. I must apologise to the reader for introducing a new terminology into a subject already overburdened with jargon. My excuse is that I felt it was essential.

II. NOTES ON DEFINITIONS

Initial assets are the individual's net worth at the beginning of the year. A point of minor interest is that, since this includes his holdings of consumer's durables, we are imputing to the individual as income the interest cost of these holdings (except during the current year).

Current income is the total of the individual's receipts (net of tax) during the year—including "earnings" in our sense and the interest yield of assets. The important point here is that contributions by employer or state to schemes for superannuation, health or unemployment insurance are clearly a part of the individual's income and must be included in computations of it.

Earnings differ from income in that (to avoid double-counting) they exclude the interest yield of assets.

Consumption would be perfectly straightforward were it not for the existence of consumers' durables. It has long been recognised (Hicks (1938), p. 176) in economic theory that purchases of consumers' durables are of the nature

of capital expenditure, whereas the consumption of durables is the appropriate current cost—that is, depreciation plus interest costs. In the early work on the consumption function this was regarded (if at all) as a theoretical nicety, inappropriate to empirical work, and a great deal of the available empirical evidence is based on the assumption that purchases and consumption are equivalent, but in recent years the importance of the distinction has become quite widely appreciated, and not merely among the New Theorists.¹

Savings are simply the accounting difference between current income and consumption. It should be noted that, as a result of their status as a residual, savings are particularly sensitive to any inaccuracies in the measurement of either income or consumption. In particular, any failure to impute employers' and state contributions to the individual's income is likely to produce a spectacular underestimate of his saving.

We may perhaps add that in *defining* savings as a residual we are not implying anything at all about the motives which lead a consumer to save.

III. THE NORMAL INCOME HYPOTHESIS

The basis of the new theories is the recognition that, if an individual plans rationally to maximise his utility over his lifetime, his consumption in any given year will depend, not on his income in that year, but on the resources of which he disposes during his lifetime. This is plausible but vague; we must first dispel the vagueness.

If an individual knows with certainty his future stream of earnings,² and faces a perfect capital market with a given rate of interest, it is possible to represent "the resources of which he disposes . . ." by the current value v of his current assets plus his expected future earnings, discounted at the rate of interest. The theory could be fully developed in terms of v , but it will assist the exposition if we convert v into an equivalent income stream. Consider a constant annual Y for the remainder of the individual's life-span; if Y is such that the current value of this income-stream is just equal to v , we shall call Y the individual's *normal income*.³

It is easy to show that if such an individual also knows his future tastes and the future course of prices, and plans his consumption so as to maximise

¹ For example, Morgan (1951), Goldsmith (1951), Boulding (1950).

² "Earnings" is slightly misleading, as we define it to include all receipts except the interest yield of assets. But the obvious alternatives—"income" or "receipts"—would be at least equally misleading.

We must also note that in point of pure theory, earnings are determined by the individual in his maximising calculations, and what is known is the function relating earnings to work done. But this point is not important in the present context.

³ It should be noted that, given the rate of interest and the life-span, normal income is uniquely determined by v . In this (as well as other ways) it differs from Friedman's "permanent income," which seems a rather flexible concept. We shall argue later that the relevant life-span is not the actuarial expectation of life, but a slightly longer period, not directly measurable. This introduces a (relatively very small) element of flexibility into our concept.

¹ The dates are misleading, as the theories were circulated in mimeograph and widely discussed as early as 1953. They had been partly anticipated by Harrod (1948) and Vickrey (1947), but these writers were interested primarily in other problems. The present paper owes a great deal to all three writers, and particularly to personal discussions with them during 1953–54. It has also benefited from the criticisms of Mr. J. S. Cramer and Dr. M. R. Fisher.

² The main ways in which my exposition differs from the original theories are as follows. First, I omit the utility theory, which is straightforward mathematics, and not really very illuminating, since uncertainty has to be introduced *after* the results have been derived. Secondly, I avoid Friedman's concept of "permanent income," which, although in some ways very attractive, involves considerable and unnecessary difficulties. In their treatment of the aggregate consumption function the New Theorists diverge sharply, and here I follow Modigliani and Brumberg.

his satisfaction over his lifetime, his planned consumption in each year will be uniquely determined by his normal income Y .

There is not, in practice, a perfect capital market, and this raises a number of theoretical difficulties—notably that it is not clear at what rate to discount future earnings. But these are complications that can easily be dealt with—the main practical difficulty lies in the penal interest rates attached to borrowing that reduces the individual's net worth below a certain level. This means that the above conclusion could not be expected to hold for income-and-consumption patterns that lead temporarily to excessive indebtedness: that is, for income streams that are, in relation to the optimal consumption pattern, unduly deferred. This qualification would probably not be very important, were it not for the problem of durable consumption goods, to which we shall return later.

The fact that we live in an uncertain world is a source of much greater difficulties. Rational behaviour in the face of uncertainty is a problem that has not yet been solved even in simple cases—it would be foolish to suppose that our simple theorem held precisely for so vast a problem as the individual's lifetime consumption plan. But in fact we need only a much more limited result, and one which is quite defensible. Let us now state

The Normal Income Hypothesis: in any given period, an individual's current income y affects his consumption c only through its effect on his normal income Y . We may write $c = \beta(Y)$, where β is independent of current income and assets.

We are only concerned with consumption in the initial period (say, one year) of the plan, and it is reasonable to suppose that the individual's expectations remain stable long enough for him to carry out that part of the plan. Uncertainty about the future will undoubtedly affect his planned consumption in many and complex ways, but we must now consider whether it is likely to give current income any direct influence on current consumption.

One way in which this might happen is that uncertainty about the future might lead people to spend every penny they could lay hands on. Some people probably do this, but not those who have positive savings or assets. This effect seems to be confined to a small minority, and to be unimportant in the community as a whole.

A second possibility is that people's expectations of future earnings should be directly and stably related to their current earnings. This again may be true for a minority, but seems unlikely to be important in the aggregate. More likely is it that, although such a relationship does not hold for individuals, it does hold in the aggregate as a result of averaging out of deviations. This seems quite possible in any particular case, but one would doubt very much the *stability* of such a relationship.

The most important possibility is that uncertainty may lead people to

abandon maximising calculations in favour of conventional rules of the Mrs. Beeton type. This is not unlikely—such rules are important in business behaviour and might be expected to be at least as popular with private individuals. Moreover, they are not necessarily inconsistent with rational behaviour; detailed planning may itself have a disutility, and a conventional rule that approximated the optimal spending pattern might well be most satisfactory to the individual. But just as the business-man adheres to his conventional prices only so long as they are roughly optimal, so we might expect consumers to stick to their conventional saving rule only so long as it gave roughly the right answer. For example, we might expect to find many of those with stable incomes following conventional rules, but we would be surprised if people with very variable incomes did so.

To sum up, for people with variable incomes, the *Normal Income Hypothesis* is roughly equivalent to postulating rational behaviour; but people habituated to stable incomes could quite rationally follow some such rule as “always save $\frac{1}{n}$ th of your income”—which would quite invalidate the hypothesis.

IV. THE PROPORTIONALITY HYPOTHESIS

The second hypothesis of the new theories concerns the shape of the function β . They postulate that it shall be a straight line passing through the origin. Formally, this is

The Proportionality Hypothesis: for any individual, the relationship between his consumption and his normal income is one of proportionality.

This was, I think, originally regarded by the New Theorists as purely a working hypothesis, but they may have given the impression that it is an integral part of their theories. At any rate, the attacks on the new theories have centred on this hypothesis.¹ I shall argue, first, that it is unnecessary, and secondly, that it is of doubtful validity.

It is unnecessary because, first and patently, the *Normal Income Hypothesis* is independent of it. More important, we shall see later that the *evidence* for the *Normal Income Hypothesis* is independent of the validity of the *Proportionality Hypothesis*.

I think it is a fair comment on the *a priori* arguments put forward by the New Theorists² for the *Proportionality Hypothesis* that they were never designed to do more than establish its plausibility as a working hypothesis; and certainly that they do no more than this. It is also, I think, fair to say (and I shall in part substantiate this comment in a later section) that neither Friedman (1957a) nor Fisher (1956) found any good evidence for the

¹ For example, Friend and Kravis (1957a), Klein and Leviatan (1957).

² Modigliani and Brumberg (1954), Part I, Friedman (1957a), Chapter II.

hypothesis. In contrast, Friend and Kravis (1957a) produce a cogent *a priori* argument as to why the proportional relationship should not hold exactly, although it might be argued that the deviations in question affect only the very rich, and are therefore negligible in the aggregate. They also produce (in Table I) the only piece of (more or less) direct evidence that I have yet found. It concerns a sample of families who had had approximately the same income in the previous year and expected much the same income in the subsequent year. The regression of consumption on current income appears to be roughly

$$c = 900 + 0.68y$$

There are four qualifications to be made. First, consumption is defined to include purchases of durables; but a similar relation holds for non-durable consumption, and there seems no reason why the case should be different for consumption correctly defined. Secondly, income is defined to exclude the value of employers' and state contributions to schemes for superannuation, health and unemployment insurance. The inclusion of these items would undoubtedly make the results more favourable to the *Proportionality Hypothesis*; but they would have to amount to over \$1,300 per family to remove the whole discrepancy. Thirdly, the constancy of current income over three years does not imply logically that normal income must, in the sample, vary proportionately with current income; but I can think of no convincing reason why there should be a substantial and systematic deviation in the requisite direction, except for the possibility that the lower-income groups contain a higher proportion of retired people. Here we must rely on a remark by Friend and Kravis (p. 544) that "when the self-employed and not gainfully employed are eliminated . . . the results are only moderately improved from the viewpoint of the permanent income theories." Fourthly, it is possible that each individual's consumption varies proportionately with his income, but that the factor of proportionality is correlated, within the sample, with income. For instance, intelligent and responsible people might tend to have both higher propensities to save and higher incomes than the rest of the population. Such a correlation would, however, make the *Proportionality Hypothesis* useless for analysing cross-section data.

In the circumstances, and particularly in view of the hostility provoked by the *Proportionality Hypothesis*, it seems wise to keep an open mind about the shape of the function β .

V. THE SHORT-PERIOD MARGINAL PROPENSITY TO CONSUME

On the *Normal Income Hypothesis* the individual's consumption function is $\beta(Y)$ and his marginal propensity to consume out of normal income simply the slope β' at the appropriate point. We cannot, of course, say anything *a priori* about the magnitude of any individual β' , but on the average we would

expect β' to be equal to, or a little less than, unity.¹ But if we wish to calculate the marginal propensity to consume out of current income, we have also to analyse the response of normal income to a change in current income. Formally,

$$\frac{dc}{dy} = \beta' \frac{dY}{dy}.$$

We must now separate the direct and indirect effects on Y of a change in y . The direct effect of an increment Δy in y can be shown to be an increment in Y of the order of $\frac{2}{N}\Delta y$, where N is the life-span used in the individual's plan.²

The indirect effect depends on his expectations. Suppose an increase Δy in current income leads him to expect an increase $\mu\Delta y$ in his earnings for n years; then it will produce an increase in normal income of approximately $\frac{n}{N}\mu\Delta y$.

Thus, adding the two effects,

$$\begin{aligned} \frac{dc}{dy} &\simeq \beta' \left(\frac{2}{N} + \frac{n}{N}\mu \right) \\ &\simeq \frac{2}{N} + \frac{n}{N}\mu \end{aligned}$$

Except for those nearing the retiring age, the first term will be small compared with the second, and the individual's marginal propensity to consume out of current income will be dominated by his elasticity of expectations. Table I gives a few illustrative examples, assuming that the individual bases his plans on retiring at 65 and living to 85.

The short-period aggregate marginal propensity to consume is simply a weighted average of all the individual marginal propensities in the community—weighted, of course, by the increments of income received. The individual marginal propensities will vary not only with age, as shown in the table, but also with family responsibilities, social class, and so on. But speaking very roughly, and ignoring possible correlations between these factors and the elasticity of expectations, we may regard the aggregate

¹ If all saving is consumption-spreading and the rate of interest is zero, the individual will plan to spend at some time or other any increment in his normal income. Hence the β 's will average to unity over his life-plan, and thus, too, they will average to unity if the averaging is performed over a "representative" group of individuals. The fact that individuals usually die before the completion of their life-plan (see footnote ² below) will tend to lower β .

Saving-for-its-own-sake would tend to lower β' , but probably not very much in the aggregate; the effects of a positive rate of interest are difficult to determine (see Graaff (1951)), but are likely to be relatively small.

² Consider an income stream ΔY for N years. Its present value, discounted at 5% per annum, falls from $6\Delta Y$ at $N = 10$ to $10\Delta Y$ at $N = 40$, so that $\Delta y \simeq \frac{1}{3}N\Delta Y$.

Since he is uncertain about how long he will live, and since great disutility attaches to exhausting one's assets before one's death, the individual will probably "play safe" by basing his plans on a life-span N considerably greater than his actual expectation of life.

marginal propensity as proportional to the weighted average of individual elasticities of expectations, the factor of proportionality being of the order of 0.5 or 0.6.

This short-period aggregate marginal propensity to consume is, of course, a relationship between hypothetical variations in income and the consequent changes in consumption, for a given group of people in a given year. It is

TABLE I

Age \ μ	0	1	2
25	0.03	0.70	1.37
35	0.04	0.64	1.24
45	0.05	0.55	1.05
55	0.07	0.40	0.73

not strictly applicable to year-to-year changes in income and consumption, as both the composition of the community and the individual β 's will, in general, change from year to year. However, in the relatively short period the composition of the community will remain fairly constant and the changes in the β 's may be assumed roughly to average out. Another complication is that the level of assets varies in time-series observations, but this can be neglected in the present rough calculations. (The marginal propensity to consume out of assets is of the order of $\frac{2}{N}$ —roughly 0.1, allowing for weighting the individual marginal propensities by asset ownership, which is positively correlated with age.)

As the elasticity of expectations is not directly measurable, observations of the cyclical marginal propensity cannot be expected to provide any very satisfactory evidence for or against the hypothesis. However, the readily available data seem to imply an *average* (over time and the community) elasticity of about unity.¹ This cuts both ways. In so far as unity is a plausible value, the data are consistent with the *Normal Income Hypothesis*; but unit elasticity of expectations also suggests the possibility of a simple relationship between y and c , which would make the hypothesis redundant.

It should be emphasised that this section relates only to the *marginal* propensity to consume. The average propensity involves the complicating factors discussed in the next section.

VI. THE LONG-PERIOD CONSUMPTION FUNCTION

It is very tempting to argue that the long-period aggregate marginal propensity to consume is derived from the individual marginal propensities

¹ Friedman (1957a), Table 12. Only lines 6–14 are based on a correct treatment of consumers' durables, and of these only lines 6–9 are for reasonably short periods. These give marginal propensities of 0.72, 0.65, 0.60 and 0.45.

in the same way as the short-period one, save that in the long-period the elasticity of expectations will be greater, thus giving a greater marginal propensity. This is indeed the essence of much of Friedman's treatment of the time-series evidence (Friedman (1957a), Chapter V), but it is, unfortunately, illegitimate. The difficulty is that the composition of the community cannot be assumed constant in the long period; and further, that the changes in composition found in real life have a considerable effect on the consumption function.

If all saving is for the purpose of consumption-spreading, and if there is no certainty, each individual will, over his lifetime, exactly consume his total income—that is, his savings would sum to zero over his lifetime. In a community which was in all relevant respects stationary, this would imply that individual savings summed over the community would be zero in any year. On these assumptions, positive savings in an economy would be evidence that it was *not* stationary. If dis-saving is principally done after retirement, there are two obvious forms of change that can lead to positive aggregate savings.

First, a growing population will, in general, mean a disproportionately small number of retired people, and hence of dis-savers. Secondly, a rising level of real income per head will mean that the savers will, on the average, have larger lifetime incomes than the dis-savers. Modigliani and Brumberg (1953), in a most striking piece of research, investigated these effects in detail. They found that, on a number of simplifying assumptions, for rates of growth up to 5% per annum, each 1% per annum of growth in either real income per head or population would lead to 3–4% of aggregate income being saved. Thus the saving-ratio is proportional to the rate-of-growth of aggregate real income, and is independent of how this growth is compounded of changes in population and in real income per head (so long as both change steadily). This suggests

The Rate-of-growth Hypothesis: In long-run equilibrium, aggregate saving is determined by changes in population structure and in real income per head. If these factors change steadily, the fraction of aggregate income saved is proportional to the rate of growth of aggregate real income.

The simplifying assumptions include a zero rate of interest, consumption spread evenly over the life-span and no bequests. There seems to be no reason why the calculations should be very sensitive to a positive rate of interest, or to plausible variations in the consumption pattern—so long as lifetime consumption equals lifetime income. But the assumption of no bequests seems crucial—and palpably false.

However, the hypothesis would still be valid if heirs planned to dis-save (the bulk of) their inheritances over their lifetime. They would do this on the *Normal Income Hypothesis* provided that saving-for-its-own-sake or for

posterity was small; but they would also do so if they were "improvident," i.e., deviated from the life-plan in the direction of shorter-run maximisation of utility. They would fail to dis-save adequately if for some reason they wished to maintain or increase their assets, or if they adopted some conventional rule (such as "never spend out of capital") which led them to do so. Such behaviour we may call "thrifty." The building-up of family fortunes over several generations is evidence of thrift, but the nineteenth-century saying, "From clogs to clogs in three generations," shows that even in the golden age of thrift, dis-saving by heirs was not uncommon.

It is obviously important to find out whether thrift generates a significant volume of saving, and Goldsmith (1955) gives estimates¹ of asset holdings by different age-groups which permit us to make a rough test. If the population is assumed to decline linearly after the age of 65 and if individuals are supposed to plan a linear running down of their assets over the same number of years, it is easy to show that average assets in the over-65 age-group will be $\frac{2}{3}$ of those at 65, assuming a history of stationary prices and incomes.

Goldsmith's Table XLII, Col. 1, shows observed net worth by age-groups, and estimating the 65 value by extrapolation, we get a ratio of 0.73. Goldsmith suggests (Vol. I, p. 217 n.) that this figure is too high, owing to the oldest age-group's having had a particularly favourable history of capital gains. In Col. 3 of the same table, Goldsmith gives estimates of accumulated life saving which yield a ratio of 0.52. (He regards these estimates as giving "the least distorted picture of life saving curves that can be fashioned out of the rough over-all data now available.") Unfortunately, the asset figures underlying both estimates exclude consumers' durables other than motor cars, and so probably give a downward bias; but this should not be large.

These calculations suggest that the population as a whole is not thrifty.² However, figures based on estate-tax data (Vol. III, Table E-29) give ratios of approximately unity, which are presumably underestimates, as no allowance is made for gifts *inter vivos*. Thus, thrift is important among those subject to estate tax, and its absence from the aggregate results is due to the cancelling out of thrift among the wealthy and improvidence among the poor.

The direct evidence of Modigliani and Brumberg's calculations leads to much the same conclusion. Saving 12% of aggregate income would, on the *Rate-of-growth Hypothesis*, correspond to aggregate real income growing at 3-4% per annum. Thus the saving that would be generated by growth on the hypothesis is of approximately the same amount as that observed; but this could easily conceal substantial cancelling of errors, and in particular

¹ Vol. I, pp. 214-25; Vol. III, pp. 102-35 and 284-381.

² The data are approximate, and the theoretical figure of $\frac{2}{3}$ depends on linearity assumptions that may well not hold exactly. We must therefore allow for sizeable possible errors. Comparing an observed 0.52 with a theoretical 0.67, it seems reasonable to rule out the possibility that the observed value is really greater than the theoretical, but it would be risky to make any more precise inference.

the balancing of thrift among the wealthy against improvidence among the poor.

We may conclude that the *Rate-of-growth Hypothesis* gives a good explanation of the long-period consumption function of the United States, but that this success depends partly on a balance of thrift and improvidence in different normal income groups. In applying the hypothesis to other economies, we must be prepared for the possibility that the balance will not hold there so that actual saving will differ from that predicted by the hypothesis. But however much thrift or improvidence may distort the picture, the rate of growth will remain the basic determinant of the aggregate savings/income ratio in the long run.

It is perhaps worth noting that the evidence considered in this section is quite unrelated to the *Normal Income Hypothesis*. Nor do Modigliani and Brumberg's calculations throw any light on the *Proportionality Hypothesis*—it is virtually impossible to make inferences from the observed long-run aggregate consumption function about the shape of the individual consumption functions. On the other hand, Goldsmith's data constitute some evidence against the *Proportionality Hypothesis*.

VII. THE FRIEDMAN EFFECT

If consumption depends on normal income, the interpretation of a sample survey which gives observations of consumption and current income must depend on the relationship in that sample between current and normal income. Friedman (1957a, Chapter III) and Modigliani and Brumberg (1954, Part II) give *a priori* arguments that there will usually be a particular relationship between these quantities. We shall now attempt a brief summary of this argument. The brevity is achieved by sacrificing some detail and rigour, and also (I regret to say) by introducing a new notation.

Suppose that for a sample of families the elasticity of consumption with respect to normal income is E , and that the apparent elasticity with respect to current income, computed from survey data, is G . Suppose, too, that we can measure the relationship between normal income Y and current income y by the elasticity F of Y with respect to y . Then we can see at the intuitive level that $G = EF$, so that, for instance, if Y increases proportionately with y (that is, if $F = 1$) G is an estimate of E .

It is argued by the New Theorists that F is usually less than unity. Suppose that for the families in question year-to-year fluctuations in current income which do not affect normal income are important. Suppose further that in the particular period to which the survey refers the magnitude and direction of these fluctuations is not correlated with normal income. (This would clearly be true if we were considering the average over a number of years, but any particular year might, for example, be a good one for the rich and a bad one for the poor.) Then, abstracting from possible complications through the correlation of normal income with age, the magnitude and

direction of the temporary fluctuations will be positively correlated with current income. In other words, in the higher current income groups a larger proportion will be enjoying a "good year." This implies that Y increases less than proportionately with y , and thus that $F < 1$.

We shall find it convenient to call this phenomenon, of $F < 1$, the "Friedman Effect," and to speak of a smaller value of F as a larger Friedman Effect. The argument for the Friedman Effect is plausible, but equally it is clear that it may apply to some societies or social groups and not to others. As normal income is not directly measurable, we can make no direct tests, but there is a certain amount of indirect evidence.

Table II, for example, shows for various American occupation groups the

TABLE II

	A	B	C
Professional	1-5	61	60
	6-8	52	70
	9-10	69	53
Self-employed or managerial	1-5	23	35
	6-8	38	51
	9-10	50	50
Clerical and sales	1-3	65	68
	4-5	60	59
	6-8	62	58
Skilled or semi-skilled	9-10	61	63
	1-3	40	50
	4-5	48	49
Unskilled	6-8	63	54
	9-10	68	63
	1-3	38	47
Farm operator	4-5	43	53
	6-10	55	68
	1-3	26	40
	4-5	47	48
	6-10	58	53

A. Income group.

B. Percentage of those in that income group for 1947 who had an (appreciably) larger income than in 1946.

C. Percentage of those in that income group for 1948 who had an (appreciably) larger income than in 1947.

Data abstracted from Katona and Fisher (1951), Table 8.

percentage of families in each current income group who were better off than they had been in the previous year. Where the Friedman Effect is present, this percentage will increase with income. Such a tendency is well marked among farm operators, manual workers and the self-employed and managerial groups. There is clearly no such effect in the "clerical and sales" group, while the professional group shows unsystematic movements.

Some similar figures for Dutch families are given in Table III. The occupation groups are *handarbeiders* (manual workers) and *hoofdarbeiders* (roughly, clerical workers). For each occupation the average current income of each current-income group is compared with its average income

over the previous four years. If the latter is taken as a measure of normal income the manual workers display the Friedman Effect while the clerical workers do not.

TABLE III

Income group Fl. per annum	<i>Handarbeiders</i>			<i>Hoofdarbeiders</i>		
	No. of families	Average current income	Average previous income	No. of families	Average current income	Average previous income
<1,600	72	1,144	1,183	34	1,136	1,110
1,600-2,000	33	1,740	1,623	21	1,745	1,672
2,000-2,600	18	2,215	2,073	36	2,287	2,151
2,600-3,400	8	2,950	2,460	31	2,983	2,739
3,400-4,500	1	4,140	3,855	22	3,847	3,503
4,500-6,700	1	4,832	4,111	12	5,094	5,034
>6,700	—	—	—	21	22,489	26,485

Taken from: "Resultaten van een inventarisonderzoek in de Gemeente Rotterdam, ingesteld door de Schade Enquête Commissie aldaar, in 1941" in *Statistische en Econometrische Onderzoekingen*, Maandschrift van het Centraal Bureau v.d. Statistiek 1943, nr. 4.

Mr. J. S. Cramer comments: "Not a very good sample, 'selected' by asking firms to ask for volunteers from among their employees." He quotes, "In several cases the current income could be ascertained with more accuracy than the 5 year average."

This evidence, limited though it is, is enough to confirm our impression that the Friedman Effect may well be present in samples from some occupation groups—such as farmers, business-men and manual workers—and missing from other samples, such as professional men and salaried workers.

VIII. THE EVIDENCE FROM THE FRIEDMAN EFFECT

Friedman's analysis of the cross-section evidence for his theory (Friedman (1957a), Chapters IV, VI and VII) is a most brilliant and fascinating economic argument, and occupies 132 pages. The present short discussion cannot do justice to the rich detail of his analysis, but it will, I hope, serve to indicate both the strengths and the weaknesses of his arguments.

His analysis of the survey data is based on three assumptions:

- that $F < 1$, i.e., that the Friedman Effect is present;
- the *Normal Income Hypothesis*;
- that $E = 1$, i.e., the *Proportionality Hypothesis*.

He finds almost all the evidence he considers consistent with this set of assumptions. Unfortunately, since direct measurement of F is impossible, it is impossible to estimate accurately the value of F for any particular sample, so that this "consistency" does not go very far towards confirming the hypotheses.

But although the absolute magnitude of F is difficult to estimate, there may be good grounds for believing that F will be smaller in one group than

another, and this will provide a test of the *Normal Income Hypothesis*. For suppose we have reason to believe that F is smaller in group A than in group B , and no reason to expect a significant difference in the values of E . Then we must expect to find a smaller G in group A on the *Normal Income Hypothesis*, and not otherwise. (Once again, the test is independent of the *Proportionality Hypothesis*.)

Friedman quotes a number of pieces of evidence of this type. In Table I, for example, he gives the values of G computed from a number of surveys in the United States, Britain and Sweden. One would expect that, of these three countries, year-to-year fluctuations would be most important in the United States and least so in Sweden. This accords with the values of G , which are systematically larger in Sweden than in the United States, with Britain in an intermediate position.

It is a commonplace of economics that farm incomes are particularly subject to year-to-year fluctuations, and much the same is true of the business community. Friedman shows (Tables 1, 3 and 4) that United States farm families exhibit smaller values of G than the rest of the community. In Table 5 he gives the results of a survey in which families are classified as "independent business," "farmers" and "others." The two former groups show values of G that are almost equal and much smaller than that of the "other." On the other hand, in Table 6 the G for independent businessmen is higher than for most other occupations; but Friedman attributes this to a faulty definition.

Fisher (1956) gives similar evidence from a British survey. He gives values of G (the figures called P , in columns (b) of Table 2.5) for four occupation groups—manual, clerical and sales, managerial and self-employed—broken down by age. If all ages are taken together, the self-employed have the lowest G , but this does not hold for all the individual age-groups. The number of observations in some of the cells is quite small, but even so, this unsystematic result may be a sign of the importance of factors that vary with age.

Perhaps a more important aspect of Fisher's results is that the manual workers have an unusually high G . In view of the low value of F in Table II above, this must suggest a breakdown of the *Normal Income Hypothesis*. Of course, the figures in Table II were for American workers, and their British counterparts may well have a much higher F ; but, as we shall see later, this in itself suggests a limitation of the hypothesis.

Friedman bases a further test on the argument that classifying a group of families according to the difference between their current income and that of the previous (or subsequent) year will increase F . The results he quotes in Tables 10 (farm families) and 11 (mainly urban) appear to show that such classification produces a definite reduction in G .

In those rare surveys that record the family's income for two consecutive years, it is possible to make rather more direct tests. Friedman develops a

method of estimating F for a group of families from the regression of their current on their previous year's income (or vice versa). I am not convinced that F is more closely related to these estimates than to the correlation coefficient of the two years' incomes, but it is clear that all four quantities are likely to be fairly highly correlated.

In Table 20 Friedman gives these statistical measures and values of G for three occupational groups—independent non-farm business, farm operators, and clerical and sales. Although one should not attach undue significance to a correlation based on three observations, it is noteworthy that the values of G (line 4) are virtually identical with those of the correlation coefficient (line 5).

A more substantial piece of evidence of the same sort is quoted by Friedman from an unpublished paper by Margaret Reid. In Fig. 15, Panel 1, he shows estimates of F , similar to those discussed in the previous paragraph, plotted against values of G for 27 groups of families. The correlation is most impressive, although it must be said that all the groups but one are farm families, and that it is not clear whether all the points plotted represent different groups, as opposed to the same group at different times. There is also the possibility that these account-keeping families might be unusually sophisticated in their economic thinking, and perhaps therefore obey the *Normal Income Hypothesis* where others would not.

In any test where one uses the previous year's income as a proxy for normal income, it is always possible that the effects one attributes to the *Normal Income Hypothesis* are really due to a simple lag in the adjustment of consumption to income. This is, of course, not so where the *subsequent* year's income is the proxy, but it is fortunate that Friedman provides a further piece of evidence on this point. In Fig. 15, Panel 2, he again plots estimates of F against G , but here the estimates use the *penultimate* year's income as a proxy. Most forms of adjustment lag would imply a substantially lower correlation for Panel 2 than Panel 1, while the *Normal Income Hypothesis* suggests a roughly equal correlation. On inspection there appears to be a slight reduction, which may be partly explained by the fact that these calculations were possible for only some of the groups in Panel 1.

Individually, each of the pieces of evidence described above lacks the conclusiveness of a direct test. But together, I feel, they constitute a fairly strong confirmation that the effects predicted by the *Normal Income Hypothesis* occur in real life. However, we must be careful not to overstate the case, for the fact that some people behave in a certain way does not mean that all do. In fact, if we look at the data with a view to determining what sort of people have gone into our tests, we find that the tests fall into three groups:

- (a) tests based solely on figures for farmers and/or business-men;
- (b) tests based on contrasting the behaviour of farmers and business-men with that of the rest of the community; and

(c) tests based on samples for the whole community, in which farmers and/or business-men may be sufficiently important to account for the results we found.

Thus the evidence shows only that farmers and business-men behave according to the *Normal Income Hypothesis*—it says nothing about the behaviour of the rest of the community. Now these two groups are those for whom year-to-year fluctuations in income are particularly important, and for whom therefore conventional saving rules are most obviously irrational. For a man with a secure job at a steady salary, on the other hand, a conventional rule might work very well. We must therefore be prepared to find that those with steady incomes do not obey the *Normal Income Hypothesis*.

We are not, then, surprised to find a negative result in the case of the British manual worker, habituated to over-full employment. The same might turn out to be true of clerical workers, who do not display the Friedman Effect either in Holland or the United States, but, on the other hand, American manual workers, a large group who show a substantial Friedman Effect, might prove to obey the hypothesis. However, this is conjecture. The fact is that there is strong evidence that the hypothesis holds for farmers and business-men, and little or no evidence on the rest of the community.

We may note finally that since all the tests depend on relative magnitudes of F , their validity is independent of the *Proportionality Hypothesis*; and in so far as we have not felt able to provide estimates of the absolute magnitude of F , we have been unable to test the *Proportionality Hypothesis*.

IX. SOME FURTHER CONSIDERATIONS

Mr. H. W. Watts (1958, pp. 126–32) has recently produced evidence of a rather different sort for the *Normal Income Hypothesis*. He argues that a measure of the expected future income of a young man is given by the “cross-section income profile” of men with the same occupation, education and so on. (The “profile” is obtained by plotting average income against age for suitably qualified members of a cross-section survey.) He compares groups of men with different educational backgrounds, but otherwise similar, and finds that, for a given current income, the better educated have higher expected incomes and higher current spending.

The possible effects of education on spending behaviour are complex. Watts argues that it would promote rational rather than improvident behaviour, and thus lead, *ceteris paribus*, to higher saving; but it would also promote rational rather than thrifty behaviour. In addition, it might be argued that education develops a taste for the good things of life—books, foreign travel, winter sports, wine and food, and so on—and that this is only partly offset by the gambling and the television sets of the uneducated. However, Mr. Watts’ results, so far as they go, favour the *Normal Income Hypothesis*.

“Contractual saving”¹—for example, saving in the form of life-assurance premiums or annuity or mortgage payments—is a common phenomenon nowadays and is likely to work against the *Normal Income Hypothesis*. It is likely to make it more difficult for a man to reduce his net saving as much as would be rational during a temporary fall in income. However, one suspects that its popularity is largely confined to people with stable incomes—those with variable incomes are likely to have any contractual saving well-cushioned with liquid assets.

We have said little about saving for posterity. My own belief is that, while many parents give generously to their children during their own lifetime (and while the children are relatively young), few would deliberately reduce their own consumption in order to make a *post-mortem* gift to children who would by then be middle-aged. Bequests are certainly made, but these would be due to uncertainty as to the life-span, or to the need to maintain an equity in utility-yielding assets. However, it is possible that some people save in order to leave money to their children, and we must ask how this would affect our various hypotheses.

The phenomenon can best be represented by making the functions $\beta(y)$ lower and less steep, on the average over a man’s life, than they would otherwise have been. This will not affect the *Normal Income Hypothesis*, but will constitute an additional reason for doubting the *Proportionality Hypothesis*. So far as the *Rate-of-growth Hypothesis* is concerned, it will be just another factor making for thriftiness.

I am indebted to Mr. N. Kaldor for pointing out that, whereas capital gains are assumed by the New Theories to be included in current income, they are not so included in most of the data we have used. The cross-section data are the most important, and it is easy to show that here the exclusion of capital gains leads to over-estimates of both the marginal and average propensities to consume. However, we have worked in terms of the income-elasticity of consumption, and the effect here depends on the relative magnitude of these two biases. In fact, if in any sample capital gains, on the average, increase more than proportionately with income—as I think we would expect—then the income-elasticity of consumption will be over-estimated. This tends to strengthen the evidence against the *Proportionality Hypothesis*, but its implications for the *Normal Income Hypothesis* are less clear. Our evidence there consisted in comparisons of income-elasticities for different groups, and would therefore be affected only by differential bias between groups—a subject on which *a priori* argument is rather hazardous, though one would expect such differential effects to be relatively small.

The omission of capital gains from aggregate time-series of personal disposable income will lead to an over-estimate of the short-period marginal propensity to consume if, as seems likely, capital gains are positively correlated with income. This means that the implied values for the elasticity

¹ See, for example, Klein (1954).

of expectations, towards the end of Section V, will be somewhat too high; but it is doubtful whether this will make an appreciable difference to such rough calculations.

X. CONCLUSIONS

We have broken the New Theories down into three independent hypotheses. Of these, we have left on one side the *Proportionality Hypothesis*; it is unnecessary to the theories, and such evidence as we have found is against rather than for it. On the other hand, we found the *Rate-of-growth Hypothesis* to be substantially valid. Certainly, in the long-run the level of aggregate net saving is determined by balancing gross saving against dis-saving, and is therefore sensitive to changes in the structure of the economy that affect this balance. But it is also determined by the balancing of thrift and improvidence, and although in the United States these factors roughly cancel each other out, in other economies the balance might be different, with a consequent difference in the savings function.

It is thus an over-simplification to assert that, in the long-run, the proportion of aggregate income saved is proportional to the rate of growth of aggregate income; but it is much nearer the truth than the linear consumption functions so often postulated. It would be interesting to see the effect on the many "theories of economic growth" of substituting in them the *Rate-of-growth Hypothesis* for their present (usually linear) consumption functions.

Once the importance of structural factors is recognised, a vast new field of useful, scientific economic research is opened up. The effects of variations in the birth-rate, of emigration and of changes in the rate of change of productivity are all of great practical importance and open to relatively easy investigation.

The *Normal Income Hypothesis* we found to be well-substantiated for farmers and business-men in the United States, but we kept an open mind as to how far it extends to other countries or occupations where incomes are more stable. However, even if the hypothesis were valid *only* for these groups (and there is no evidence for this) its importance is established. Friend and Kravis (1957b) estimate (p. 296) that these groups are responsible for 55–65% of personal saving in the United States, and (p. 278) that their marginal propensity to save is more than twice that of the rest of the community. As it is well known that their incomes are more variable, it is clear that their behaviour determines a very large proportion of the variation in personal savings.

This serves to put trade-cycle theory on what one might call "a fully expectational footing." The recent orthodoxy has been an expectational theory of investment *plus* a mechanistic consumption function—now consumers can have their expectations too! We must, however, enter a caution. Employment and trade-cycle theory depend, not on the marginal propensity to consume but on the marginal propensity to *spend*, which may be very much

higher. A consumer may well use a temporary increase in his income to purchase some item of durable equipment, thus spending a large part while consuming only a small fraction of the increase. Another way of putting this might be to say that consumers' durables introduce a sort of accelerator effect on the consumer side.

The hypothesis also has implications for the use of budget studies to throw light on consumption behaviour. We have seen that where the Friedman Effect is present and the *Normal Income Hypothesis* holds, a simple regression analysis will give a biased estimate of the income elasticity of consumption. If a partial regression analysis of, say, consumption on current income and a third variable x is made in the same circumstances, even more misleading results can be produced. For a linear function of x and current income may well be significantly more closely correlated with normal income than is current income alone. In this case, x will appear to have a significant effect on consumption, even though there is no causal relation at all. Friedman (1957a, pp. 86–90) gives an excellent discussion of such possibilities.

It is clear that these phenomena make the interpretation of cross-section studies where only current income is recorded extremely hazardous. So far as existing data are concerned, it behoves the research worker to tread warily, with an eye constantly turned towards the possible effects of Normal Income. For future surveys, one hopes that, at the very least, income for several previous years and expected future income will be recorded; it is perhaps too much to hope that cross-section surveys will be replaced by the much more informative continuous budget studies.

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OPEN-MARKET OPERATIONS *VERSUS* RESERVE- REQUIREMENT VARIATION¹

I

WIDELY divergent conclusions have been reached in discussions of the proper choice between open-market operations, on the one hand, and variation of cash-reserve requirements, on the other. Whereas several academic economists have suggested increases of reserve ratios as an alternative or supplement to counter-inflationary open-market operations, central banking authorities, as well as commercial banking circles, are generally opposed to such an innovation.

Thus, in comparing the short-period application of the two instruments, Professor R. S. Sayers considers it a "fact that, if restriction of the deposit total [of commercial banks to a particular level] is thought right by the central bank, its enforcement by one weapon is merely a substitute for enforcement by another."² Accordingly, he adjudges as lacking in substance "the bankers' dislike of the reserve variation weapon . . . when this weapon is confined to short-period service as an alternative to open-market operations."³ His reason for this view is that, "on the assumption that the central bank achieves equal success in its attempts to regulate the liquidity of the economy, the profitability of banking is unaffected by the choice between variation of reserve ratios and open-market operations."⁴

By contrast, the Board of Governors of the United States Federal Reserve System states that:

"The reserve requirement instrument is not . . . interchangeable with the open-market instrument. Unlike open-market operations, the results affect immediately and simultaneously all banks. . . . Changes in requirements, moreover, cannot be made frequently—especially on the up side—without unduly disturbing the operations of individual banks. . . . Changes in reserve requirements are, therefore, made infrequently and typically involve a fairly sizable volume of funds. The effects tend to be large and concentrated within a short period of time. The instrument is more appropriate for making a major change in the volume of available bank reserves than it is for short-run adjustments. It is not adaptable to affecting bank reserve positions on a day-to-day and week-to-week basis, as are open-market operations. In fact, it may be desirable to engage in partially offsetting open-market

¹ I am indebted to Professors G. H. Evans, Jr., F. Machlup, A. T. Peacock and R. S. Sayers for some very helpful suggestions.

² R. S. Sayers, *Central Banking After Bagehot* (Oxford, 1957), p. 89.

³ *Ibid.*, p. 90.

⁴ *Ibid.*, p. 89.

actions in order to cushion the impact of reserve requirement changes in credit markets.”¹

In general accord with Professor Sayers' reasoning, other academic economists have recurrently argued that changes in reserve requirements could be made by much smaller degrees than hitherto, correspondingly reducing the resultant shock effects.² Furthermore, it has been suggested that the immediate and simultaneous impact of a given change in reserve requirements upon all commercial banks may be advantageous by comparison with the delayed effects of open-market operations upon banks removed from the money-market center.³ Finally, it has been pointed out that the direct price and yield effects on the government securities market would be more moderate for a given increase of reserve requirements than for a comparable magnitude of open-market operations.³

*Member-bank Reserve Requirements in the United States,
January 1951–June 1959*
(Per cent of deposits)

Effective date of change.	Demand deposits.			Time deposits.	
	Central reserve. City banks.	Reserve. City banks.	Country banks.	Central reserve and reserve. City banks.	Country banks.
1951—Jan. 11, 16 *	23	19	13	6	6
Jan. 25, Feb. 1 *	24	20	14	—	—
1953—July 1, 9 *	22	19	13	—	—
1954—June 16, 24 *	21	—	—	5	5
July 29, Aug. 1 *	20	18	12	—	—
1958—Feb. 27, Mar. 1 *	19½	17½	11½	—	—
Mar. 20, Apr. 1 *	19	17	11	—	—
Apr. 17 . . .	18½	—	—	—	—
Apr. 24 . . .	18	16½	—	—	—
In effect June 1, 1959 . . .	18	16½	11	5	5

* First-of-month or mid-month dates are changes at country banks, and other dates are at central reserve city or reserve city banks.

SOURCE: *Federal Reserve Bulletin*, Vol. 45 (June 1959), p. 600.

Renewed suggestions that variable reserve ratios be adopted as an additional counter-inflationary weapon by the Mother of central banks⁴ contrast with refusal of the American monetary authorities, already endowed

¹ Replies of the Chairman of the Board of Governors of the Federal Reserve System to Questions Submitted by the Subcommittee on Economic Stabilisation of the Joint Committee on the Economic Report, in volume of Hearings on *United States Monetary Policy: Recent Thinking and Experience* (Washington: December 6 and 7, 1954), pp. 11–12.

² See Charles R. Whittlesey, “Reserve Requirements and the Integration of Credit Policies,” *Quarterly Journal of Economics*, Vol. LVII (August 1944), pp. 558–64.

³ See Ira O. Scott, Jr., “The Regional Impact of Monetary Policy,” *Quarterly Journal of Economics*, Vol. LXIX (May 1955), p. 283.

⁴ See Sayers, *op. cit.*, p. 91, and his reference (p. 85) to Sir Theodore Gregory, *The Present Position of Central Banks*, 1955 Stamp Memorial Lecture (London, 1955), p. 21.

with this tool for more than two decades, to apply it restrictively ever since March 1951. As shown in the table on p. 698, all changes of member-bank reserve requirements in the United States since March 1951 to the time of this writing have been in but one direction: downward. Hence, a reconsideration of reserve-requirement variation in comparison with open-market operations appears timely, both on theoretical and on policy grounds.

In line with the literature on the subject, the next three sections will assume a closed economy. In the final section this assumption will be dropped.

II

Before analysing the differential impact of open-market operations versus that of variation of reserve requirements, it is useful to specify the economic situation in which central-bank action is assumed to take place. Three types of situation may be distinguished.

In the first type of situation commercial banks are without significant excess reserves, and are attempting to switch out of government securities into private loans because of a boom in the demand for credit. This would be the state of affairs associated with a restrictive monetary policy.

In the second type of situation, too, commercial banks are without significant excess reserves but in equilibrium with respect to the structure of their assets. This would be the state of affairs associated with a “neutral” monetary policy.

In the third type of situation commercial banks have considerable or substantial excess reserves. This would be the state of affairs associated with an expansionary monetary policy. Let us take up these three types of situation in turn.

(a) Suppose that, being confronted with the first type of situation, the central bank has decided to reduce the volume of member bank reserves by a given amount, and is considering whether to do so by open-market sale of government securities or by raising member-bank reserve requirements. In this case, is it correct to say that there is no substantive difference between the effects of the two courses of action? Certainly not.

In the face of a boom in the demand for private credit the proximate objective of a restrictive monetary policy is to curb the switching by banks from government securities into commercial loans. Yet the magnitude of such switching would be greater in response to newly raised reserve requirements than it would be in response to an equivalent sale of government securities by the central bank. This outcome is due to two factors.

In the first place, the imposition of higher cash-reserve requirements reduces to zero the rate of return on a certain proportion of commercial banks' assets that have hitherto yielded a positive rate of return. In other words, the higher reserve requirements reduce not only the total volume of commercial-bank assets but also the proportion of commercial-bank earning assets to their total assets. On the other hand, open-market operations

reduce the total volume of commercial-bank assets without reducing the proportion of earning assets to total assets. Thus imposition of the higher reserve requirements has a more restrictive impact on commercial-bank income than the equivalent volume of open-market sales by the monetary authority. Therefore, the marginal utility of bank income is higher as a result of increased reserve requirements than as a result of open-market sales by the monetary authority.¹ The higher marginal utility of bank income induces commercial banks to sacrifice liquidity, *i.e.*, to shift further out of government securities into loans, in order to obtain additional earnings.

Secondly, the liquidity needs of commercial banks are to a greater extent met by the required reserves themselves with a higher reserve requirement than with an unchanged reserve requirement. To illustrate, if required reserves are 10%, and bank deposits drop from 100 to 80, only 2 units of the 20 cash are available for paying off the deposits. On the other hand, if required reserves are 20%, and deposits drop from 100 to 80, 4 units of the 20 cash are available for paying off the deposits. Thus the higher reserve requirements reduce the marginal utility of the moneyness of bank assets, also inducing commercial banks to sacrifice liquidity in order to obtain additional earnings.

Under conditions of a restrictive monetary policy, *i.e.*, amid excess demand in the market for private credit, banks are afforded ample opportunity to offset the constraint placed upon them by the higher reserve requirements. The "income" effect and the "liquidity" effect of imposition of the requirements jointly induce banks to switch out of governments into loans on a larger scale than in response to open-market sales on the part of the monetary authority.

(b) Our foregoing analysis of the first type of situation applies with but minor modification to the second type of situation. Here the "income" effect and the "liquidity" effect of an increase in reserve requirements will still take place. But since there is, by assumption, no excess demand for private credit, commercial-bank shifting from governments to loans will be smaller than in the first type of situation. In other words, the impact of imposition of higher reserve requirements will not be as perverse if, to begin with, commercial banks were in an equilibrium position with respect to the structure of their assets.

(c) Though the third type of situation—widespread excess reserves in commercial banks—is commonly associated with an *expansionary* monetary policy, let us for the moment consider the implications of *higher* reserve requirements in this context.² Here the imposition of higher reserve requirements produces neither the "income" effect nor the "liquidity" effect so long as the pre-existing excess cash reserves fully meet the higher reserve

¹ Alternatively stated, the marginal rate of substitution of bank income for bank liquidity will be lower as a result of the rise in reserve requirements than as a result of open-market sales by the monetary authority.

² The relevance of this consideration to the real world will be discussed in Section IV.

requirements. For so long as the higher reserve requirements can be satisfied without liquidation of earning assets, their imposition does not adversely affect the present income position of the banks involved; nor does it imply an increase in the proportion of liquid assets to total bank assets.

Indeed, amid widespread excess reserves, imposition of higher reserve requirements is, from the viewpoint of debt management, preferable to open-market sale of government securities by the central bank. This is so because higher reserve requirements that merely absorb pre-existing excess reserves do not necessitate any reduction in the government-security holdings of commercial banks and, hence, do not entail a rise in interest rates on government securities. On the other hand, the open-market operation of the central bank adds to the supply of government securities in the open market, a factor making for higher interest rates.

III

The upshot of the preceding section is that there exists an asymmetry between imposition of higher cash-reserve requirements and open-market sale of government securities by the central bank. Specifically, the following conclusions emerge:

(1) Amid booming demand for credit, concomitant with a widespread absence of excess cash reserves in commercial banks, higher reserve requirements will induce more shifting by banks from government securities to private loans than would restrictive open-market operations. This, in turn, implies that effective interest rates on government securities will rise to a greater extent in response to higher reserve requirements than in response to restrictive open-market operations.

(2) In the absence of buoyant demand for credit and in the absence of widespread excess reserves that could meet higher cash-reserve requirements, the higher requirements would still induce more shifting by banks from governments to loans than would restrictive open-market operations. This asymmetry, however, would not be as pronounced as under the conditions of conclusion (1). It still follows that interest rates on government securities will rise to a greater extent in response to higher reserve requirements than in response to restrictive open-market operations; though, again, the asymmetry will be less significant than under the conditions of conclusion (1).

(3) Amid widespread excess reserves that will fully satisfy higher cash reserve requirements, the imposition of higher requirements avoids a rise in interest rates on government securities, since bank shifting from governments to private debt is not made necessary. On the other hand, restrictive open-market operations will raise interest rates on government securities because they augment the supply of governments to the private economy.

Manifestly, conclusions (1) and (2) must be qualified to the extent that there are differences in the reserve positions of different commercial banks. Likewise, conclusion (3) must be qualified to the extent that all or some

banks lack excess cash reserves to fulfil the higher reserve requirements and are, therefore, compelled to liquidate earning assets.

IV

We may now attempt to reassess the running debate between various academic economists and the Federal Reserve on the comparative advantage of changes in reserve requirements versus open-market operations.

To recall, it has been suggested, particularly in the United States and largely in criticism of Federal Reserve policy, that increased use of changes in cash-reserve requirements is preferable to heavy reliance on open-market operations. This suggestion has been justified on two distinct grounds. First and most frequently, it has been asserted that restrictive open-market operations involve direct price and yield effects in the government-securities market which could be avoided by more extensive use of changes in reserve requirements. Second, it has been contended that transmission of the impact of monetary policy throughout the commercial banking system could be accomplished with shorter time lags by means of variation of reserve requirements than through open-market operations. To the oft-repeated Federal Reserve argument that changes in reserve requirements are too blunt an instrument, the repeated counter-argument has been that "The large and infrequent changes in reserve ratios, which the Federal Reserve takes to be the result of defects in the instrument, may instead be responsible for those defects. Open-market operations of comparable magnitude can also be a shock to the markets."¹

What implications do our conclusions have for the opposing views in this continuing controversy? Is there really no more to the Federal Reserve's rejection of the academicians' argument than a tendency "toward an immaculate, high-church, and 19th century view of [central-bank] responsibilities"?²

Our first and second conclusions sharply reduce the validity of the argument that changes in cash-reserve requirements will shield the government-securities market from the price and yield effects entailed by open-market operations. As implied by our third conclusion, this argument is applicable only to the type of situation wherein commercial banks possess sufficient excess cash reserves to meet higher reserve requirements without having to resort to the liquidation of earning assets. (This, indeed, is the type of situation adduced by Professor Sayers in his denial of an asymmetry between the two central banking instruments.³) And this kind of situation is rarely one that is associated with the choice between higher reserve requirements and restrictive open-market operations: rather, it is the kind

¹ Edward S. Shaw in *United States Monetary Policy: Recent Thinking and Experience*, *op. cit.*, p. 74. Substantially the same point was made by Whittlesey, *op. cit.*, pp. 559-60. ² Shaw, *op. cit.*

³ Sayers, *op. cit.*, p. 89. He uses for illustrative purposes the case in which, "with a normal cash reserve of 10 against deposits of 100 the banks hold excess reserves equal to 10," and compares the effects of variation of reserve ratios with those of open-market operations.

of situation that is usually associated with the choice between lower reserve requirements and expansionary open-market operations. Thus, little solace remains to be drawn from the sharply limited applicability of the argument in question: under the conditions which (apart from international transactions) give rise to consideration of the choice between higher reserve requirements and restrictive open-market operations, commercial banks are "loaned up" to begin with. Under such conditions, higher reserve requirements do, in fact, generate a shock effect upon commercial banks—and, hence, upon prices and yields of government securities—that is greater than the impact of restrictive open-market operations.

This difference in shock effect means that it is pointless to argue that more extensive use could be made of higher reserve requirements if increases in requirements were made in very small fractions. Obviously, if a given increase in reserve requirements is minute, its shock effect is also likely to be minute. But this is hardly comforting. The point is that restrictive open-market operations generate less of a shock effect that correspondingly higher reserve requirements. Thus, if what is called for in a given case is *highly* restrictive central bank action, the difference in the shock effects of the two instruments will be of much greater significance than if only *slightly* restrictive action is needed. In brief, substantial increases in reserve requirements will produce substantial differential shock effects; negligible increases in reserve requirements will produce negligible differential shock effects.

The differential shock effect of higher reserve requirements results from the fact that, in conditions wherein commercial banks are "loaned up" to begin with, higher reserve requirements are more deleterious to commercial-bank earnings than are restrictive open-market operations. Unawareness of this fact is at the bottom of the opposition to the Federal Reserve's views on the variation of reserve requirements.

Our conclusions also weaken the argument that changes in reserve requirements are preferable to open-market operations on the ground that the former will reduce time lags in the transmission of the effects of monetary policy to the commercial banking system. If results of the variation of reserve requirements were the same as results of open-market operations in all respects but speed of transmission, preference for the former weapon over the latter would be quite plausible. We have seen, however, that there is an asymmetry in results, an asymmetry which militates against the increase of reserve requirements as a counter-inflationary measure. For one major objective of a restrictive monetary policy is to curb the expansion of commercial-bank credit to private borrowers. This objective is thwarted by the variation of reserve requirements in comparison with the use of open-market operations. When, in addition, differences between the two weapons in administrative flexibility and in ease of reversibility are taken into account, little is left of the case for the use of changes in reserve requirements as a short-period alternative to open-market operations.

Would it be proper to conclude that adoption of the reserve-variation weapon is entirely without merit? ¹ Not quite.

In economies where a government-securities market is lacking or where the central bank's portfolio of government securities is too meagre for the exclusive use of open-market operations, the additional weapon of changing reserve requirements is well nigh indispensable. But, as the government-securities market in such economies expands over time—owing, say, to a growing national debt in the course of governmental deficit financing of economic-development projects—open-market operations may become an increasingly realistic alternative to variation of reserve requirements.

Even in economies with an extensive government-securities market and with a central bank in possession of a relatively large securities portfolio, there may still be a distinct advantage in endowing the central bank with both weapons. Suppose that a country is experiencing, as was the United States in the mid 1930s, a heavy inflow of gold and that the monetary authority seeks to neutralise the potentially expansionary effect of this inflow upon the domestic money supply. To absorb the increase in commercial banks' excess reserves by means of open-market operations would imply a substantial reduction in the central bank's portfolio of government securities and, hence, a rise in the net interest burden of government debt. On the other hand, to absorb the increase in excess reserves by means of higher cash-reserve requirements for commercial banks would leave the central bank's security portfolio intact and would avoid any addition to the net interest burden of government debt. Thus, the variation of reserve requirements is not devoid of all merit as an instrument of monetary control, particularly in a world of open economies.

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¹ This is a conclusion reached by Professor Milton Friedman. With reference to the Federal Reserve System, he states, "The tools we now have are in large measure an accidental outgrowth of past history and are by no means best suited to present day needs. The rediscount tool is technically defective as an instrument of stabilization policy. Its effects are difficult to judge and it tends to be discontinuous in operation, so producing speculation in advance of changes. From the point of view of stabilization policy, it would seem to me desirable to eliminate completely the Federal Reserve's rediscounting powers. For similar reasons, control over reserve requirements is a defective instrument of stabilization policy. If we are to have legally prescribed reserves, they should, it seems to me, be fixed and not subject to change by administrative action."

"If rediscount powers and powers over reserve requirements were eliminated, monetary policy would be implemented predominantly through open-market operations. This is by all odds the best monetary instrument of stabilization policy at the present time. There is nothing that can be done by rediscount policy and changes in reserve requirements that cannot be made by open-market operations, and open-market operations are continuous in time and amount. They can be changed in amount from day to day and reversed if necessary frequently and without difficulty. And one ball is easier to juggle than three." Milton Friedman, "Consumer Credit Control as an Instrument of Stabilization Policy," in Board of Governors of the Federal Reserve System, *Consumer Installment Credit*, Part II, Volume 2: *Conference on Regulation* (Washington: National Bureau of Economic Research, 1957), pp. 100-1.

SOME THEORY OF AN IGNORANT MONOPOLIST ¹

ALTHOUGH no one doubts the importance of limited information and uncertainty as factors affecting the management decisions of actual business firms, economists have long been reluctant to introduce such complications into traditional models of price and output determination. To the extent that learning and uncertainty phenomena have been considered at all, they have been studied mainly in the context of recent developments in the mathematical theory of games of strategy; and results reached via this route have had little if any effect upon the thinking of the majority of professional economists, most of whom continue, as before, to think and work in terms of models which presuppose a world of perfect information, perfect certainty and instantaneous response to changing circumstances.

The purpose of this paper is to initiate a redress of the balance: to encourage study of learning processes and delayed responses *within the framework of traditional economic theory*. The paper is directly concerned only with certain natural and potentially fruitful dynamical generalisations of traditional monopoly theory, but it has implications for every branch of established price theory. The discussion rests more on plausible than on demonstrative reasoning, and the mathematical argument is correspondingly elementary. However, some of the economic considerations which motivate the formal analysis are fairly subtle and are developed, accordingly, with more than usual care.

I. THE KNOWLEDGEABLE MONOPOLIST: STATICS

In keeping with the spirit, if not the letter, of traditional theory, consider a market in which, during any specified period of time, t , the quantity of output $x(t)$ of a particular commodity is produced by a monopoly firm and offered for sale to a group of prospective buyers at the unit price $p(t)$. Assume that market demand (monopoly sales) in the same period, denoted by $D(t)$, is a given function of price: $D(t) = D[p(t)]$; and suppose that total costs incurred by the monopolist in producing and selling the quantity $x(t)$ are given by a function $C(t) = C[x(t)]$. Assume also that the function D has a

¹ A complete list of acknowledgments would be embarrassingly long, but it would be indecent to ignore the obligations which I owe to my colleagues R. B. Heflebower, A. A. Charnes, Alvin L. Marty and Mitchell Harwitz, to Alan Walters of the University of Birmingham and to Arnold Zellner of the University of Washington. I am grateful also to the Graduate School of Northwestern University for partial but generous financial support. The research underlying this paper was undertaken for the project on *Temporal Planning and Management Decisions Under Risk and Uncertainty* at Northwestern University, under contract with the U.S. Office of Naval Research (contract Nonr-1228(10), Project 047-021). Reproduction of this paper in whole or in part is permitted for any purpose of the United States Government.

unique inverse, F , and provisionally define the *profit function* of the monopolist to be

$$R(t) \equiv F[x(t)]x(t) - C[x(t)] \quad (0 \leq R(t))$$

(this function being presumed to be continuously differentiable). Finally, let the output of the monopolist in period t be set at any value of $x(t)$, in the domain of definition of the profit function, which satisfies the "maximum profit" condition

$$(1) \quad F[x(t)] + x(t)F'[x(t)] - C'[x(t)] = 0;$$

and let the price of the monopolist in period t be determined by the "market clearance" requirement

$$(2) \quad p(t) - F[x(t)] = 0.$$

Provided the profit function has a unique maximum value (this will henceforth be taken for granted), the conditions (1) and (2) then determine unique *equilibrium values* $\bar{x}$ and $\bar{p}$ of output and price which the monopolist will select and maintain in any and every market period. For purposes of reference, call the system (1), (2), *Model I*.

The preceding remarks provide a complete and accurate account of the essentials of traditional monopoly theory.¹ If the exposition has less "sparkle" than the usual classroom discussion of the same topic, that is probably because the typical instructor adds dynamical and other "frills" to his account of the statical theory. Among other things, it is usual to ask such questions as: "What happens if the monopolist makes a mistake and sets his output at some level other than $\bar{x}$?" Strictly speaking, however, it is not legitimate to ask this kind of question in the context of the model described by (1) and (2). For these equations (and the assumptions underlying them) assert unequivocally that *the monopolist has no choice*: either values of output and price $\bar{x}$ and $\bar{p}$ satisfying Model I exist and are set accordingly; or such values do not exist, in which case monopoly output and price are not specified by the model. To make the question meaningful, it is necessary to relax the assumptions of the theory, tacitly if not explicitly.² Let us relax them explicitly.

II. THE KNOWLEDGEABLE MONOPOLIST: DYNAMICS

Let condition (2) in Model I be retained, but replace (1) by the alternative requirement

$$(3) \quad x(t) - x(t-1) = \lambda \{F[x(t-1)] + x(t-1)F'[x(t-1)] - C'[x(t-1)]\},$$

where λ is a positive constant; *i.e.*, suppose the output of the monopolist varies from one period to the next in direct proportion to the difference

¹ Compare J. R. Hicks, "Annual Survey of Economic Theory: Monopoly," *Econometrica*, Vol. III (1935), pp. 2-4.

² See, *e.g.*, the discussion of "stability" at pp. 3-4 of the Hicks article just cited.

between "marginal revenue" and "marginal cost." To determine output in any period t , it is only necessary to adjoin to (3) an initial condition specifying the value of output in the preceding period; equation (3) then generates a well-defined time series in $x(t)$ and the "market clearance" condition (2) may be used to obtain a corresponding time series in $p(t)$. Call the system described by (2) and (3) *Model II*.

It is now possible to pose the question stated earlier, for Model II does not *require* that output be set at some equilibrium value $x = \bar{x}$ as is the case in Model I. Of course, if output should just happen to be set at the value $\bar{x}$, the expression in braces in equation (3) will be zero and output will not change over time; *i.e.*, $\bar{x}$ is a stationary solution of (3) (which means that Model II is *statically equivalent* to Model I). If output is initially specified to have a value different from $\bar{x}$, however, output (and price) will vary in every subsequent period of time in accordance with (2) and (3). In the neighbourhood of an equilibrium point $(\bar{x}, \bar{p})$, for example, variations in output, starting from some initial value x_0 of x are approximately described by the equation

$$x(t) = \bar{x} + (x_0 - \bar{x})[1 + \lambda(F' + F'' - C'')]^t.$$

From this it is easy to verify that, except for special functions F and C , or special values of the "adjustment coefficient" λ , the approach to equilibrium is always gradual, may be oscillatory, and never terminates; also, that output will tend toward its equilibrium value over time for most "plausible" choices of the functions F and C , provided λ is not too large.

Model II is more interesting in several respects than Model I; in particular, since it is capable of generating states $[x(t), p(t)]$ other than the single equilibrium state $[\bar{x}, \bar{p}]$, it is potentially referable to empirical data sets of the kind which one associates with "going concerns" in the real world. Since it is still a rather dull affair, however, another "classroom" question is in order. Let it be the following: "What happens if the position of the market demand curve shifts?"

From one point of view, this question presents no new problem. Starting from a situation of monopoly equilibrium, and assuming that a shift in the demand function occurs, we have simply to deal with the question "What happens if the output of the monopolist is not set at the equilibrium level?" For the original value of output is simply one of many non-equilibrium values after the shift in demand occurs. In this respect, Model II is more satisfactory than might have been supposed at first sight.

From another point of view, however, the model gets into difficulties. So long as one deals with a fixed demand function, it is reasonably sensible to suppose that the profit and price calculations of the monopolist are made with reference to this function; one has merely to assume that the model applies to a situation in which, following various trial-and-error experiments with different prices, the monopolist knows the precise character of market

demand (at least within some relevant range of price and output quantities). If the demand function is not assumed to be fixed, however, Model II would seem to have definite *empirical* relevance only on a *recurrent* basis; *i.e.*, it would be devoid of practical interest, following any shift in demand, until it could be supposed that a "sufficiently long" interval of time had elapsed to permit the monopolist to discover the new position and properties of the demand function.

Now, a model of the latter type is rather awkward to justify; but if one is to retain Model II, the only obvious alternative to some kind of "recurrent validity" hypothesis is to suppose that the monopolist has *correct a priori knowledge* about market demand in every period of time. Since this postulate imputes a highly artificial type of omniscience to the monopolist, however, it can easily prove more embarrassing than the assumption it is designed to replace. On this view, it seems desirable to seek a generalisation of Model II. A "knowledgeable monopolist" is clearly just a theoretical curiosity; it is time to see whether an "ignorant monopolist" might not be something more.

III. THE IGNORANT MONOPOLIST: NAÏVE STRATEGY

As a preliminary to the development of a more general model, return for a moment to the "recurrent validity" theme and assume that, *although the position of the demand function shifts from time to time, its general form remains always the same*. Specifically, assume that the function is always representable as a straight line of constant slope, say

$$(4) \quad D(t) = ap(t) + b,$$

where $a < 0$ and the constant b is a "shift parameter." If the demand function is assumed to shift not too frequently (no more often, say, than every three market periods), it is then permissible to argue that the monopolist will acquire *correct information* about the *slope* of the function from *ex post* price and sales data (the three-period scheme allows time for two consecutive observations so that a conjecture concerning slope can be made, and then time for a final observation to "confirm" the conjecture!). This having been accomplished, the monopolist's only remaining problem is to find the *position* of the demand function in different periods of time. In the latter connection, however, it is possible to argue that the monopolist—having settled on a definite slope—will *estimate* the current position of the demand function on the basis of recent sales experience—say by reference to sales and price in the immediately preceding market period. But this, added to our earlier assumptions, is formally equivalent to postulating the existence of a (linear) *conjectural demand function* of the form

$$(5) \quad E(t) = a\{p(t) - p(t-1)\} + D[p(t-1)],$$

where the symbol $E(t)$ denotes *conjectured sales* (in period t).¹ Then, since the coefficient a in (5) is assumed to have the same value as the corresponding coefficient in (4), it follows that the *conjectural demand function in period t* is identically the same as the *market demand function in period $t-1$* . Hence, if the revenue function in Models I and II is replaced by a "conjectural" revenue function involving the inverse of the conjectural demand function (5), monopoly output and price behaviour following a shift in market demand may still be analysed in terms of our earlier discussion. It is only necessary to argue as if shifts in the market demand parameter, b , occur one period later than is actually the case. In particular, the conditions for market equilibrium in Models I and II are entirely unaffected by the suggested substitution of a "conjectural" for an "objective" profit function.

Suppose, however, that the monopolist is less clever than the preceding discussion suggests, or that shifts in the market demand function occur so frequently that no amount of ingenuity permits the monopolist to obtain *correct* information about the form of the market demand relation. Specifically, suppose the monopolist believes the market demand function is linear when it is actually non-linear. Suppose, also, that the conjectural demand function of the monopolist is still given by (5). Then the position of the conjectural demand function in any given period is no longer determined simply by the position of the market demand function in the preceding period; its position depends also upon the behaviour of market price. To be sure, the conjectural demand function in period t will always coincide *at a single point* with the market demand function in period $t-1$; but unless the two functions coincide everywhere (which cannot happen on our present assumptions) a change in price (resulting, *e.g.*, from a change in output) leads to a shift in the conjectural demand function even though the position of the market demand relation is fixed.

It follows from this that the output and price behaviour of an "ignorant" monopolist (*i.e.*, a monopolist who manages to make only a "reasonable" guess about the form of the market demand function) cannot be described by working with the simple "lagged" version of Model II described above.

Let us carry out a modification of Model I to allow for the presence of a minimal degree of ignorance and see where it leads. First, replace the function F (which indicates the maximum unit price at which prospective buyers *will* purchase any given level of output) by an appropriate con-

¹ The notion of a "conjectural," or "imagined" demand, or "demand as seen by the seller," is, of course, a standard feature of traditional theories of oligopoly and pure competition; and it has been applied before to problems of monopoly. See Sidney Weintraub, "Monopoly Equilibrium and Anticipated Demand," *Journal of Political Economy*, 1942, pp. 427 ff.; N. Kaldor, "Market Imperfection and Excess Capacity," *Economica*, 1935; R. Triffin, *Monopolistic Competition and General Equilibrium Theory*, pp. 62-6; Bushaw and Clower, *Introduction to Mathematical Economics* Chapter Seven, Sections 4-5; S. Thore, F. Billstrom and O. Johansson, "Models Involving Monopolistic Strategies for Price and Supply," *Report 2 of the Uppsala Econometric Seminar* (mimeographed, no date); Cyert, Feigenbaum and March, "Models in a Behavioral Theory of the Firm," *Econometrica*, October 1958, pp. 611-12 (abstract).

(and so the comparative statics) properties of the model can be analysed in "supply and demand" terms using strictly traditional techniques of analysis.¹ This is most easily seen by working with a model in which

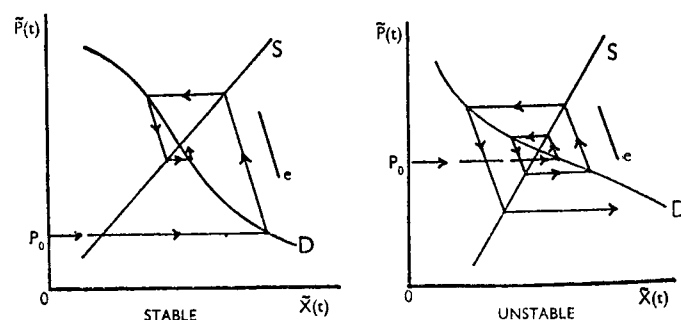


FIG. 2

units of measurement are chosen to make the equilibrium point $(\bar{x}, \bar{p})$ coincide with the point $(0, 0)$. For, in this case, (9) implies that

$$\tilde{x}(t-1) \equiv \tilde{x}(t) / \{(k/a + 1)(1 - k/(2k + c))\} \equiv K\tilde{x}(t),$$

which means that the behaviour of "momentary equilibrium" price (as given by (8)) may be described in the alternative form

$$(10) \quad \Delta \tilde{p}(t) \equiv \tilde{p}(t) - \tilde{p}(t-1) = k\{D[\tilde{p}(t-1)] - K\tilde{x}(t-1)\} (k > 0).$$

And (10) asserts that price variations are directly proportional to *market excess demand*. The analogy with price behaviour in a competitive model requires no further emphasis.

IV. QUASI-SOPHISTICATED STRATEGIES

Although Model III is similar in many respects to a simple dynamical model of a competitive market, the analogy is not perfect. The clearest expression of a basic difference is probably provided by noting that Model III makes no provision for the possibility that the price conjectures of the monopolist may be *parametric*; in every case it is assumed that the coefficient k in the conjectural price function is strictly positive, which implies that the value of conjectured price, $e(t)$, depends on the value assigned to the output variable $x(t)$ —that is to say, the price conjectures of the monopolist are *non-parametric*. Indeed, if these conjectures are assumed to be parametric in Model III (*i.e.*, if it is assumed that $k = 0$), (9) yields the value x_0 of $\tilde{x}(t)$ for all values of t , and (10) asserts that $\Delta \tilde{p}(t) \equiv 0$! This makes nonsense of the entire model.

It is instructive, however, to explore the consequences of this situation a little further. Assume, therefore, that $k = 0$ in Model III, that $\tilde{x}(t_0) = x_0$ and $\tilde{p}(t_0) = p_0$, but that $D[p_0] \neq x_0$ (x_0 and p_0 being given constants).

¹ For a description of the more usual situation in models of this kind, see Bushaw and Clower, *Introduction to Mathematical Economics*, pp. 186–7, 190–92.

Then, although the actual sales of the monopolist differ from desired sales (*i.e.*, output) in every period, the implication of Model III is that output and price will be maintained indefinitely at their initial levels, x_0 and p_0 . Notice, however, that everything would be well *if the coefficient k in the conjectural price function were not also used as the "adjustment coefficient" in (10)*. Specifically, if some positive constant, α , were substituted for k in (10), output variations would be governed by "marginal cost-price" considerations (the assumption $k = 0$ causes no trouble in (7)); and price variations would be governed by straightforward "market clearance" considerations. Moreover, this would seem to be a desirable solution on other grounds. There is no reason to assume that the monopolist is so naïve as to believe that his price conjectures in *any* given period will be correct; and in the absence of such an assumption, it is gratuitous to suppose that price is set *in every* period on the basis of the conjectural price function. It would be just as appropriate to describe price movements in terms of a strictly pragmatic "trial-and-error" process, as suggested above. Following the same line of argument, neither is there any reason to assume that output is set *in every period* to maximise conjectural profit as *currently* conceived; gradual adjustment "in the light of current conditions," as implied by one of the assumptions in Model II, would be an equally legitimate and in some ways a more satisfactory assumption.

In the light of the preceding remarks, it is in order to consider first the model

$$(7) \quad p(t-1) + k\{D[p(t-1)] - 2x(t)\} - C'[x(t)] = 0$$

$$(11) \quad p(t) - p(t-1) = \alpha\{D[p(t-1)] - x(t-1)\} (\alpha > 0),$$

which is simply Model III with a "price adjustment equation" in place of the "market clearance requirement" (8).

Equilibrium conditions for this model are precisely the same as for Model III, namely,

$$(7') \quad p + kD[p] - 2kx - C'[x] = 0,$$

and

$$(8') \quad D[p] - x = 0;$$

but the dynamical characteristics of the system are rather different.

This is conveniently illustrated in Fig. 3, where the curves D , S and e are defined precisely as in Fig. 1, and α is assigned the value $\alpha = 1/2$. Starting from values p_0 , D_0 and x_0 of price, sales and output in period t_0 , output in period t_1 , denoted by $\tilde{x}_1$, is defined by the intersection of the curves e_1 and S . Price in period t_1 is determined, however, by the requirement (11); *i.e.*, $p(t_1) = p_0 + \frac{1}{2}(D_0 - x_0) = p_1$.¹ In period t_1 , therefore, sales are D_1 and

¹ The "tilde" is dropped from the variable p in the present model because the values of $p(t)$ generated by (11) may not (and ordinarily will not) be "momentary equilibrium" values; *i.e.*, if a value $\tilde{x}(t)$ of x which satisfies (7) and a value of $p(t)$ which satisfies (11) are both inserted into (8') the resulting expression is not *identically* zero.

where a and c are defined as before. Compare the corresponding equation for the system (7), (11) given in footnote 1, p. 714.

however, with the warning that the task is difficult and not particularly instructive.¹

IV. CONCLUSION

Ignorance about demand conditions is a ubiquitous feature of market life in the real world. Whatever its source—whether it arises from competition among sellers, lagged response of buyers to changing prices, exogenous changes in tastes, techniques, government policies, etc.—it is a factor to be reckoned with in any attempt to describe observed output and price behaviour. Moreover, the phenomenon of demand ignorance implies—in a plausible if not in a demonstrative sense—the existence of learning and response mechanisms through which output and price decisions arrived at on the basis of provisional conjectures are revised in the light of realised results and ultimately reconciled with experience. The purpose of this paper has been to provide an intuitively plausible and logically coherent description of some particularly simple mechanisms of this kind.

In this connection, it is worth emphasising that the orientation of the preceding analysis is not entirely philosophical. On the contrary, the argument has been influenced at every stage by tacit recognition of the fact that the lack of provisionally plausible theoretical models of other than “purely competitive” markets has long been a major obstacle to systematic empirical study of the production and marketing activities of individual business firms. Whether any of the models outlined above can be used to obtain fruitful results in this area is, of course, another question. It may be noted, however, that most of these models have the econometrically convenient properties of linearity and identifiability. Also, most of them owe their existence to an earlier (and unsuccessful) attempt to reconcile traditional monopoly theory with factual descriptions of the behaviour of “monopolistic” firms in the United States.² Finally, preliminary analysis of price, sales, output, and inventory data obtained from a selected sample of major U.S. firms has yielded results which are sufficiently promising to encourage deeper and more systematic studies in the same direction. While the immediate emphasis of the present paper is theoretical, therefore, its original sources, current uses and ultimate aims are all distinctly practical.

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¹ The relevant characteristic equation (cf. footnote 1, p. 715) is

$$f(r) \equiv \begin{vmatrix} r - \alpha/a - 1 & \alpha \\ -\beta(k/a + 1) - \gamma/a & r + \beta(2k + c) + \gamma - 1 \end{vmatrix} = 0,$$

² A particularly instructive study of this kind is by Edmund P. Learned, “Pricing of Gasoline: A Case Study,” *Harvard Business Review*, Vol. XXVI, November 1948, pp. 723–56.

MALTHUS AND KEYNES—A RECONSIDERATION¹

I

In the past few years the view has solidified that Malthus was one of the most important precursors of the Keynesian way of thinking. This view was given its main impetus by Keynes himself, first in the memoir on Malthus and later in the *General Theory*.³ It bears testimony to the revolutionary nature of Keynes' contribution that prior to his work very little attention was paid to this element in Malthus' thought. This is true not only of general histories of economic thought,⁴ but also of the specialised works devoted solely to Malthus. Bonar,⁵ for example, devoted only a few pages to Malthus' treatment of the possibility of overproduction, whilst Palgrave gave only a paragraph to this topic.⁶

There are, of course, one or two exceptions to this early appraisal of Malthus' work; John Lalor is perhaps the most interesting of these, and as early as 1851 he was prepared to rate Malthus' theorising in this field higher than his contribution to population economics.⁷ But in general, a glance at the standard histories before the “Keynesian Revolution” will affirm that this aspect of Malthus was largely ignored.

It is often said that the question of overproduction was, after all, only a minor part of Malthus' total contribution and hence did not warrant much consideration, but there is very little evidence for this view. It is one of the dominant themes in the Ricardo–Malthus correspondence and occupies nearly a third of Malthus' *Principles*.

Keynes changed all this. Malthus' fear of over-saving now receives an honourable mention in the “new” histories, and some writers claim to have found many of the “building blocks” of the *General Theory* in Malthus. In particular, the writings of Hansen, Lambert and O'Leary may be mentioned.⁸

¹ I should like to acknowledge the advice of Professor Lionel Robbins, Mr. Piero Sraffa, Mr. H. Klappholz and Mr. G. C. Archibald in the preparation of this paper.

² “Robert Malthus: the First of the Cambridge Economists,” *Essays in Biography* (1933).

³ *The General Theory of Employment, Interest and Money*, Chapter 26.

⁴ E.g., L. H. Haney, *History of Economic Thought* (1917), p. 204, “As to overproduction, Malthus agreed with the majority of his contemporaries in believing it possible as a general condition, his real being that there are limits to parsimony or saving. He was clearly in error.”

⁵ James Bonar, *Malthus and his Work* (1924).

⁶ *Palgrave's Dictionary of Political Economy* (Ed. by H. Higgs, 1925), Vol. 11, article on Malthus.

⁷ John Lalor, *Money and Morals* (Preface). Mention should also be made of J. M. Robertson's *Fallacy of Saving* (1892).

⁸ A. H. Hansen, *Business Cycles and National Income*; Paul Lambert, “Keynes und Malthus,” *Wirtschaftliches Archiv*, 1955; Preface to J. Stassart, *Malthus et la Population*; J. J. O'Leary, “Malthus and Keynes,” *Journal of Political Economy*, 1952; “Malthus's General Theory of Employment and the Post-Napoleonic Depressions,” *Journal of Economic History*, 1943. Professor O'Leary seems to have taken up the most extreme position on this point. He argues that “le

In my view this attempt to set up Malthus as a forerunner of Keynes is of doubtful validity. My criticism is not merely that the functional relationships making up the theoretical framework of the *General Theory* are not to be found in Malthus. It is more fundamental than this. I also object to the view, stated so strongly by Keynes, that "the almost total obliteration of Malthus' line of approach and the complete domination of Ricardo's for a period of a hundred years has been a disaster to the progress of economics."¹

In what follows I shall investigate rather more closely than has so far been the case the exact nature of Malthus' dispute with the orthodox school, and seek to show, first, that Malthus was not in fact an analytic forerunner of Keynes, and second, that victory for Malthus in his dispute with Ricardo over the effects of capital accumulation would probably not have hastened the development of Keynesian economics.²

II

Various parts of the Keynesian system have in their time been highlighted as the "essence" of the contribution. For example, the consumption function has been described by some writers as the key to Keynes' break with the Classics,³ but clearly this is not so. With a mechanism to ensure the equality of planned saving and investment, the fact that a percentage of national income is not consumed does not necessarily present any problems of aggregate demand. On the other hand, there does seem to be general agreement about the fact that one of the important differences between Keynes and the Classics concerns the assumptions made about the savings-investment mechanism. Professor Paul Samuelson has put this point of view clearly in the following terms:

"I myself believe the broad significance of the *General Theory* to be in fact that it provides a relatively realistic, complete system for analyzing the level of effective demand and its fluctuations. More narrowly, I conceive the heart of its contribution to be that subset of its equations which relate to the propensity to consume and to saving in relation to offsets-to-saving. In addition to linking saving explicitly to income, there is an equally important denial of the implicit 'classical' axiom that motivated investment is *indefinitely expansible or contractable*, so that

parenté entre les deux auteurs est plus intime encore que Keynes en l'a cru." The reasons he gives for this view appear to ignore the question of the *ex ante* equality—or otherwise—of savings and investment. Cf. the Preface to Stassart, *op. cit.*, p. 14. Professor Robbins allowed me to see some correspondence he had had on this subject with Professor Lambert which was helpful in clarifying this issue.

¹ J. M. Keynes, *Essays in Biography*, pp. 140–1.

² There are, of course, certain contemporary economists who have never accepted this new verdict on Malthus; R. F. Harrod, for example, has written, "I cannot believe that Malthus, splendid as he was as a population theorist, contributed much of value to economics, in which he was always muddled." *The Life of John Maynard Keynes*, p. 460.

³ "It has been my conviction for years that the great contribution of Keynes' *General Theory* was the clear and specific formulation of the consumption function." A. H. Hansen in *The New Economics* (ed. S. E. Harris), p. 135.

whatever people *try* to save will always be fully invested. It is not important whether we deny this by reason of expectations, interest rate rigidity, investment inelasticity with respect to over-all price changes and the interest rate, capital or investment satiation, secular factors of a technological and political nature, or what have you. But it is vital for business-cycle analysis that we do assume definite amounts of investment which are highly variable over time in response to a myriad of exogenous and endogenous factors, and which are not automatically equilibrated to full employment saving levels by any internal efficacious economic process."¹

Now an essential feature of Classical macro-economics was the equality of saving and investment; the Classics were prepared to admit the possibility of a breakdown in the economic system if uncompensated hoarding were to be attempted, but generally speaking, its occurrence on any significant scale was thought to involve an assumption of irrational behaviour.² At the root of Classical (and Neo-Classical) economics was the "saving is spending" theorem, and this clearly has to be sharply contrasted with the Keynesian approach. As Keynes himself pointed out:

"Contemporary thought is still deeply steeped in the notion that if people do not spend their money in one way they will spend it in another. Post-war economists seldom, indeed, succeed in maintaining this standpoint consistently; for their thought today is too much permeated with the contrary tendency and with facts of experience too obviously inconsistent with their former view. But they have not drawn sufficiently far-reaching consequences; and have not revised their fundamental theory."³

It is against this background that we must look at Malthus' contribution.

III

The first point to make clear is that throughout his analysis Malthus assumed that an act of saving was always translated into an act of investment, that is to say, he assumed the equality of *ex ante* saving and *ex ante* investment. Thus in this respect Malthus was at one with the orthodox Classical school; as Professor Robbins has remarked, "unfortunately Malthus had insisted that savings were always invested; he spurned the suggestion that they were often hoarded."⁴

Take the following quotation from Malthus:

"A third very serious error of [Say, James Mill and Ricardo] and practically the most important of the three, consists in supposing that

¹ Paul A. Samuelson, "The General Theory (3)" in the *New Economics* (ed. S. E. Harris), pp. 150–1.

² This statement requires qualification: there are instances in the classical literature of discussions of the effects of sudden changes in the demand for money. Perhaps the *locus classicus* here is J. S. Mill's, "Of the Influence of Consumption upon Production."

³ J. M. Keynes, *The General Theory of Employment, Interest and Money*, p. 20.

⁴ L. C. Robbins, *Robert Torrens and the Evolution of Classical Economics*, p. 248. See also Schumpeter, *History of Economic Analysis*, p. 641 n.

No. 276.—VOL. LXIX.

accumulation ensures demand; or that the consumption of the labourers employed by those whose object is to save, will create such an effectual demand for commodities as to encourage a continued increase of produce.

"Mr. Ricardo observes, that 'If £10,000 were given to a man having £100,000 per annum, he would not lock it up in a chest, but would either increase his expenses by £10,000, employ it himself productively, or lend it to some other person for that purpose; in either case demand would be increased, although it would be for different objects. If he increased his expenses, his effectual demand might probably be for buildings, furniture, or some such enjoyment. If he employed his £10,000 productively, his effectual demand would be for food, clothing, and raw materials, which might set new labourers to work. But still it would be demand.'

"Upon this principle it is supposed that if the richer portion of society were to forego their accustomed conveniences and luxuries with a view to accumulation, the only effect would be a direction of nearly the whole capital of the country to the production of necessities, which would lead to a great increase of cultivation and population. But this is precisely the case in which Mr. Ricardo distinctly allows that there might be a universal glut; for there would undoubtedly be more necessities produced than would be sufficient for the existing demand."¹

It is clear from this passage that Malthus assumed that savings "became" investment, and to reiterate within such a model it is not possible to deduce that lack of effective demand is caused by oversaving.² What problem, then, was it that really troubled Malthus? His starting-point was the (obvious) fact that unemployment was a normal feature of capitalist economies. "The actual wealth of all states with which we are acquainted is very far short of their powers of production."³ Somehow or other this situation was due to "overproduction" caused by a deficiency of effective demand.

At this point it is important to distinguish two possible interpretations of the concept of overproduction. First, "general" overproduction caused by an excess of planned saving over planned investment—which was ruled out by assumption both in Malthus' and orthodox Classical reasoning. Secondly, the idea that the translation of "revenue into capital" led to a falling rate of profit and if continued would kill the motive for further accumulation. It is in this second sense that Malthus' fears must be interpreted; assuming profits to be determined by a simple relationship between investment and consumption, then a switch from consumption to investment would (inevitably) produce a lower monetary return than had been expected: if profits had only been "normal" to begin with, the transfer would result in a stagnation in economic activity. The way out of such a situation, Malthus argued, was to stimulate "unproductive" consumption. To

¹ *Principles of Political Economy*, 2nd ed., pp. 322-3.

² For our purposes it is immaterial whether we assume that savers are entrepreneurs, or that there is a flexible rate of interest. Classical macro-economics contains both assumptions.

³ Malthus, *op. cit.*, p. 329.

summarise this view in Malthus' own words: "it appears to be quite . . . certain that an inordinate passion for accumulation must inevitably lead to a supply of commodities beyond what the structure and habits of . . . society will permit to be profitably consumed."¹

Using this second definition of overproduction, it is easy to show that Malthus and Ricardo were in basic agreement concerning its logical possibility, a point which comes out clearly in their correspondence. The typical case Malthus considered was the switch to a higher rate of capital accumulation financed by means of a decrease in luxury spending; continued for a sufficient length there would be a point where nobody wanted the extra output, the rate of profit would be below the minimum necessary for net capital accumulation.

In a sense Ricardo was in complete agreement with all of this, provided that this hypothetical case of Malthus' really did occur. "I affirm with you," he wrote to Malthus, "that 'if the farmer has no adequate market for his produce, he will soon cease to distribute more necessities to his labourers,' with a view to the production of more necessities."²

And when Malthus wrote to say:

"I also fancy that I am fortified with new arguments to prove demonstratively that a neat revenue is *absolutely impossible* under the determination to employ the whole produce in the production of necessities, and consequently that if there is not an adequate taste for luxuries and conveniences, or unproductive labour, there must necessarily be a general glut."³

Ricardo replied: ". . . with a very slight alteration I should entirely concur in your proposition."⁴

Moreover, Ricardo agreed that if Malthus' diagnosis were correct, interventionist measures to maintain total output were required.⁵

So far I have argued that in so far as the dispute between Malthus and Ricardo was about the possibility of a rate of profit insufficient to promote capital accumulation, the two writers were in basic agreement, but a great deal of the heat generated in the controversy was due to the fact that they did not realise this. Malthus fought for a confession that conditions could be thought up which would produce a general glut; Ricardo, equally fervently, regarded such conditions as unrealistic. As usual, a great deal of the misunderstanding was due to imprecise language and terminology, and in addition one cannot help feeling that neither side had a particular desire to end the controversy—it was, after all, one way of keeping the friendship going.

¹ Malthus, *op. cit.*, p. 325.

² *Works of David Ricardo* (ed. P. Sraffa), Vol. VIII, p. 278. Letter dated October 9, 1820.

³ *Ibid.*, Vol. VIII, p. 285. Letter dated October 26, 1820.

⁴ *Ibid.*, Vol. VIII, p. 300. Letter dated November 24, 1820.

⁵ For more on Malthus' attitude towards public works see the present author's "The Theory of the Economic Effects of Public Expenditure in English Classical Economics," *Economica*, 1958.

Each accused the other of wilful refusal to understand; Ricardo complained to James Mill:

"I think he [Malthus] has not understood himself, for what are all his attacks on Say and me, surely not because we have said in all cases there would be motives sufficient to push production to its utmost extent . . ." ¹

Whilst Malthus's apparent frustration appears in a letter to Ricardo:

"I am either unfortunate in my explanations, or your mind is so entirely prepossessed with your own views on the subject of our discussion, that you will not give to any statement, which departs from them the degree of attention which is necessary to put you in possession of what is meant." ²

What has been said so far may be summarised in the following manner: Malthus' main concern was with the effects of a declining rate of profit caused by "excessive" capital accumulation. Unfortunately within the framework of orthodox Classical theory a falling rate of profit was not necessarily a disequilibrating force. The rate of interest (a real phenomenon determined mainly by the rate of profit) equated saving and investment, hence a change in it merely altered the allocation of total spending between consumption and investment. As Keynes himself pointed out, Malthus had no alternative theory of interest to offer, so that ultimately his idea of excessive capital accumulation rested on the assumption that investment was independent of its rate of return.

IV

In this section some of the policy implications of Malthus' analysis will be examined, to elaborate the point that he had much less in common with Keynes than is usually supposed.

First of all, what did Malthus have to say about the effect of wage cuts on the volume of employment? It will be recalled that within the framework of the *General Theory* direct effects are zero—although there is some possibility of secondary effects via the rate of interest. Malthus, on the other hand, thought that full employment would eventually be achieved through wage reductions. In writing of the post-1815 situation he mentions the consequences of a "sudden abundance of corn," it reduced prices,

"the consequence of which was that, in the midst of plenty, thousands were thrown out of employment—a most painful but almost unavoidable preliminary to a fall in the money wages of labour, which it is obvious could alone enable the general income of the country to employ the same number of labourers as before, and, after a period of severe check to the increase of wealth, to recommence a progressive movement." ³

¹ *Economica*, 1958, Vol. IX, p. 13. Letter dated July 9, 1821.

² *Ibid.*, Vol. IX, p. 90. Letter dated October 9, 1821.

³ Malthus, *op. cit.*, p. 393, my italics.

This is straightforward Classical reasoning with the idea of a "wages fund" lurking in the background.

Let us now take a look at the "Malthusian policy for full employment," for it is here that the apparent Keynesian nature emerges most strikingly; probably it is the use of the term "effectual demand" that has misled people. Malthus argued (in clear contrast to Keynes) that, starting from a low level of economic activity, the only way to increase effective demand was to raise consumption or "unproductive expenditures." As he put it: "the greatest stimulus to the continued production of commodities, taken altogether, is an increase in the exchangeable value of the whole mass, before a greater value of capital has been employed upon them." ¹

Or in more explicit terms: "a country with great powers of production should possess a body of consumers who are not themselves engaged in production." ²

All this followed from Malthus' concern with the effects of a falling rate of profit, and the possibility of an equilibrium situation ³ where further investment was unprofitable.

"It is undoubtedly possible by parsimony [Malthus wrote] to devote at once a much larger share than usual of the produce of any country to the maintenance of productive labour; and suppose this to be done, it is quite true that the labourers so employed are consumers as well as those engaged in personal services, and that as far as the labourers are concerned, there would be no diminution of consumption or demand. But . . . the consumption and demand occasioned by the workmen employed in productive labour can never *alone* furnish a motive to the accumulation and employment of capital."

The only expenditure that investment created was the wages of the labour directly involved, hence—

"it is impossible that the increased quantity of commodities, obtained by the increased number of productive labourers, should find purchasers, without such a fall of price as would probably sink their value below that of the outlay, or, at least, so reduce profits as very greatly to diminish both the power and the will to save." ⁴

There seem to be some peculiar assumptions involved here: there are no multiplier effects and investment is replaced in total every period.

There is, finally, the question of the social control of the saving-investment process. Perhaps the most important implication of Keynes' work was the notion that in an unregulated capitalist economy, full employment was fortuitous; hence given maximum production as an end, government control of aggregate saving and investment was necessary. But Malthus did

¹ Malthus, *op. cit.*, p. 361.

² *Ibid.*, p. 398.

³ A situation where only normal profits were being earned.

⁴ *Ibid.*, pp. 314–15.

not think along these lines; for him *laissez-faire* was the guiding rule. At the end of the *Principles* he sums up the position thus:

"Saving, as I have before said, is, in numerous instances, a most sacred private duty. How far a just sense of this duty, together with the desire of bettering our condition so strongly implanted in the human breast, may sometimes, and in some states of society, occasion a greater tendency to parsimony that is consistent with the most effective encouragement to the growth of public wealth, it is difficult to say; but whether this tendency, if let alone, be ever too great or not, no one could think of interfering with it, even in its caprices. There is no reason, however, for giving an additional sanction to it, by calling it a public duty. The market for national capital will be supplied, like other markets without the aid of patriotism. And in leaving the whole question of saving to the uninfluenced operation of individual interest and individual feelings, we shall best conform to that great principle of political economy laid down by Adam Smith, which teaches us a general maxim, liable to very few exceptions, that the wealth of nations is best secured by allowing every person, as long as he adheres to the rules of justice, to pursue his own interest in his own way."¹

V

The conclusion must surely be that victory for Malthus would not necessarily have led to the happy results that Keynes supposed. There appears to be little in the Malthusian analysis which bears affinity to the model of the *General Theory*. Needless to say, this does not imply that victory for the Ricardian approach was a "good thing." The fundamental weakness of Classical economics was its failure to work out a coherent analysis of the determination of income and employment in the short run. And whilst it is true that Ricardo, at least, emphasised that his analysis was meant as a contribution towards the theory of long-period equilibrium, one cannot escape the conclusion that by and large the orthodox Classical group were prepared to let this long-run analysis serve without modification as a description of the economic system at all points of time.

Malthus was undoubtedly a better observer of the economic scene than his opponents—he was aware that full employment was not the norm—but it cannot be said that his analysis provides an explanation of this phenomenon. Probably critics have been misled by Malthus' policy proposals, for to a certain extent they are not dissimilar to what a "Keynesian" would recommend. But a vague feeling that something is wrong does not justify the overthrow of a particular analytic framework. When a particular theory has been falsified it must be replaced with another theory, but Malthus did not do this, he did not break through the inner logic of Classical macro-economics.²

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¹ Malthus, *op. cit.*, p. 434.

² It may be added here that in the present writer's opinion this breakthrough was Keynes' greatest achievement.

A FURTHER NOTE ON FACTOR-COMMODITY PRICE RELATIONSHIPS

IN a recent issue of this JOURNAL (June 1958) Mr. R. F. Harrod has raised again the question of the effect of trade upon factor prices. As it is not intended to disagree fundamentally with Mr. Harrod, it would seem proper to give at once reasons for wearying the reader with further argument. These are:

(i) That there does remain an important difference of opinion between Mr. Harrod, on the one hand, and Professor Samuelson and the present writer, on the other, regarding the exact nature of the conditions required to ensure factor-price equalisation when the number of goods and factors exceeds two. I do not believe that these conditions can be expressed in terms of factor intensities as implied by two-dimensional accounts and as explicitly stated by Mr. Harrod.¹

(ii) That the devastatingly restrictive nature of the conditions referred to, for many goods many factors, has been widely overlooked because of the general difficulty of the multi-dimensional argument.

(iii) That the only full proof of the general case so far published² can be very much simplified. In this connection there is also a small point of difference between the present writer and Professor Samuelson which it has seemed desirable to clear up in an appendix.

(iv) That it seems appropriate in this context to comment briefly on two points of methodology.

It is convenient to begin with the methodological complaints. First, following well-tried and undeniably useful habits of thought we tend, in economic argument, to assume that, even if our hypotheses are not precisely in accord with the facts, our results are still applicable *in some degree*. We suppose that, in general, our assumptions are reasonable approximations or simplifications which give insight into the problem. If they are not strictly met, then there may be impediments of one kind or another to the working of the mechanism, but the mechanism itself remains fundamentally unaltered.

Without wishing to question the value of this procedure in a general way it cannot be too strongly emphasised that it will, in some cases, let us down severely. Professor Samuelson's celebrated factor-price equalisation theorem³ provides us with a case in point. Most of the assumptions upon which this theorem rests can be regarded as reasonable first approximations. This is not true, however, of the factor intensity assumption, to which Mr.

¹ ECONOMIC JOURNAL, June 1958, p. 249.

² James and Pearce, "The Factor Price Equalisation Myth," *Review of Economic Studies*, 1951/52, p. 111.

³ ECONOMIC JOURNAL, June 1948, June 1949.

Harrod has recently directed our attention (*loc. cit.*). If this restriction on the production function does not apply in the real world there exists much more than a mere impediment to the complete realisation of factor-price equalisation. On the contrary, there exists a mechanism in its own right which could lead to precisely the *opposite* effect. Factor prices could move apart as a result of trade. A step-by-step account of how this might occur is given in the present writer's paper already cited. Moreover, the same considerations apply *a fortiori* if the more complicated conditions appropriate to the general case are not met. As far as the factor-price equalisation theorem is concerned therefore *everything* depends on precise correspondence between the assumptions and the facts of the real world.

My second methodological point concerns the assumption of two goods two factors only, in terms of which a great deal of economic theory is commonly set out. It is tempting to regard two-dimensional analysis as reasonable on the ground that the number of goods and factors is merely a matter of classification. If some measure of classification is inevitable, why not go all the way and fit all goods into one or other of two groups? The difficulties involved are no more than a facet of the index-number problem, which is always with us in any case.

The objection to this attractive argument is that there are times when the economic theory under review is itself no more than a facet of the index-number problem. The price of a composite commodity is precisely the weighted sum of the prices of the factors which produce it, the weights being the ratio of inputs to output. The factor-price equalisation theorem seeks to establish that the connection between factor prices and their weights is such that there is one, and only one, way of building up any given commodity price, at the same time fulfilling all the economic conditions for equilibrium. If the world in fact contains many factors, whose relative prices vary independently, then we are simply assuming away the major part of our problem by trying to fit it into two dimensions. We must not say that by a composite commodity price we mean a "suitable" price index, when the very question we are investigating is what weights are admissible and what are not? However complex and clumsy our methods may be, we have no alternative but to face the complications of the general case. To do otherwise is merely to engage in self-deception.

Accepting this necessity, we have reluctantly to conclude that the very clarity of Mr. Harrod's explanation of the factor-intensity hypothesis is, in a sense, unfortunate. This is because factor intensities are entirely irrelevant to the problem as soon as we pass to three or more goods and factors. The whole concept of factor intensity in fact turns out to be, in this context, a particularly disastrous red herring; for the supposition that factor intensities do not reverse themselves seems to have been widely regarded as empirically likely and, accordingly, the factor-price equalisation theorem has come to be accepted, even by economists who would readily concede the existence in

the real world of more than two factors of production as defined above. Thus it may be that support for the theorem rests, either upon the belief that arbitrary classification of goods and factors cannot affect the issue, or upon an incomplete understanding of the n dimensional complications. In short, Mr. Harrod's case, strong though it is, is weaker than it need be. It seems important to make it clear that the likelihood of factor-price equalisation is immeasurably reduced by the addition of more goods and factors, not simply because there are now more factor intensities which might reverse themselves, but because the whole nature of the necessary condition on the production function is transformed and becomes progressively more restrictive with each extra dimension. An attempt is made to explain this below.

As the extent of the controversy suggests that what follows may have interest even for non-specialists, it is assumed throughout (except in the appendix) that the reader is not familiar with the languages of topology and matrix algebra. Apologies are tendered to those who are. For the same reason I have tried to develop analogies which I hope will gain in clarity what they lose in rigor.

For the sake of brevity we use the following notation. x_{ij} is the quantity of the i th factor used in the production of the j th good. ϕ_j is the output of the j th good and q_j is its selling price. Now Professor Samuelson's assumption of constant returns to scale means that the total value of every product will be just exhausted by the total paid out to the factors which produce it whatever the output. In our notation therefore we may write down n equations as under, where n is the number of goods and factors.

$$\sum_{i=1}^n p_i x_{ij} = q_j \phi_j \quad (j = 1, 2, \dots, n)$$

[$p_i x_{ij}$ is, of course, the bill for the x_{ij} units of the i th factor used in the production of product j . $\sum_{i=1}^n p_i x_{ij}$ means the sum of all the bills for all factors from 1 to n used to produce j . This is equal to the value of output $q_j \phi_j$ of the j th good. There are n of these equations, one for each j .]

Now we know that the quantities x_{ij} of factors used are determined by factor prices p_i and commodity prices q_j so that our n equations can be thought of as n equations in the $2n$ variables p_i, q_j . That is to say, given the q_j , we may solve for all p_i or, given p_i , we may solve for all q_j . Professor Samuelson's proof that trade will equalise factor prices is essentially a proof that these n equations have one, and only one, solution. That is, there is one and only one set of p_i corresponding to each set of q_j , and vice versa. The necessity for factor-price equalisation follows at once from this; for the assumption of identical production functions in all countries means that the same set of n equations will do for all countries. And, since trade equalises commodity prices as between countries and there is one, and only one, set of corresponding factor prices, factor prices also must be equalised.

Normally, of course, a set of equations will have more than one solution unless they are of a particular form. For example, $q = p^2$ has two solutions: $q = +p$ or $q = -p$. Any given value of q has two corresponding values of p , not one. If our n equations above are of a form which gives rise to multiple solutions countries may have different factor prices with the same commodity prices so that factor-price equalisation is not a necessary consequence of trade. Our problem in short is to find the condition which ensures that our equations have only one solution.

Consider for a moment the simple example $q = p^2$. For most values of q the two corresponding values of p will be very different; e.g., when $q = 9$, p will be either $+3$ or -3 . The difference between the two p 's is 6. But we can always choose some value of q such that the two solutions are very close; e.g., when q equals one over 1 million the corresponding p 's will be $+0.001$ and -0.001 . The difference between solutions is reduced to 0.002. We can in fact say that we can find some values of p and q in the region of which a small change in p (namely from $+0.001$ to -0.001) leaves q unchanged whilst still satisfying the equation $q = p^2$.

This principle is of very general validity and applies to all types of equations likely to have any economic significance.¹ If a set of n equations in n variables p_i and n arbitrarily chosen constants q_j have more than one solution (not close together) for any one set of q_j it will usually be possible to choose some other set of constants q_j such that the solutions are close together. Conversely, if there exists, for any q_j , solutions close together, it will usually be found that at least two solutions exist for any set of q_j for which a real solution is possible.

To put this another way, where the form of the equations we have to solve is such that we can find *some* set of factor and commodity prices such that a small change in factor prices leaves commodity prices unchanged, then our equations will in general give rise to multiple solutions for *any* set of commodity prices. If, on the other hand, we can be sure that *every* small change in factor prices must lead to some change in commodity prices, then there will be one, and only one, solution for each set of commodity prices.

We now apply this principle to our equations.

$$\sum_{i=1}^n p_i x_{ij} = q_j \phi_i \quad (j = 1, 2, \dots, n)$$

As we are interested only in values of p , x , q and ϕ , which satisfy these equations, any small change in the left-hand side must be equal to the corresponding change in the right-hand side. Thus

$$\sum_{i=1}^n (dp_i x_{ij} + p_i dx_{ij}) = dq_j \phi_j + q_j d\phi_j \quad (j = 1, \dots, n)$$

where a d preceding any variable is read "the small change in."

¹ It is not, however, universally true. Readers who have doubts are invited to turn to the appendix to this note.

[It will be recognised that the principle followed in writing the change in $p_i x_{ij}$ as $dp_i x_{ij} + p_i dx_{ij}$ is precisely the same as that invoked when we show the marginal revenue of a monopoly to be made up of the change in sales multiplied by price plus the (negative) change in price multiplied by sales.] But we know already that:

$$\sum_{i=1}^n p_i dx_{ij} = q_j d\phi_j \quad (j = 1, 2, \dots, n)$$

for factor prices p_i are equal to the marginal-value products of the factors. These last equations merely state that the value of the change in the product is equal to the sum of the marginal-value products of the new factor quantities employed.

Hence by subtraction

$$\sum_{i=1}^n dp_i x_{ij} = dq_j \phi_j \quad (j = 1, \dots, n)$$

Now we have argued above that whether factor prices are equalised or not depends on whether or not we can find some p_i and q_j (i.e., some x_{ij}) such that a set of small changes dp_i leave q_j unchanged. That is, if we can find some x_{ij} such that in the above equation all dq_j are zero so that

$$\sum_{i=1}^n dp_i x_{ij} = 0 \quad (j = 1, \dots, n) \quad \dots \dots \dots (i)$$

then multiple solutions are possible and factor prices need not be equalised. If, on the other hand, no such set of x_{ij} is possible, then only one solution of our equations exists and factor-price equalisation is inevitable. Mathematicians will recognise at once that a non-zero solution to equations (i) will exist if, and only if,

$$|x_{ij}| = 0$$

Hence the condition for factor-price equalisation given in my paper (*loc. cit.*).

For those unfamiliar with the relevant mathematical theorem equations (i) are set out in full for the two-factor case. Multiple solutions demand that for one set of values of p_i and q_j the x_{ij} should be such that

$$x_{11} dp_1 + x_{21} dp_2 = 0$$

and

$$x_{12} dp_1 + x_{22} dp_2 = 0$$

Given the x 's, what we now have is two equations in two variables dp_1 and dp_2 . These equations are also of a particular kind. In geometric terms

they are straight lines with slopes $\frac{x_{11}}{x_{21}}$ and $\frac{x_{12}}{x_{22}}$ which cut only at the origin

where all dp 's are zero. In general, all dp 's zero, is the only simultaneous solution of these equations, a fact which merely tells us that if we do not change factor prices we will not change commodity prices either whatever the value of the x 's.

Our two equations can have a solution with non-zero dp 's, however, if

the two straight lines are coincident. That is, if they have the same slope so that

$$\frac{x_{11}}{x_{21}} = \frac{x_{12}}{x_{22}}$$

This is, of course, the factor-intensity condition, where the ratios of the inputs of factors one and two are the same for both products. This means that, in the region where factor intensities are equal, any small change in factor prices will leave commodity prices unchanged. If such a region exists factor-price equalisation is not inevitable.

This geometrical interpretation can be extended to the case of three factors three goods. The three equations (i) now become three *planes* whose common point is the origin (zero dp 's) as before. (Three planes will, of course, in general cut only in a single point.) If we think of the point of intersection (the origin) as the corner of a room, then the slopes of the lines formed by the intersection of each plane with walls and floor will be numerically equal to factor intensities. If all three planes were coincident factor intensities would be the same in all cases. But the important point is that for a non-zero solution we do *not* require all three planes coincident. This is much too strong a condition. Three planes may intersect in more than one point without being coincident. Two planes intersect in a straight line. If the third plane cuts the second in the same straight line as the first, then all three planes have a *line* in common. It may in fact be possible to choose non-zero dp 's which satisfy the three equations (i) even if the three planes are not coincident, that is, even if there is no point where factor intensities are the same in all cases. Moreover, this may be possible even if no factor intensities are ever at any time the same for any two goods.

To find the true condition which ensures a non-zero solution to equations (i) we imagine that our x 's are now variables and the dp 's are constants. Since the dp 's are the same in all three equations, then all three must be the same equation and the x 's must be values satisfying it. The graph of this single equation must again be a plane through the origin, the slopes of which are determined this time by the dp 's. If the x 's all satisfy this equation they must lie on this plane.

We now see geometrically how the factor-price equalisation condition generalises. In two dimensions (measuring x 's along the axes) the point x_{11} , x_{21} must never lie on the same straight line through the origin as the point x_{12} , x_{22} . In three dimensions the points x_{11} , x_{21} , x_{31} ; x_{12} , x_{22} , x_{32} ; and x_{13} , x_{23} , x_{33} ; must never be on the same *plane* through the origin. This condition cannot be explained in terms of factor intensities. Written in full, it requires that the expression,

$$x_{11}(x_{22}x_{33} - x_{23}x_{32}) - x_{12}(x_{21}x_{33} - x_{23}x_{31}) + x_{13}(x_{21}x_{32} - x_{22}x_{31})$$

should never be zero for any values of p and q . This is a condition highly unlikely to be met even if the factor-intensity condition is. To show this consider again the two-dimensional case.

Let us represent the production functions for two commodities in terms of single equi-product contours showing combinations of inputs which will produce one unit of each good. On a single large playing-field we set out two race-tracks in the shape of these iso-product curves and set a spectator at a point representing the origin. Two runners set off, one on each track, their speed of running being determined by the curvature of the tracks. If at any time during the race the runners and the spectator lie on a straight line, *i.e.*, if the two runners appear as one to the spectator, this is equivalent to the case where trade does not necessarily tend to equalise factor prices.

The three-dimensional analogue would be three aircraft flying at different speeds along curved tracks at different and varying heights. The three aircraft must never lie in the same plane as a fixed control tower on the ground. That is they must never *appear* to the observer in the control tower to lie in a straight line.

It is evident that there are vastly more degrees of freedom in the three-dimensional case than the two. In the first place, since we now have equi-product surfaces rather than curves, our aircraft must be thought of as taking part in an infinity of races, one for each possible track across the equi-product surfaces. If at *any* time in *any single* race the aircraft appear to the observer to be in a straight line this is equivalent to the breakdown of the factor-price equalisation theorem. Moreover, there are more degrees of freedom in any single race in the three-dimensional case than in the two. To demonstrate this think of two runners on two straight parallel tracks as two-dimensional and three runners on three parallel tracks as three-dimensional. The runners do not begin from the same start, but they run at different speeds. In the two-dimensional case if one runner overtakes and passes the other this is the analogue of the breakdown of factor-price equalisation. In three dimensions the breakdown occurs not as we might think if the runners come three abreast or if any two runners pass one another. It occurs if the three ever at any time lie in a straight line. This can happen whether any one runner passes any other or not.

Finally, attention is drawn to the fact that as we increase the number of goods and factors above three we increase still further the number of degrees of freedom. It is not only that we cannot express the factor-price equalisation condition in terms of factor intensities when there are more than two goods. We must take note also of the fact that the condition on production functions becomes more and more restrictive as we increase the dimensions. To assume that it is met is merely to assume what we want to prove.

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APPENDIX

The reader who has followed Professor Samuelson's brief account of the general case¹ treated above may be disturbed by what appears to be a difference of opinion. It has been contended here that

$$|x_{ij}| \neq 0$$

is a necessary and sufficient condition for factor-price equalisation, whereas Professor Samuelson seems to argue that it is not even sufficient. Conceding that this condition is both necessary and sufficient to exclude the possibility of multiple solutions of our equations in the region of a point, he expresses a doubt whether it is so in the large. He goes on to prove that one *sufficient* condition for uniqueness in the large includes the additional requirement that there should be some numbering of factors such that the leading principal minors of $|x_{ij}|$ of every order should be non-zero also.

The key to the apparent contradiction lies in my contention that, if a set of equations for one value of the parameters has more than one solution in the large, it will usually be possible to choose some value of the parameters such that the new equations have multiple solutions in the region of a point. If this were universally true, conditions necessary for the exclusion of multiple solutions in the region of a point must exclude also multiple solutions in the large, provided they hold for all possible values of the variables.

In fact, Professor Samuelson's extra conditions cover cases where solutions exist in the large which cannot be brought together by any change of parameters. For example, the equation

$$\tan p - k = 0 \quad (k \text{ constant})$$

has an infinity of distinct solutions $p', p'', \text{ etc.}$, such that $p' - p'' = 2\pi$ whatever the value of k . But this arises solely out of the fact that the curve $\tan p$ has an infinite number of parts. $\tan p$ becomes infinite for finite values of p .

The interested reader may care to consider the properties of the following pairs of equations

$$\begin{aligned} & \left. \begin{aligned} (p_1 - a)(p_2 - a) - q_1 &= 0 & q_1 &\neq 0 \\ (p_1 - a)^2 - (p_2 - a)^2 - q_2 &= 0 & q_2 &\neq 0 \end{aligned} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (i) \\ & \left. \begin{aligned} \tan p_1 - \frac{1}{2}p_2 - q_1 &= 0 \\ \frac{1}{2}p_1 - p_2 - q_2 &= 0 \end{aligned} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (ii) \end{aligned}$$

Equations (i) have two solutions for every set of q . Equations (ii) have an infinity of solutions for every set of q . In case (i) the Jacobian (see Professor Samuelson, *loc. cit.*) does not vanish (equivalent to $|x_{ij}| \neq 0$), but all principal minors do. In case (ii) neither the Jacobian or its minors can ever vanish whatever the values of p , but one minor does become infinite. It would seem from case (ii) that even Professor Samuelson's sufficient conditions are not really sufficient if any minor of the Jacobian can become infinite.

The common feature of these examples is that at least one equation can be satisfied with one p infinite, leaving the remaining prices finite. Now it would seem economically unsound to suppose that our functions should be such as to allow a finite set of commodity prices to generate some infinite factor prices. Thus we can exclude cases where the functions have two or more parts on purely economic grounds.

In short, I hold that the assumption that only infinite commodity prices can give rise to infinite factor prices is a suitable alternative to Professor Samuelson's conditions on the minors of the Jacobian. If we make this assumption the additional condition $|x_{ij}| \neq 0$ is necessary and sufficient to ensure factor-price equalisation both in the small and in the large as I have argued. Unfortunately, as we have seen, even this modified condition is highly restrictive and unlikely to be met.

¹ *Review of Economic Studies*, Vol. XXI, 1953/54, p. 9, and Appendix 3, p. 16.

PROTECTION, REAL WAGES AND REAL INCOMES¹

1. IN a recent article in this JOURNAL² on "Protection and Real Wages: A Restatement," Mr. Lancaster has re-examined the famous Stolper-Samuelson theorem and concluded:

"This paper does not deny that protection will raise the real wage of one of the factors, but shows that no general statement about which of the factors this will be can be deduced from the relative 'scarcity' of the factors in the Stolper-Samuelson sense.

"Although the Stolper-Samuelson theorem 'Protection raises the real wage of the scarce factor' is shown to be an incorrect generalisation, a restatement in the form 'Protection raises the real wage of the factor in which the imported good is relatively more intensive' has general validity."

It is proposed in Section I of this paper to review systematically the original Stolper-Samuelson contribution, therewith to advance a critique (distinct from Mr. Lancaster's criticism, which is not accepted), of the Stolper-Samuelson formulation of the theorem and then to restate the theorem: this restatement being considered to be the only true and general statement about the effect of protection (prohibitive or otherwise) on real wages of factors in the context of the basic Stolper-Samuelson model. The logical truth of the restated theorem is briefly analysed then in the context of alternative models. Section II proceeds to extend the scope of the discussion with the argument that, with a non-prohibitive tariff, a sharp distinction must be drawn between the impact on the real wage of a factor and the effect on its real income; some implications of this distinction are then analysed.

I. PROTECTION AND REAL WAGES

2. In the following analysis, we shall take the *basic* Stolper-Samuelson model to mean that the protecting country has two factors, two commodities enjoying different factor intensities, linear and homogeneous production functions subject to diminishing returns (along isoquants) and incomplete specialisation in production. Full employment of factors, pure competition and perfect mobility of factors are also assumed.

Founded on this model, we have three alternative formulations of the theorem concerning the impact of protection on the real wages of factors:

(1) *Restrictive Stolper-Samuelson Theorem*. "International trade necessarily lowers the real wage of the scarce factor expressed in terms of

¹ This paper was read to the Nuffield Economics Society. My thanks are due to Professor Hicks and J. Black for substantial help with the exposition of the paper. I am also happy to record my heavy indebtedness to Professor Harry Johnson, who has been generous with suggestions that have led to numerous improvements in the paper.

² *ECONOMIC JOURNAL*, June 1957, pp. 199-210. The following quotation is from p. 199.

any good.”¹ This formulation restricts itself to the comparison of the free-trade real wage with the self-sufficiency real wage of the scarce factor. The comparison is confined to the case of a prohibitive tariff and excludes non-prohibitive protection. The theorem can be rewritten as follows: prohibitive protection necessarily raises the real wage of the scarce factor.

(2) *General Stolper-Samuelson Theorem*. Protection raises the real wage of the scarce factor.² This formulation is clearly intended to be more general and includes non-prohibitive tariffs as well. To emphasise this, we may rewrite it thus: protection (prohibitive or otherwise) necessarily raises the real wage of the scarce factor.

(3) *Stolper-Samuelson-Metzler-Lancaster Theorem*. “Protection [prohibitive or otherwise] raises the real wage of the factor in which the imported good is relatively more intensive.”³

In the ensuing analysis any reference to “the Stolper-Samuelson theorems” should be taken to relate to the initial two formulations alone; reference to the last formulation will always be by its full title.

3. We can begin by setting out the basic elements in the argument leading to the twin formulations of the Stolper-Samuelson theorem:

(1) protection increases the internal relative price of the importable good:

(2) an increase in the relative price of a good increases the real wage of the factor used intensively in its production;

(3) the importable good is intensive in the use of the scarce factor.

Therefore,

(4) protection raises the real wage of the scarce factor.

These arguments must each be closely examined.

4. Concerning argument (1), we must distinguish between prohibitive and non-prohibitive protection:

(i) Protection will necessarily raise the relative price of the im-

¹ Stolper and Samuelson, “Protection and Real Wages,” *Readings in the Theory of International Trade* (A.E.A., Blakiston Co., 1949), p. 346.

² The actual formulation of the general Stolper-Samuelson theorem is from Lancaster, *op. cit.*, p. 199. While the bulk of their analysis relates explicitly to the restrictive formulation, there are several indications that Stolper and Samuelson had in mind the general formulation as well: (1) a large number of quotations they cite from other authors to outline the problem refer to tariffs in general rather than to tariffs of a prohibitive nature alone; (2) they feel it necessary to assume that “the country in question is relatively small and has no influence on the terms of trade. Thus any gain to the country through monopolistic or monopsonistic behaviour is excluded” (*op. cit.*, p. 344); this assumption is quite superfluous, as we shall later see, if we wish to sustain only the restrictive formulation of the theorem; and (3) the title chosen for the article is not “International Trade and Real Wages” but “Protection and Real Wages.” Lancaster, *op. cit.*, p. 201, also construes the Stolper-Samuelson theorem in its general form; thus witness his argument that “Protection will cause a movement in the general direction $Q'Q$, away from the free-trade point towards the self-sufficiency point” (my italics).

³ Lancaster, *op. cit.*, p. 199. This theorem has been given its stated name on grounds which are made explicit later.

portable good when the tariff is prohibitive; the free-trade relative price of the importable good is lower than under self-sufficiency.¹

(ii) Non-prohibitive protection may either raise, leave unchanged or lower the internal relative price of the importable good. Metzler has demonstrated that this last “perverse” possibility will occur, in the context of our present model, when the elasticity of foreign demand for imports (η_x) is less than the domestic marginal propensity to consume exportable goods (c).² It follows, then, that if imports are not inferior goods in the protecting country’s consumption this case requires inelastic foreign demand; and we can ensure that the internal relative price of the importable good always rises with the imposition of a tariff by assuming *either* elastic foreign demand (sometimes done in the form of assuming a small country) *or* a big enough tariff (in the limit, a prohibitive tariff) for demand to be elastic.

5. Argument (2) follows necessarily from the basic Stolper-Samuelson model. To show this simply, we should recall the technological features of the model employed by Samuelson some years later in this JOURNAL³ to demonstrate factor-price equalisation: these features are identical with those of the Stolper-Samuelson model in all respects. We propose thus to avoid altogether the use of the box-diagram and work instead with the unique relationships that Samuelson derived in these later articles between commodity price-ratios, factor price-ratios and factor proportions in the two industries in a country, from the given assumptions concerning technology alone. These are summarised in Fig. 1, which is reproduced, with slight changes, from Samuelson’s 1949 article.⁴

Let L_C and L_F represent the labour employed in producing clothing and food respectively; T_C and T_F being the quantities of land so employed.

¹ This is true except in a *limiting* case where the terms of trade will not change with trade. This case, however, can be ruled out, in the context of the model used here, by assuming that the community indifference curves (used here without any welfare connotation) are strictly convex. This limiting case will henceforward be ignored.

² Metzler, “Tariffs, the Terms of Trade and the Distribution of National Income,” *Journal of Political Economy*, February 1949, pp. 1–29. It should be emphasised that the Metzler formula for determining the impact of protection on the internal commodity price-ratio relates to the case where the initial situation is that of free-trade. Where, however, the initial situation itself has a tariff and the impact of *increased* protection is the subject of analysis, the “perverse” possibility mentioned in the text will occur, as argued in Section II, when a slightly altered condition is fulfilled. The discussion in Section I, however, is confined to initial situations of free trade, as with Stolper and Samuelson, Metzler and Lancaster.

³ “International Trade and the Equalisation of Factor Prices,” *ECONOMIC JOURNAL*, June 1948, pp. 163–84; and “International Factor-Price Equalisation Once Again,” *ECONOMIC JOURNAL*, June 1949, pp. 181–97.

⁴ This diagram is to be found on p. 188 of Samuelson’s 1949 *ECONOMIC JOURNAL* article, *op. cit.* Full exploration of this diagram is to be found in an excellent article by Professor Johnson, “Factor Endowments, International Trade and Factor Prices,” *Manchester School*, September 1957, pp. 270–83. Professor Johnson, however, works with a slightly adapted diagram, to be found in Harrod, “Factor-price Relations Under Free Trade,” *ECONOMIC JOURNAL*, June 1958, pp. 245–55. On grounds of economy, the discussion of this well-known diagram has been kept brief in this paper.

W/R represents the ratio of wages to rents; L/T the factor endowment ratio of the country; and P_F/P_C the price of food over the price of clothing. Clothing is the labour-intensive industry, food the land-intensive industry, at all relevant factor price-ratios. ($L_C/T_C > L_F/T_F$ at all relevant W/R .)¹ As wages fall relatively to rents, the price of food is shown to rise relative to that of clothing in a monotonic fashion. The factor endowment ratio of the country (L/T) fixes the range of the diagram which is relevant. This is a purely technology-determined diagram, and demand conditions are totally absent from it.

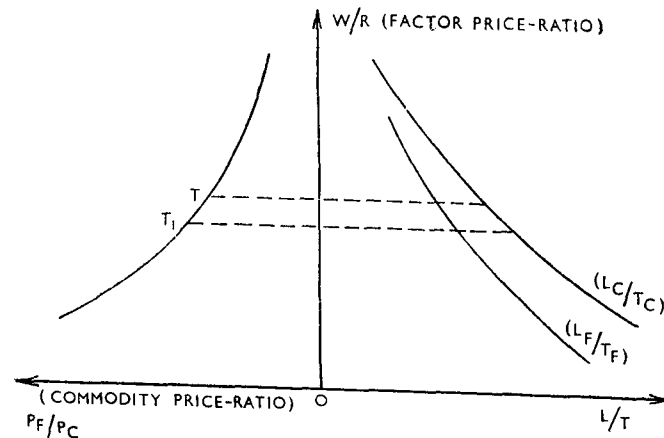


FIG. 1

T being any given commodity price-ratio (P_F/P_C), change it to T_1 such that the relative price of food rises. With it, the labour-to-land ratios in both food and clothing will rise. The marginal physical product of land in both products will thus rise and of labour fall, so that the real wage of land will be unambiguously increased and of labour decreased. Increase in the relative price of food thus increases the real wage of land, the factor intensively employed in its production; and reduces the real wage of labour, the factor intensively used in producing clothing (whose relative price has fallen).

This argument, it should be noted, rests on the assumption, part of the basic Stolper-Samuelson model, that the rise of the relative price of food does not go so far as to make the country specialise completely on food, in so far as the fall in the real wage of labour is concerned; for, once the country is specialised completely, further increases in the relative price of food will raise the real wage of *both* labour and land, which is destructive of the full validity of argument (2).

Given the basic Stolper-Samuelson model, therefore, an increase (de-

¹ Although the factor-intensities of the commodities may be reversible, they *cannot* reverse for a country with a *given* factor endowment. At the present stage of our argument, therefore, we do not need to make the strong assumption that factor-intensities are non-reversible at *all* factor price-ratios.

crease) in the relative price of a good will necessarily increase (decrease) the real wage of the factor intensively used in its production.

6. Argument (3) that the importable good is intensive in the use of the scarce factor is really the well-known Heckscher-Ohlin theorem. The crucial question that it raises is: does the Heckscher-Ohlin theorem follow from the basic Stolper-Samuelson model? To answer this question, we should first have to define "factor scarcity." We may choose from three alternative definitions of factor scarcity:

A. *Lancaster Definition.* A country's scarce factor is that which is used more intensively in the production of the importable good. This definition may be described as tautological, since it turns the Heckscher-Ohlin theorem into a valid proposition by *definition*. It may also be described as an internal definition, since it excludes any comparison with the foreign country. It has been suggested by Lancaster.¹

B. *Heckscher-Ohlin Definition.* A country's scarce factor is that whose relative price is higher than abroad under self-sufficiency. This may also be described as a price definition, since the country's scarce factor is that factor which is more expensive prior to trade than abroad. This definition has been used by Heckscher and Ohlin.²

C. *Leontief Definition.* A country's scarce factor is that of which there are fewer physical units per unit of the other factor than abroad. This may also be described as a physical definition, since it defines scarcity with reference to the relative physical quantities of factors.³

Using each of these definitions in turn, let us analyse the Heckscher-Ohlin theorem.

A: If the Lancaster definition of factor scarcity is used, then the Heckscher-Ohlin theorem holds by definition.

B: If the Heckscher-Ohlin definition of factor scarcity is used then the further assumptions of international identity of production functions and non-reversibility of factor-intensities of commodities between the

¹ Lancaster, *op. cit.*, p. 208, argues that "the only acceptable definition" of a scarce factor is that which defines it as the factor "which is used more intensively in the good of which more is produced in isolation than in trade." It is of some interest to note that tariffs designed to influence distribution are probably set with reference to such internal criteria: to raise the real wage of labour, for instance, tariffs are imposed on labour-intensive industries rather than on products of industries using a factor which is scarcer at home than abroad; with the possible exception of the super-labour argument for such tariffs.

² For a convincing attribution of the authorship of this definition of factor scarcity to Heckscher and Ohlin, see a masterly article by R. Jones, "Factor Proportions and the Heckscher-Ohlin Theorem," *Review of Economic Studies*, 1956-57, pp. 1-10. The definition may be also illustrated in terms of Fig. 1: country A is labour-abundant and country B land-abundant if, under self-sufficiency, $(W/R)_A < (W/R)_B$.

³ W. Leontief, "Domestic Production and Foreign Trade: The American Capital Position re-examined," *Proceedings of the American Philosophical Society*, September 28, 1953; reprinted in *Economia Internazionale*, February 1954. Again, country A is labour-abundant and country B land-abundant if, under self-sufficiency, $(L/T)_A > (L/T)_B$.

two countries will suffice to ensure the full validity of the Heckscher-Ohlin theorem.¹

C: If the Leontief definition of factor scarcity is used, then the three-fold assumptions of non-reversibilities of factor-intensities of commodities between the trading countries and the international identity of both production functions and tastes will ensure the validity of the Heckscher-Ohlin theorem.²

7. We can now sum up on the Stolper-Samuelson formulations as follows:³

A. (1) The restrictive Stolper-Samuelson theorem is logically true if we use: (a) the basic Stolper-Samuelson model, and (b) the Lancaster definition of factor scarcity.

(2) The general Stolper-Samuelson theorem is logically true if we use the further assumption that the elasticity of foreign demand is greater than the marginal propensity to consume exportable goods ($n_x > c$).

B. (1) The restrictive Stolper-Samuelson theorem is logically true if we use: (a) the basic Stolper-Samuelson model, (b) the Heckscher-Ohlin definition of factor scarcity, (c) the assumption of international identity of production functions, and (d) the assumption of non-reversibility of factor-intensities of commodities between the countries.

(2) The general Stolper-Samuelson theorem is logically true if we use the further assumption that $n_x > c$.

C. (1) The restrictive Stolper-Samuelson theorem is logically true if we use: (a) the basic Stolper-Samuelson model, (b) the Leontief definition of factor scarcity, (c) the assumption of international identity of production functions, (d) the assumption of non-reversibility of factor-

¹ This can be seen readily from Fig. 1. If $(W/R)_A < (W/R)_B$ and production functions with non-reversible factor-intensities are common between the countries, then we can see that $(P_F/P_C)_A > (P_F/P_C)_B$ under self-sufficiency and the labour-abundant country A will necessarily export the labour-intensive commodity, clothing. We could, of course, specify what appears to be a less restrictive condition than that set out in the text: for instance, we could sustain the Heckscher-Ohlin theorem by assuming merely that, instead of identical production functions between countries, the differences in the production functions are not large enough to outweigh the effect of differences in factor scarcity on the pre-trade commodity price-ratios. We have preferred to use the strong condition (identity of tastes) instead of the weak one on the ground that the use of the latter seems to be bad methodology, amounting to the argument that the Heckscher-Ohlin definition of factor scarcity will suffice to sustain the Heckscher-Ohlin theorem if other factors do not work to invalidate it.

² The Heckscher-Ohlin theorem would not hold as a logically true proposition in this case unless we also postulate now international identity of tastes (or the weak postulate that differences in tastes between countries do not affect the issue). This follows from the fact that while, with identical production functions, country A will show a bias towards the production of the labour-intensive commodity, clothing, by virtue of her physical abundance in labour, this bias in production may be more than offset by a bias in A towards the consumption of clothing: such that, in self-sufficiency, we find that $(P_F/P_C)_A < (P_F/P_C)_B$ and country A, although physically abundant in labour, would export the land-intensive commodity, food.

³ The phrase "logically true" in the following statements is used in the strict mathematical sense: "A statement that is true in every logically possible case is said to be *logically true*" (Kemeny, Snell and Thompson, *Introduction to Finite Mathematics* (Prentice-Hall, 1957), p. 19.

intensities of commodities between countries, and (e) the assumption of international identity of tastes.

(2) The general Stolper-Samuelson theorem is logically true if we use the further assumption that $n_x > c$.

A tree-diagram, based on this analysis, is presented in Table I.

8. We are now in a position to decide whether Stolper and Samuelson derived their theorems logically. Aside from their basic model:

(1) they adopt, though without complete clarity, the Heckscher-Ohlin definition of factor scarcity and the postulate concerning the non-reversibility of factor-intensities; and, quite explicitly, the assumption of international identity of production functions: ¹ this establishes the restrictive Stolper-Samuelson theorem as logically true (B(1));

(2) they further assume that "the country in question is relatively small and has no influence on the terms of trade"; ² this establishes the general Stolper-Samuelson theorem as logically true (B(2)).

9. No critique of the Stolper-Samuelson formulations can thus be founded on the argument that they are not logically true, given the premises. What we could say, however, is that the theorem should be founded as closely as possible on the *basic* Stolper-Samuelson model alone; and

(1) that, if we use the Heckscher-Ohlin definition of factor scarcity, the assumptions that we find ourselves making about the international identity of production functions and the non-reversibility of factor-intensities to sustain the twin formulations of the Stolper-Samuelson theorem are, on this criterion, *restrictive*; and

(2) that, if we use the Leontief definition of factor scarcity (as we should probably want to since it is, in a sense, the most "objective" definition we could adopt in this context), we discover ourselves adopting the threefold restrictive assumptions (C(1)) of international identity of production functions and tastes plus the non-reversibility of factor-intensities of commodities, to sustain the Stolper-Samuelson formulations.³

10. It will be remembered, however, that these restrictive assumptions were made only because we wished to use argument (3) concerning the validity of the Heckscher-Ohlin theorem.⁴ This may also be seen indirectly from

¹ Stolper and Samuelson, *op. cit.*, pp. 335-40. Some of the argument is, of course, obscure in view of the pioneering nature of the article: a sympathetic interpretation, therefore, is called for. Metzler, *op. cit.*, p. 5, also adopts the Heckscher-Ohlin definition of factor scarcity in discussing the Stolper-Samuelson theorem.

² Stolper and Samuelson, *op. cit.*, p. 346.

³ The additional restrictive assumption that $n_x > c$ has not been listed here because we wish at this stage to concentrate on only those restrictive assumptions which are made to sustain argument (3).

⁴ It is important to remember that these assumptions are restrictive only in so far as we wish to found our theorem exclusively on the basic Stolper-Samuelson model.

TABLE I

Basic Stolper-Samuelson Model									
Definition of factor scarcity	Leontief		Heckscher-Ohlin		Lancaster		Relative values of η_z and c	Does protection necessarily raise the real wage of the scarce factor?	
	Identical	Different	Identical	Different	Prohibitive	Non-prohibitive			
Production functions	Non-reversible	Reversible	Non-reversible	Reversible	Prohibitive	Non-prohibitive	$\eta_z > c$	$\eta_z \leq c$	
Factor-intensity	Identical	Different	Identical	Different	Prohibitive	Non-prohibitive	$\eta_z > c$	$\eta_z \leq c$	
Tastes	Identical	Different	Identical	Different	Prohibitive	Non-prohibitive	$\eta_z > c$	$\eta_z \leq c$	
Tariffs	Prohibitive	Non-prohibitive	Prohibitive	Non-prohibitive	Prohibitive	Non-prohibitive	$\eta_z > c$	$\eta_z \leq c$	
Does protection necessarily raise the real wage of the scarce factor?	Yes	No	Yes	No	Yes	No	Yes	No	

the fact that, if we use the Lancaster definition of factor scarcity, no such restrictive assumptions are necessary (A(1)): for the Heckscher-Ohlin theorem has been rendered valid by definition!

The suggestion follows readily from these considerations that we should reformulate our theorem in terms of arguments (1) and (2) alone, while eliminating the use of the troublesome argument (3). This can be done readily: protection (prohibitive or otherwise) raises the real wage of the factor intensively employed in the production of the importable good. This theorem is logically true if we use: (a) the basic Stolper-Samuelson model, and (b) the assumption that $n_x > c$.

This theorem has been described as the Stolper-Samuelson-Metzler-Lancaster theorem on the following grounds:

(1) It is *implicit* in the Stolper-Samuelson argument, towards the end of their paper: "It does not follow that our results stand and fall with the Heckscher-Ohlin theorem. Our analysis neglected the other country completely. If factors of production are not comparable between countries, or if production functions differ, nevertheless, so long as the country has only two factors, international trade would necessarily affect the real wage of a factor in the same direction as its relative remuneration."¹

(2) Metzler *explicitly* states it as "the Stolper-Samuelson conclusion that tariffs benefit the factors of production which are required in relatively large amounts in the industries competing with imports."²

(3) Lancaster advances this formulation directly as an *alternative* to the Stolper-Samuelson formulations considered above on the ground that it is more general than the latter.

11. Whereas, however, Lancaster's observation that the Stolper-Samuelson formulations are "non-universal" (restrictive) is well taken, the argument by which he supports it is erroneous and different from that set out in this paper. Lancaster proceeds by establishing, with the aid of a highly ingenious model, the proposition that, in the context of the basic Stolper-Samuelson model combined with the assumption of a small country facing fixed terms of trade, differences in demand conditions ("which good is the wage-good") will affect the composition of a country's foreign trade. On this proposition he founds the following critique:

"The non-universality of the [Stolper-Samuelson] theorem is due to incorrect formulation: if the scarce factor is defined as that which is used more intensively in the good of which more is produced in isolation

¹ Stolper and Samuelson, *op. cit.*, pp. 355-6. Homogeneity of factors between countries has not been listed separately as an assumption in this paper because it is believed that this is implicit in both the Heckscher-Ohlin and the Leontief definitions of factor scarcity.

² Metzler, *op. cit.*, p. 13. Metzler, of course, does not state it as a rival formulation, but it is abundantly clear that he is aware that this formulation is implicit in the general Stolper-Samuelson theorem.

than in trade (the only acceptable definition), then the previous analysis has shown that different wage-goods may make for different factor scarcities. In this sense, the Stolper-Samuelson formulation is meaningless, since the phrases 'real wages . . . in terms of any good' and 'scarce factor' represent incompatible concepts."¹

The following comments on Lancaster's critique seem warranted here, in view of our preceding analysis.

To begin with, it is difficult to understand what Lancaster means by the statement that "the previous analysis has shown that different wage-goods may make for different factor scarcities. In this sense, the Stolper-Samuelson formulation is meaningless, since the phrases 'real wages . . . in terms of any good' and 'scarce factor' represent incompatible concepts." Which good will be imported into a country will depend in our model on the pre-trade commodity price-ratios in the trading countries; these price-ratios are determined by domestic supply and demand; and domestic demand is affected by "which good is the wage-good." If the scarce factor good it follows then, from elementary considerations, that "different wage-goods may make for different factor scarcities." But surely, how can this render the Stolper-Samuelson formulations *meaningless* or make 'real wages . . . in terms of any good' and 'scarce factor' *incompatible* concepts? And, more pertinently, why should this make the Stolper-Samuelson formulation "non-universal"?

Indeed, if the tautologous definition of factor scarcity is adopted, as Lancaster suggests, then the general Stolper-Samuelson theorem and the Stolper-Samuelson-Metzler-Lancaster theorem are *identical*: the phrases "scarce factor" and "factor intensively employed in the importable good" can be used interchangeably. Lancaster cannot, therefore, claim one formulation to be "non-universal" and the other to be "universally true": on his own definition of factor scarcity, the two formulations come to the same thing!

To be sure, Lancaster's critique would be valid (though, as we have shown, incomplete) only if the physical, Leontief definition of factor scarcity were proven to have been adopted by Stolper and Samuelson, and were adopted by Lancaster as well; as formulated, however, the criticism is merely erroneous.² In failing to investigate precisely what Stolper and Samuelson assumed by way of their definition of factor scarcity, Lancaster has further by-passed the only legitimate critique that can be sustained against the actual formulation of the theorem by Stolper and Samuelson: namely, that advanced in this paper.

¹ Lancaster, *op. cit.*, p. 208.

² Lancaster has pointed out to me, in private communication, that he really had in mind the physical definition of factor scarcity, despite the printed commitment to the tautologous definition: the tenor of the argument on p. 209, *op. cit.*, seems to suggest this, although it follows upon the nullation of the tautologous definition. None of the criticism advanced here should obscure the fact that Lancaster has handled his model with admirable expertise.

12. Our task is yet incomplete. Even the Stolper-Samuelson-Metzler-Lancaster formulation does not found the theorem completely and solely on the basic model. We must still make the restrictive assumption that $\eta_x > c$. We should, however, clearly want to go the whole way and remove all restrictive assumptions and restate the theorem to include the entire matrix of possibilities: such that the theorem is logically true, given only the basic Stolper-Samuelson model. This formulation is:¹

Protection (prohibitive or otherwise) will raise, reduce or leave unchanged the real wage of the factor intensively employed in the production of a good according as protection raises, lowers or leaves unchanged the internal relative price of that good.

This is really the fundamental theorem that Stolper and Samuelson contributed to our knowledge of the properties of the basic model they were using. Given the basic model, our formulation is logically true for all possible cases.

13. It should perhaps be emphasised that the preceding analysis has been centred entirely on the problem of analysing the impact of protection on real wages of factors in the context of the basic model employed by Stolper and Samuelson. It should be possible, of course, to analyse the problem afresh in terms of models employing alternative assumptions. This, however, would be mostly destructive of the full validity of our theorem.

If we allow for complete specialisation with trade, for instance, we can claim only that protection will raise, lower or leave unchanged the real wage of the factor in which the *exportable* good is postulated to be intensive according as protection raises, lowers or leaves unchanged the internal relative price of the exportable good. But we cannot extend the theorem to the factor postulated to be used intensively in the production, if any, of the importable good because any increase in the internal relative price of the exportable good after complete specialisation must raise the real wage of *both* factors.

However, if we allow the optimum factor-proportions within industries, at given factor price-ratios, to change with scale, our theorem will continue to be logically true and the real wage of the factor intensively employed in a good will rise, fall or be unchanged according as the internal relative price of that good rises, falls or is unchanged with the imposition of protection.²

¹ This formulation stems directly from argument (2), which is founded exclusively, as the reader will remember, on the basic Stolper-Samuelson model.

² An apparent exception to this proposition may be investigated. Where the optimum factor-ratio changes with scale, at given factor price-ratios, it may happen, for instance, that if the production of labour-intensive importables expands, a higher proportion of labour is released than is needed in import-substitution, even though importables are *on average* more labour-intensive. In this case, increase in the production of importables will lead to a *rise* in the labour-to-land ratios, and hence *reduce* the real wage of labour. This case, however, does not constitute an exception to our theorem, because such technology involves a concave production frontier, so that increase in the production of importables occurs when the price of importables *falls* (and not rises). Hence the logical truth of our proposition, even when we allow for changing optimum factor-ratios with scale.

On the other hand, if we allow for changing returns to scale in either or both of the two activities, clearly it becomes impossible to maintain that our theorem will be logically true.

II. PROTECTION AND REAL INCOMES

14. Our analysis has so far been concerned with the original Stolper-Samuelson problem of discovering the impact of protection on the *real wage* earned by factors in employment. It seems useful, however, to emphasise that if we are interested in finding out the net change in the *real income* of the factors it is only in the case of a prohibitive tariff that a complete identity obtains between change in real wage and change in the real income of a factor. Where the tariff is non-prohibitive, the complication arises from the revenue earned by the Government. If this revenue is assumed to be redistributed to the owners of factors according to some formula, factors will derive incomes *both* from the real wage in employment and from the redistributed proceeds of the tariff-revenue.

Hence arises the interesting possibility that the factor whose real wage has been damaged by protection may still find its real income improved if the formula for the redistribution of the tariff-revenue is heavily biased in its favour. Since this possibility constitutes a qualification to the generally accepted implication of the Stolper-Samuelson analysis, it should be of some interest to delimit the conditions under which it may occur.

To begin with, this possibility of over-compensating the damaged factor *from the tariff-revenue* clearly cannot arise unless the real income of the country as a whole is improved by protection. We know from the preceding analysis that where the real wage of one factor is reduced, that of the other necessarily rises; hence, if protection did not bring some gain to the country as a whole, it should be impossible to overcompensate the factor with the damaged real wage (from tariff-revenues). To rephrase the proposition, then, accrual of gain to the protecting country from the imposition of protection is a necessary, though not sufficient, condition for the possibility of over-compensating the factor with the damaged real wage.¹

In the following brief analysis we seek to relate this proposition to Metzler's formula for determining the impact of protection on the internal commodity price-ratio: partly to establish link with Metzler's pioneering analysis in this field and largely because it enables us to define, and distinguish between, situations in which the factor with the damaged real wage will be export-intensive (intensively used in exportables) and those where it will be import-intensive. The discussion is then briefly extended to the

¹ That is to say, whereas the country must have gained from protection before the damaged factor can be overcompensated from the tariff-revenues (necessary condition), this gain must be large enough to permit overcompensation (sufficient condition).

case where the initial situation is that of a tariff instead of free-trade and the effect of an *increase* in protection is the subject of inquiry.

15. In Fig. 2 let O_b be the foreign reciprocal demand curve facing country A . F is the free-trade point, OF yielding the corresponding terms of trade. I_a' is the trade-indifference curve of A passing through F at a tangent to OF and intersecting O_b at U . Its postulated curvature derives from

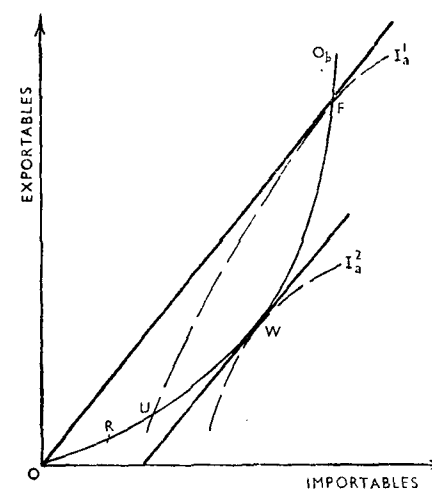


FIG. 2

the assumption of strict convexity of the production frontier and community indifference curves.

(1) Assume that the tariff-added offer curve of country A intersects O_b at U . The internal relative price of the importable good is then given by the slope of the trade-indifference curve I_a' at U , which is clearly, by virtue of the postulated curvature of I_a' , greater than at F . We can deduce, therefore, that protection can leave the real income of the country unchanged only if the internal relative price of the importable good rises from the free-trade level with the imposition of protection (in turn, only if $\eta_x > c$).

(2) Similarly, by considering points on O_b to the left of U such as R , we can argue that protection can reduce the real income of the country only if the internal relative price of the importable good rises with protection (in turn, only if $\eta_x > c$).

(3) However, protection can increase the real income of the country whether the internal relative price of the importable good rises, is unchanged (W) or falls with the imposition of protection (in turn, whether $\eta_x \geq c$).

Thus, where $\eta_x \leq c$, the real income of the country will necessarily improve with the imposition of a tariff; whereas if $\eta_x > c$, the real income may rise, fall or be unchanged.

16. Where the comparison is confined to the real income and real wage of the factors in an initial free-trade position and after the imposition of a tariff, we can then conclude as follows:

(1) the export-intensive factor will necessarily become better off and it may be possible to overcompensate the import-intensive factor if $\eta_x < c$; ¹

(2) neither factor will become worse off and at least one better off if $\eta_x = c$; ² and

(3) the import-intensive factor will necessarily become better off and it may be possible to overcompensate the export-intensive factor if two conditions obtain: (i) $\eta_x > c$, and (ii) the tariff is small enough to yield some gain to the country.

If we assume that importables are not inferior goods in the protecting country, it is clear, then, that inelastic foreign demand ($\eta_x < 1$) is a necessary, though not a sufficient, condition for the emergence of the possibility of overcompensating the import-intensive factor. Where, however, foreign demand is elastic and importables are not inferior goods, the export-intensive factor will necessarily find its real wage reduced by protection; and, for the possibility of overcompensating it to arise, it will be necessary, though not sufficient, that the tariff be small enough to make the country better off than under free-trade.

17. If, however, we wish to compare the real incomes and wages of factors in an initial situation of a tariff and after *increase* in the tariff, the analysis must be somewhat modified.

To begin with, the Metzler formula must be altered so as to read: the internal relative price of the importable good will rise, be unchanged or fall according as $\eta_x \geq \frac{1 - c'(1+t)}{1 - c't}$ where t is the initial tariff-rate and $c' = (1 - c)$ is the domestic marginal propensity to consume importables.³

¹ When $\eta_x < c$ we know now that: (1) the internal relative price of the importable good falls, thus increasing the real wage of the export-intensive factor and reducing that of the import-intensive factor; and (2) the country must have become better off. Hence the proposition in the text.

² Where $\eta_x = c$, we know that: (1) the internal relative price of the importable good is unchanged, thus leaving unchanged the real wages of both factors; and (2) the real income of the country must increase. Hence the proposition in the text.

³ This formula is derived from the following analysis, furnished by Professor Johnson and replacing my earlier, unsatisfactory attempt.

Symbols: Let p be the *external* terms of trade, measured as the price of the importable good over the price of the exportable good, t the initial tariff rate and $\pi = p(1+t)$ the *internal* terms of trade. c' is the marginal propensity to consume importables, at the initial terms of trade. C is the initial

domestic consumption of importables, Y their production and $M \equiv C - Y$ the initial quantity of imports. $r_m = (\eta_x - 1) = \frac{p}{M} \cdot \frac{\delta Sm}{\delta p}$ ($Sm \equiv M$) is the elasticity of foreign supply of importables; and $R = tpM$ is the tariff revenue.

Analysis: The simplest approach to the problem is to assume the internal terms of trade to be constant and to investigate the excess demand for importables when t changes. If the excess demand is positive, the internal terms of trade will rise to restore equilibrium; if negative, they will fall: assuming, of course, that we have "well-behaved" substitution elasticities in production and consumption.

Now, $R = tpM = \frac{t}{1+t} \pi \cdot M$. With π constant, we have $\frac{dp}{dt} = -\frac{p}{(1+t)}$. The shift in the demand for imports due to the change in the tariff is then given by:

$$\frac{dC}{dt} = c' \frac{dR}{dt} = c' \pi \left\{ \frac{1}{(1+t)^2} M + \frac{t}{(1+t)} \cdot \frac{dM}{dt} \right\}$$

The shift in the supply of imports is given by:

$$\frac{dM}{dp} \cdot \frac{dp}{dt} = \frac{M}{p} \cdot r_m \cdot \frac{dp}{dt} = -\frac{M}{(1+t)} \cdot r_m$$

The excess demand for importables is then given by:

$$\begin{aligned} & \left(\frac{dC}{dt} - \frac{dM}{dt} \right) \\ &= \frac{c' \pi}{(1+t)^2} \cdot M + \left\{ \frac{c' \pi t}{(1+t)} - 1 \right\} \left\{ \frac{-M}{(1+t)} \right\} r_m \\ &= \frac{M}{(1+t)} \{ c' p + (1 - c' p t) r_m \} \end{aligned}$$

Substituting $r_m = \eta_x - 1$, assuming p to be unity initially by choice of units and simplifying, we arrive at the formula that the excess demand for importables will be positive, zero or negative according as:

$$\eta_x \geq \frac{1 - c'(1+t)}{1 - c't}$$

It will be seen that this formula reduces to the Metzler formula when t is zero.

These considerations lead to the following conclusions:¹

(1) The export-intensive factor will necessarily become better off and it may be possible to overcompensate the import-intensive factor from *increased* tariff-revenues when three conditions obtain:

(i) $\eta_x < \frac{1 - c'(1+t)}{1 - c't}$; (ii) $t < \frac{1}{\eta_x - 1}$; and (iii) *either* the post-increase tariff rate is also less than the optimum tariff *or*, if it exceeds the optimum tariff, it is still small enough to leave some gain in real income to the country from the increase in the tariff.

(2) The import-intensive factor will necessarily become better off and it may be possible to overcompensate the export-intensive factor from *increased* tariff-revenues when three conditions obtain:

(i) $\eta_x > \frac{1 - c'(1+t)}{1 - c't}$; (ii) $t < \frac{1}{\eta_x - 1}$; and (iii) *either* the post-increase tariff rate is also less than the optimum tariff *or*, if it exceeds the optimum tariff, it is still small enough to leave some gain in real income to the country from the increase in protection.

(3) Where, however, $\eta_x = \frac{1 - c'(1+t)}{1 - c't}$, the real wage of neither factor changes with the increase in protection. It follows, therefore, that the real income of both factors will increase, decrease or remain unchanged according as the increase in tariff raises, lowers or leaves unchanged the real income of the country: assuming, of course, that the tariff-revenues are divided among the factors in a given proportion.

18. In conclusion, it should be re-emphasised that the brief discussion presented here has been concerned only with the limited task of exploring some of the implications of the proposition that accrual of gain to the protecting country from the imposition of protection is a necessary, though not sufficient, condition for the emergence of the possibility of overcompensating, from tariff-revenues, the factor with the damaged real wage. It is planned to present a rigorous analysis of the sufficient conditions for the emergence of this possibility in a subsequent article.²

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¹ The first two propositions that follow assume that the factor stated to become necessarily better off continues to receive *at least* the same revenue as in the initial situation; this assumption being made explicit by the use of the phrase "from *increased* tariff-revenues." This assumption is needed because otherwise improvement merely in the real wage of a factor due to increased protection could be offset by an accompanying unfavourable distribution of tariff-revenues to the factor after the increase in the tariff.

² This calls for a rigorous analysis of distribution and demand, so that factor earnings and income subsidies from tariff-revenues could be related to real incomes of factors. Such analysis would preclude us from taking as given, as we have done here, the set of community indifference curves: this practice has been adopted in the present paper for strictly pedagogic reasons.

DUESENBERY ON GROWTH AND FLUCTUATIONS

I

IN the theoretical models of long-run growth in mature capitalist economies which have proliferated in recent years, two different streams of thought may be distinguished, going back to two different sources. The first of these sources is the theory of distribution. Models such as those of Mrs. Robinson and Mr. Kaldor¹ hark back to the classical economists and to Marx in relating the analysis of growth closely to the analysis of the forces determining the distribution of income between labour and capital. The relation between distribution and growth in these models arises partly from the mutual interdependence of the rate of profit and the pace of capital accumulation and partly from the effect of the distribution of income on the proportion of income saved. The other source is the theory of the trade cycle. It is in trade-cycle theory that the growth models of Dr. Goodwin and Professor Smithies,² for example, have their origins. The relation between trade-cycle theory and growth theory is that trade-cycle theory is concerned with what determines the level of demand in the short period, and this provides a possible approach to the explanation of what determines the long-run movement of income on the demand side.

The difference between these two approaches is partly a matter of substance and partly a matter of emphasis and selection—of what one chooses to explain by the model and what one treats as extraneous to it. Thus growth models originating in trade-cycle theory tend to treat as exogenously determined the "normal" (in the Marshallian sense) rate of profit on capital, whereas in distribution-oriented models the possibility of systematic long-run changes in the profit rate are a central theme. On the other hand, distribution-oriented theories commonly fail to give any very explicit account of the way in which fluctuations occur around the growth path, whereas models developed from analysis of the cycle are naturally more exact on this point.

The model of economic growth put forward by Professor Duesenberry in his important and distinguished new book³ belongs to the second of the

¹ Joan Robinson, *The Accumulation of Capital* (1956); N. Kaldor, "Alternative Theories of Distribution," *Review of Economic Studies*, 1955–56, pp. 83–100, and "A Model of Economic Growth," *Economic Journal*, 1957, pp. 591–624.

² R. M. Goodwin, "The Problem of Trend and Cycle," *Yorkshire Bulletin of Economic and Social Research*, 1953, pp. 89–97, and "A Model of Cyclical Growth," in *The Business Cycle in the Post-War World*, edited by E. Lundberg (1955), pp. 203–21; A. Smithies, "Economic Fluctuations and Growth," *Econometrica*, 1957, pp. 1–52.

³ J. S. Duesenberry, *Business Cycles and Economic Growth* (New York: McGraw-Hill, 1958, Pp. xi + 341. 50s. 6d.).

two types referred to. It springs from trade-cycle theory in that the concepts and relationships it uses are primarily those that have figured in trade-cycle theory rather than in the theory of distribution. But unlike most growth models that spring from this source, Duesenberry's model does not in its basic form yield a system of growth and fluctuations. Instead it yields a system of steady growth. Fluctuations are held to be due to causes extraneous to the main process of growth and are separately explained. This is the first distinctive feature of the model. The second is its treatment of the relative importance of demand and supply. A central topic in any growth model is the relation between the rate of growth of demand and the rate of growth permitted by population increase and technical progress—Harrod's natural rate of growth. By what mechanism, if any, are these two brought into alignment? Duesenberry takes his stand firmly on the side of demand as the prime determinant of the rate of growth. In normal circumstances factor limitations are held not to impose any significant constraint. Either there are plenty of resources or else the natural rate of growth adjusts itself to the requirements of demand.

In what follows we shall be concerned with discussion and criticism of the main outline of Duesenberry's model of growth (Sections II–IV, VI) and of fluctuations (Section V). In the course of building up this model Duesenberry provides much closely packed, original and at times controversial analysis of such topics as oligopoly, the effect of uncertainty on investment decisions, the supply of finance, investment in residential construction and the labour market. This analysis is conducted mainly in *a priori* terms, but with frequent supporting reference to empirical data on the American economy. Interesting as this material is and noteworthy as a contribution to the study of the topics concerned, large parts of it are not essential to the main purposes of the model, and will therefore not here be discussed.

The prime aim of the model is to explain the historical experience of the United States since the Civil War. The analysis is therefore conducted almost exclusively in terms of a closed economy. Moreover, the economic activities of government are not brought into the main model, though some indication is given of the way in which they will impinge in theory and have impinged in practice during the past twenty years.

II

We will begin by summarising the model.

The level of investment in any firm, and hence in the economy, is determined by the intersection of a marginal efficiency of investment schedule and a supply of funds schedule. The marginal efficiency of investment schedule is based ultimately on the idea underlying the acceleration principle, viz., that the marginal efficiency of investment is an increasing function of income (Y) and a diminishing function of the stock of capital (K). How-

ever, this is interpreted with a good deal of qualification to take account of uncertainty, indivisibilities, oligopoly, special factors affecting housing and so on. Moreover, it is held that even in the fairly short period there is a good deal of scope for varying the capital intensity of production, and this causes the marginal efficiency of investment schedule corresponding to a given level of income and capital stock to be fairly elastic.

The supply of funds schedule facing a firm is strongly affected by the imperfection of the capital market and by the principle of increasing risk. As a result, it is relatively inelastic, at least over certain ranges, and this serves to lessen the effect on investment of a shift in the marginal efficiency of investment schedule. Of the various sources of funds, internal finance has the lowest opportunity cost, up to the point where shareholders become restive; borrowing is the next cheapest, up to the point where the principle of increasing risk becomes important—where this point is depending on the size of the firm's initial debt in relation to its own capital, and also on the riskiness of the particular industry concerned; finally, there is the issue of equities, the cost of which, as measured by earnings yields, is the highest of the three. Duesenberry does not give any extended treatment to the possibility of systematic shifts over time in the forces affecting the supply of funds schedule, such as changes in firms' views as to the acceptable rate of return on their own funds, or as to the acceptable ratio of retained to total profits. In this sense the theory remains within a short-period framework and treats as given many of the considerations relevant to a long-period theory of distribution.

However, since the supply of funds is affected by the level of profits, some theory of distribution is needed, and this is duly provided. It is essentially a short-period theory, based on the doctrine that profits' share in income will rise if there is an increase in the degree of utilisation of capital (Y/K), (a) because the relative share of overhead factors, such as salaries, pensions and debt interest, if any, will fall, (b) because mark-ups over direct cost will tend to increase.¹ (The actual size of profits' share in income corresponding to any given degree of utilisation will be affected by the extent of competition as well as by the structure of cost.²) The degree of utilisation itself is determined by the working of the system as a whole.

The investment function that is yielded by bringing together all these considerations is in the first instance quite complicated. On the marginal efficiency side, investment is a function of Y and K (houses and other capital being entered separately), and on the supply of funds side it is a function of a number of variables, including profits, debt, depreciation allowances and retained earnings. However, most of the financial variables turn out to be themselves functions of profits, past or present, and profits are a function of Y and K (since profits' share in income depends on the degree of utilisation).

¹ Duesenberry, pp. 102, 261.

² *Ibid.*, p. 216.

So by a certain amount of simplification and by assuming linear relationships we arrive at the following familiar-looking equation

$$I_t = \alpha Y_{t-1} + \beta K_{t-1} \quad (1)$$

Subscripts denote time-periods ($K_{t-1} = K$ at end of period $t-1$). α and β are coefficients of a catch-all character whose numerical value will be determined by the parameters in the marginal efficiency and supply of funds schedules. α will be positive and β negative.

This equation looks like no more than the generalised acceleration principle, and so indeed it is formally. But its interpretation is rather different. In the pure acceleration principle α in the above equation would represent the capital-output ratio aimed at by entrepreneurs and β would be minus unity: the postulate being that investment adds such an amount to the initial capital stock as will bring the capital-output ratio to the desired level (on the assumption of no change in income). In more generalised versions of the acceleration principle¹ account is taken of expectations, indivisibilities, time-lags and other such complications, so that although I_t is still a function of Y_{t-1} and of K_{t-1} , the parameters α and β are affected by a wider range of influences, and are not necessarily equal to the aimed-at capital-output ratio and -1 respectively. In Duesenberry's model the relationship is affected not only by these complications but also by the supply of funds. Thus because funds are in inelastic supply to the firm, the coefficient α , which measures the amount by which investment will change in consequence of a given change in income in the previous period (given the initial capital stock), will be much smaller than it would be in a pure acceleration principle model: a large rise in income may greatly raise the marginal efficiency of investment, but the amount by which investment can actually rise will be severely restricted by the supply of funds. (One result of this is that an initial deficiency of capital may take quite a long time to make up, and any minor shock-induced recession will therefore be cushioned by the backlog of investment opportunities.) This is partially offset by the effect of an increase in income in raising profits, and hence the availability of internal finance—this effect is held to contribute substantially to the value of α —but none the less α is expected to be well below unity.

The above investment function is combined with a fairly straightforward consumption function. Personal consumption out of disposable income is held to be a function of lagged income and lagged consumption, the latter to reflect the inertia of consumption habits, but in the first approximation this is simplified by omitting the lagged consumption term. Consumption is also affected by the distribution of income, since the corporate propensity to retain earnings is higher than the personal propensity to save and is affected by different considerations. Since the distribution of income

¹ Cf. for example H. B. Chenery, "Overcapacity and the Acceleration Principle," *Econometrica*, 1952, pp. 1-28.

depends upon the degree of capital utilisation, the level of consumption can be given by an equation similar in form to the investment equation above, namely

$$C_t = aY_{t-1} + bK_{t-1} \quad (2)$$

The parameters a and b here are catch-all coefficients measuring the net effect of Y_{t-1} and K_{t-1} on C_t through the several channels mentioned. The parameter a may be thought of as the marginal propensity to consume out of the previous period's income that applies if K is constant. Both a and b are positive. (The higher K_{t-1} , given Y_{t-1} , the smaller the share of profits in income, and hence the smaller the proportion of income saved.)

Further to these two equations we have the identities

$$Y_t = C_t + I_t \quad (3)$$

and

$$K_t = (1 - k)K_{t-1} + I_t \quad (4)$$

(k = the proportion of capital stock disappearing through capital consumption in each period.)

Substitution between these four equations leads to the following

$$Y_t = [(\alpha + a) + (\beta + 1 - k)] Y_{t-1} + [\alpha(\beta + b) - (\alpha + a)(\beta + 1 - k)] Y_{t-2} \quad (5)$$

This equation describes the path of income through time. It stands at the centre of the whole model. It involves certain important simplifications in that it neglects various lags, does not distinguish residential construction from other investment and is cast in aggregative rather than sectoral form, and these simplifications can be removed one at a time at the appropriate stage of the argument. However, the general working of the model can be sufficiently seen from the equation.

A second-order difference equation of this general form is what is used in the models of Hicks and others to yield not growth but a cycle. But, as is well known, this form of equation may yield several totally different patterns of behaviour according to the values of the parameters. One range of values will lead to damped or anti-damped cycles around a constant average value (cycles around a rising trend are not a possibility); another to convergence to stationary equilibrium; and another to explosive growth at what ultimately becomes an exponential rate. In the latter case the growth rate is stable in response to shocks in the upward direction; in response to downward shocks it is stable so long as the shocks do not exceed a certain crucial magnitude, but if they do exceed this magnitude the explosive growth will turn into explosive contraction.

The essence of Duesenberry's theory is that the parameters have in fact been within the range that leads to exponential growth and that the process so described has lain at the heart of the economic expansion actually ex-

perienced in the United States.¹ The exact values of the parameters have been what determined the rate of growth attained.

The possibility of deriving a model of steady growth from a second-order difference equation had not entirely escaped previous writers.² These writers encountered the following snag, however. In models based on the acceleration principle, if the parameters are such as to yield growth rather than cycles or convergence, the rate of growth per period tends to come out quite unrealistically high in relation to any reasonable definition of a "period" in terms of calendar time. In some models it is actually impossible to get moderate growth,³ with others it can result only from very unlikely values of the acceleration coefficient or the propensity to save. Duesenberry's model avoids this difficulty because its assumptions are more complicated. Investment is determined not only by the forces described by the acceleration principle but also by the supply of funds. As a result α and β will be much lower than in an unqualified acceleration-principle model. Moreover, the term b in his model—which is absent from simpler models—helps to avoid cyclical solutions and so enlarges the range of possible rates of exponential growth. Hence it is possible with reasonable values of the parameters to get a solution which shows a rate of growth that is not grossly excessive in relation to historical experience and to what is physically possible.

III

It will be observed that equation (5) does not contain any term reflecting population growth or technical progress. The question arises, therefore, what forces, if any, bring the rate of growth that emerges from the model into line with the natural rate of growth. In particular, what happens if the rate of growth yielded by the model exceeds the natural rate of growth, so that growth is obstructed by shortage of resources? Duesenberry makes several points in answer to this.

(1) He does not think it likely that the parameters will be such as to

¹ Whether the process of growth in other capitalist countries has been of basically the same nature as in the United States is an issue not explicitly discussed. The contention would presumably be that it has in very broad terms, but with rather substantial qualifications due to the larger role of international trade.

² Cf. S. S. Alexander "The Accelerator as a Generator of Steady Growth," *Quarterly Journal of Economics*, 1949, pp. 174-97.

³ Consider, for example, the very simple model where

$$I_t = v(Y_{t-1} - Y_{t-2})$$

$$\text{and} \quad Y_t = \frac{1}{s} I_t$$

$$\text{so that} \quad Y_t = \frac{v}{s} (Y_{t-1} - Y_{t-2}).$$

Here if v/s is less than 4 in numerical value the system yields fluctuations without growth; if, on the other hand, v/s exceeds 4, it yields growth at not less than 100% per period.

yield a very high rate of growth. This point will be reverted to in Section IV below.

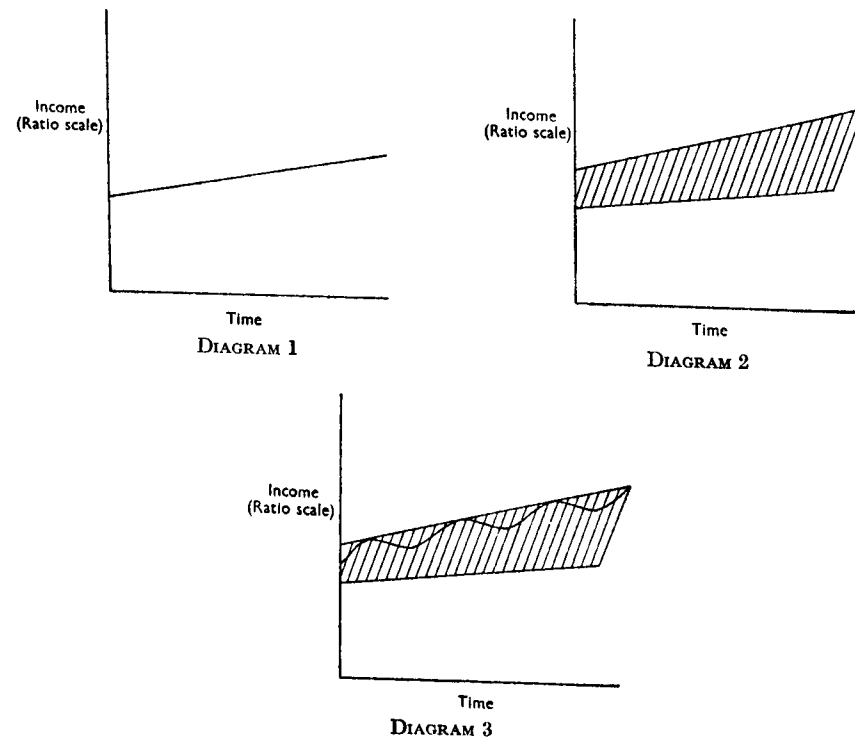
(2) Population growth and technical progress will indirectly affect the rate of growth of the system in so far as they affect the size of the parameters. This they are likely to do in a number of ways, in particular: (a) technical progress in the form of new industries will tend because of indivisibilities to increase the extent to which capacity is built ahead of demand and so will raise investment—this is held to have contributed significantly to the growth of the United States economy; (b) population growth by lowering the rate of growth of real income per head for any given rate of growth of total income will raise the consumption-income ratio and will also raise the proportion of consumption expenditure devoted to the capital-intensive item housing. These and certain other similar influences tend in the direction of accommodating the rate of growth yielded by the model to the natural rate of growth. But the relation is indirect and there is no necessary tendency for a given change in the natural rate of growth to lead to an equal change in the actual rate of growth. It might be much more or much less.

(3) The natural rate of growth is itself flexible to some degree. If the economy is pressing against the limit of resources, the supply of resources will tend to respond in one or other of the following ways: (a) immigration will be encouraged; (b) labour force participation will be increased (this has a long-period aspect as well as a short-period one, e.g., labour shortage may contribute to a gradual change in institutions and attitudes affecting the employment of women); (c) labour will be encouraged to move from low-productivity sectors, especially agriculture, to high-productivity sectors, since frictions in the labour market inhibit this movement unless there is excess demand for labour in the high-productivity sectors; (d) employers may be stimulated to improve efficiency, especially in low-productivity sectors. Some of these represent a permanent increase in the natural rate of growth; others a once-for-all increase in the supply of resources. But even if the increase is once-for-all it may take many years to exhaust if it is a large one and if the excess of the actual rate of growth over the natural rate of growth in the initial situation was not large.

What is being contended under (3) is thus that the natural rate of growth is not a single path as in Diagram 1 but rather a band as in Diagram 2. An actual rate of growth falling anywhere within the band can be accommodated without a manifest increase or decrease in unemployment. Under (2) it is contended that if the band is steeply inclined upwards this will have some tendency to cause the actual rate of growth also to be high. Under (1) the contention is that the parameters are not likely to yield a rate of growth greatly in excess of the upper limit of the band.

Finally, however, it is admitted that notwithstanding these considerations it could happen, at least in principle, that the system tended to yield a rate of growth higher than the maximum physically possible. In this event

Duesenberry maintains that the system would bounce off the ceiling in a series of *mild* cycles, as shown in Diagram 3. It will be noted that apart from the mildness of the cycles, on which see below, pp. 761-3, this result is substantially identical to that of the Goodwin model. Duesenberry does not, however, regard this as a really likely or important contingency. If admitted as the typical case it would, of course, imply a very radical change



in the interpretation of the system's working, since the average rate of growth actually achieved would depend not on the parameters in the income-generation equation, as in the normal case of Duesenberry's model, but on the maximum value of the natural rate of growth.

IV

We come now to the main and most obvious criticism of this model of growth.

As we have seen, because the model is not based merely on the acceleration principle, it is not open to the objection that plausible values of the parameters are incapable of yielding moderate growth. (Indeed, because of the multiplicity of forces affecting the parameters it is not easy to say anything very definite about what *are* plausible values of the parameters.) But granted that there is no inherent reason why the parameters should not

yield moderate growth, it does not follow that they actually will. For the model to provide an acceptable account of reality, the parameters must fall within a certain range, so as to avoid, on the one hand, non-growth solutions and, on the other hand, growth at an excessive rate. This is *prima facie* rather restrictive and lays the model open to the objection that the parameters are unlikely to have stayed around the same level for the space of a hundred years or so. This is similar to the objection brought against linear cycle models that unless recourse is had to shocks they can explain the persistence of cycles of roughly constant amplitude only if the parameters have a certain unique value. In the present case the parameters do not have to have a unique value, so the objection is less strong; they merely have to fall within a certain range. The force of the objection therefore turns very much on how wide this range can in fact be without giving unacceptable results.

Whatever the answer to this may be, it is certainly a gap in Duesenberry's exposition that he does not at any stage come to grips with this problem. The reasons he puts forward at various points for doubting that the rate of growth is likely to be "very high" are practically all put in non-quantitative form. No numerical examples are given to show the orders of magnitude that may be involved, either in the working of the main equation or in the working of the various lags and qualifications it is subject to. The same applies to the treatment given to the effect of structural changes; ingenious and interesting as much of it is, it is essentially non-quantitative.

The table annexed shows the results of some simple arithmetical experiments done to test the issue. What we do here is to hold constant the values of four of the five parameters in equation (5) and then see how widely the fifth parameter α , which measures the responsiveness of I_t to a change in Y_{t-1} , is capable of varying without giving unacceptable results. Values are chosen for the four parameters a , b , β , and k which appear reasonable in themselves and not such as actually to preclude moderate growth. If moderate growth is to result, the lower limit of the rate of growth (r) is clearly zero. Where we place the upper limit—the limit beyond which growth is excessive—depends on the duration of the "period" in calendar time. This is an empirical matter, depending on the duration of the relevant reaction lags, etc. Duesenberry himself reckons that the "period" will be in the region of six months. On this basis a value of r of 0.05,

a	β	b	k	α		
				$r = 0.0$	$r = 0.05$	$r = 0.1$
0.75	-0.1	0.05	0.1	0.333	0.375	0.420
0.8	-0.1	0.05	0.1	0.267	0.313	0.360
0.8	-0.2	0.05	0.1	0.400	0.438	0.480
0.8	-0.2	0.1	0.1	0.300	0.350	0.400
0.8	-0.2	0.1	0.2	0.267	0.321	0.375

representing an annual rate of growth of slightly over 10%, may perhaps be taken as a reasonable maximum. But to give the benefit of the doubt, the table also shows the values of α yielding $r = 0.1$, which would represent an annual rate of growth of 21% on the basis of a six-month period.

This table does appear to suggest that there is only a fairly narrow range within which α is free to vary, given the other parameters. It also illustrates the sensitivity of the rate of growth to changes in the other parameters: e.g., if $\alpha = 0.333$, $\beta = -0.1$, $b = 0.05$ and $k = 0.1$, then the table shows that if the marginal propensity to consume $a = 0.75$, r is zero, while if $a = 0.8$, r is between 0.05 and 0.1 (0.07 approximately).

This is a test in terms of *structural stability*—the ability of the model to withstand changes in its parameters without its performance undergoing a major transformation. The result is not very favourable. But of course a test in these terms is by no means decisive (even apart from the obvious fact that the table given covers only a certain range of values of a , β , b and k). In the first place, a change in circumstances which alters one parameter is quite likely to alter one or more of the others as well, and also perhaps the natural rate of growth. This may work in such a way as to help. In the second place, equation (5) is only a first approximation, and needs to be qualified as a description of reality, particularly by certain additional lags. These lags have the effect of damping down the rate of growth, and they operate more strongly the higher the rate of growth tends to be.¹ They thus serve to raise the value of α yielding the maximum plausible rate of growth, while also raising, but to a less extent, the value of α yielding the minimum rate of growth; the range of values of α that will yield acceptable results is thus enlarged. In assessing these limitations on the test, however, note must be taken of the ease with which the unqualified model can involve absurdly high rates of growth or else fail to yield growth at all. If the qualifications referred to are to endow the system with an adequate degree of structural stability, their effect must be pretty strong.

Duesenberry is aware that his model implies rather serious restrictions on the values of the parameters. (He appears to be more concerned with the possibility of non-growth solutions than with the possibility of growth at an excessive rate.) The reply he offers carries conviction only on the assumption that no alternative type of growth model can be devised. He writes (pp. 238–9):

“It may be said that it is a remarkable coincidence that out of all conceivable savings functions, profits functions, and so on, the ones which came into existence in the nineteenth century were so well adapted to the rates of population growth, development of new industry, and other characteristics of that century. One can only say that, if they had not been, capitalism would never have survived so long.”

¹ Duesenberry, pp. 211, 278.

V

We come now to the theory of fluctuations. Duesenberry suggests that there are in principle two quite different ways in which depressions could occur. In the first place, the parameters of the system might temporarily or permanently become such as to yield a non-growth solution. Recovery from this sort of depression would be problematical. In the second place, depressions might come about from causes such as speculation and financial crises that are extraneous to the main dynamics of growth. Duesenberry maintains that all depressions in the United States since the Civil War, with the possible exception of the 1930s, have been of the second type. He thus reverts to a rather old-fashioned kind of view and denies that any form of the multiplier–accelerator interaction (referred to as the capital-adjustment process) has been responsible for historical fluctuations. This contention is based both on empirical observation of certain characteristics of past depressions—notably the absence of regular periodicity—and on *a priori* reasoning as to the unlikelihood of major depressions resulting from the normal process of growth and capital accumulation, so long as the underlying parameters are such as to permit growth at all.

It may be noted that some such theory of fluctuations is called for by the theory of growth discussed above, since if the capital-adjustment process is to be used to explain growth it cannot also explain fluctuations. The converse does not apply: it would be possible to have this theory of fluctuations, but some different theory of growth.

Duesenberry does not deny that capital adjustment elements play some part in fluctuations and act as a destabiliser. What he does deny is that these elements have been or could be by themselves responsible for major depressions. He also denies that they provide a satisfactory explanation of turning points.

We may consider first the upper turning point. Any realistic treatment of historical fluctuations must give a good deal of emphasis to shocks and disturbances that are random from the capital-adjustment point of view. Duesenberry's brief sketch of the history of fluctuations in the United States since the Civil War performs a valuable service in showing the great diversity of the various depressions within this period, both in severity and in apparent causes. The misleadingness of any purely mechanical model as an account of reality is plain. The question remains, however, whether the tendency for the stock of capital to rise faster than the level of output, which is the basic cause of the downturn in capital adjustment models and which Duesenberry denies, has not in some form or other been present at a sufficiently large proportion of upper turning-points for it to rank as a systematic element in the explanation of the downturn, even though it has sometimes been heavily overlaid with other factors and even though the tendency to fluctuations from this cause has in certain periods been largely

damped out. Duesenberry's contention appears to be (p. 244) that only a small percentage change in the ratio of capital to output can be brought about by the investment of a single year or two, and that such a small change could not initiate such sharp reversals as have been experienced. But it is surely impossible to say *a priori* how large a change in the capital-output ratio is needed to cause a sharp reversal. It is marginal adjustments that matter, and changes which are not large when averaged over the economy as a whole may substantially affect the inducement to invest. It is true that many if not most depressions have been triggered off by spectacular events in the financial sphere rather than by any manifest tendency for capital accumulation to outstrip the rate of growth of income. But it is always open to question whether the crash was not a reflection of a more deep-seated tendency to capital saturation. This is recognised by Duesenberry, and he also concedes that the conditions needed for a financial shock to cause a severe contraction will not normally be satisfied until some years have elapsed since the last one, thus producing a limited element of regularity in timing. In other circumstances shocks may occur without causing any serious general contraction.

With regard to the interpretation of individual upper turning-points, it may be observed that while there is nothing in the capital-adjustment theory of the cycle to imply that the downturn will be reached simultaneously in all industries, slumps caused by capital saturation in particular lines, *e.g.*, railway-building or house-building, are none the less often better regarded not as a manifestation of the capital-adjustment process but as the result of the exhaustion of a particular batch of investment opportunities, the size of which, in the short run at least, does not depend much on the level of income. The doctrine that slumps of this sort recur systematically is essentially Schumpeter's theory of the cycle. Duesenberry attaches a good deal of importance to non-maintainable bouts of investment in particular industries, but he treats them as falling into the category of shocks, and Schumpeter's views are not mentioned. Admission of a systematic cyclical tendency of the Schumpeterian type in innovatory investment or some classes of it would on the face of it strengthen Duesenberry's model rather than weaken it, since such cycles can without inconsistency be superimposed on an upward trend due to the capital-adjustment process.

Let us consider now the lower turning-point. The difficulties experienced by capital-adjustment models in explaining this phase of the cycle are well known, and Duesenberry states them very cogently. The crucial difficulty is the non-occurrence in all historical slumps except the 1930s of the negative net investment which the models appear to require. In Duesenberry's model, where the original downturn is explained as due to a shock, the difficulty of explaining the recovery is much less, for in the nature of things the shock is likely to reverse itself presently—*e.g.*, after a financial crisis, confidence in the banking system will presently be restored—and this

will provide a stimulus to recovery. This stimulus will be effective in actually bringing about a recovery so long as the cumulative forces of contraction have not in the meanwhile carried the system down too far. Duesenberry analyses with much ingenuity the conditions which on his model determine whether or not this will be the case.

It would be generally agreed that recessions caused by shocks will be terminated sooner and more easily than recessions caused by partial or general capital saturation.¹ But is the difficulty of recovery in the latter case so great that only the depression of the 1930s fits the description, as Duesenberry suggests? In facing this well-known crux, all one can do is to mention such factors as sectoral disinvestment, especially in inventories, sectoral floors and the less obvious form of negative net investment that consists of proceeding abnormally slowly with innovatory investment, and suggest that, when they are taken into account, it is not impossibly difficult to explain how recovery occurs from a capital-saturation slump without total net investment becoming negative.

An important implication of Duesenberry's theory of the cycle is that so long as the parameters are such as to permit growth there is no inherent *need* for the economy to suffer severe slumps. If speculation and financial disturbances can be avoided, steady growth is possible without cycles. This applies both to the past and to the future. For example, had there been stronger financial institutions and less speculation between the Civil War and 1914, slumps would then have been avoided. Growth and capital accumulation would, on the average, have proceeded rather faster than they did.² In the absence of slumps the rate of growth and the rate of capital accumulation would have been at most, say, 25% higher on the average than they actually were. Whether growth at this rate would have been possible without running into a ceiling depends on how much flexibility there was in the natural rate of growth. Flexibility of the order of 25% does not in the circumstances of that time appear an unreasonable postulate.

The consequences of impact with the ceiling have, however, a more general significance to the theory. As mentioned earlier, Duesenberry holds that if the growth of the system were at any time so rapid as to bring it up against an inflexible ceiling on the supply side, the result would be a mild recession rather than a severe one and that impact with the ceiling can therefore not serve as the explanation of the more severe depressions experienced in history. This conclusion appears to depend on a particular interpretation of the way the ceiling works, though this is not expressly stated. This interpretation is that impact with the ceiling has similar effects to an unfavourable but temporary shock. Impact with the ceiling

¹ Cf. R. A. Gordon, "Investment Behaviour and Business Cycles," *Review of Economics and Statistics*, 1955, pp. 23–34.

² Capital accumulation would not, however, have been quite so high as it was at the cyclical peaks, since speculation raises investment abnormally in the boom as well as depressing it in the slump.

tends to lessen the rate of growth of income, and thus may lead to actual contraction; but once sufficient slack in the system has reappeared the depressing influence of the ceiling will cease to be felt, and this will act as a positive stimulus—a reversal of the initial shock—which will carry the economy back into its normal growth path. Moreover, recovery can be relied upon to happen more promptly than after some other kinds of shock, since *ex hypothesi* the depressing influence of the ceiling will cease to be felt before the contraction in income has gone very far.

This is not quite the way in which the ceiling has been looked upon in most models of the cycle where it has been invoked. As a rule the ceiling is regarded as a constraint rather than as an actual deflationary shock—a cause of non-linearity in the investment schedule rather than of a downward shift in the schedule. Therefore, when the pressure of the ceiling ceases to be felt because income has fallen, it does not act as a positive stimulus. On this reckoning a ceiling-induced recession will not have any particular tendency to correct itself quickly. Suppose that the parameters of the system are such as to cause explosive growth, and that the system then in period t runs against the ceiling. The rate of growth is thereby reduced below what it would otherwise have been. This will serve to discourage investment in period $t + 1$, and as a result investment and income in that period may fall below the ceiling level. Suppose they do, and that, moreover, income in $t + 1$ is actually lower than in t .¹ Net investment in $t + 1$, however, remains positive. Assuming that investment (I) is a function of the levels of income (Y) and capital (K) in the preceding period, I_{t+2} must be lower than I_{t+1} since Y_{t+1} was lower than Y_t and K_{t+1} was higher than K_t . The falling away from the ceiling does not have any tendency to cause a rise in I between period $t + 1$ and period $t + 2$, since Y was already below the ceiling level in $t + 1$ and I_{t+1} was therefore unaffected by the ceiling (except insofar as the ceiling had served to hold down Y_t). A cumulative contraction therefore sets in. This may, of course, be held in check by such things as the arrears of investment left over from the boom, lagged reactions of some classes of investment, etc., but the distinctive feature of the early reversal of an adverse shock is absent and the slump is therefore liable to be prolonged.

In which of these two possible ways does the ceiling actually work, supposing it works at all? It is not particularly difficult to make a case for treating it as a shock subject to reversal as Duesenberry's argument requires. All that is needed is to postulate an appropriate lag or lags in its operation. Thus in the example above, although the ceiling is not actually in operation in the period $t + 1$, I_{t+1} may be held down by the ceiling because the ceiling was in operation when this investment was being decided on. I_{t+2}

¹ A downturn need not necessarily result from impact with the ceiling. Whether it does so depends on the values of the parameters. I have discussed this point more fully in "A Note on Crawling along the Ceiling," to be published shortly in *The Review of Economic Studies*.

may then represent a recovery. Equally, however, it is arguable that lags of this sort are not sufficiently important to have the effect required.

The suggestion that the operation of the ceiling is subject to lags in such a way that its removal acts as a positive stimulus is a distinct contribution to the theory of the lower turning-point. It is not, however, obviously more compelling than the various other explanations of recovery put forward in capital-adjustment models which Duesenberry rejects as unsatisfactory. It does not therefore establish convincingly that a ceiling-induced contraction will necessarily reverse itself more easily than a contraction due to some other shock, or indeed that it will reverse itself easily at all. The possibility of impact with the ceiling leading to a severe slump is not ruled out.

VI

In principle it is not difficult to devise means of testing alternative theories of the cycle. The large number and diversity of historical fluctuations may make generalisation difficult, but at least it affords plenty of material. There is not yet agreement on the conclusions, but it is reasonably clear how one may proceed. But when it comes to choosing between alternative models of long-run growth, the appropriate procedures and criteria are rather less plain. Different models involve different assumptions, especially assumptions about the stability of certain functional relationships, and they can be criticised if the required assumptions are not plausible. This is the nature of the criticism of Duesenberry's model put forward about on the score of structural stability. But to judge a theory by its assumptions is open to methodological objection,¹ and though this approach is not without its uses (especially in view of the difficulty of getting any decisive answer from empirical tests) it is unsatisfactory in many ways. After all it is essentially a matter of degree—the assumptions may be rather restrictive or implausible, but who is to say whether they are prohibitively so? The alternative procedure is to see how closely the results implied by the model conform to past experience and how successful they are in predicting the future.²

¹ Cf. M. Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (1953), pp. 1–43.

² A third approach which is sometimes used, explicitly or implicitly, is to judge a model by how much it explains and how much it treats as extraneous. But this is really an indication of the completeness of a model rather than of its validity. There is nothing inherently fallacious about the notion that the values of some important magnitudes—the rate and character of technical progress, the degree of monopoly, the rate of interest—are determined by forces that are essentially different from those that govern the behaviour of the economy when these are taken as given. A model-builder who takes this view may either have a separate theory as to how these magnitudes are determined or else he may treat them as unexplained constants, in which case his theory will be incomplete but not necessarily invalid within its limited field. Likewise there is no logical reason why growth and fluctuations should both be explained in terms of a single theory. The virtue of explaining everything important within the ambit of a single simple model is æsthetic rather than scientific.

These two aspects of empirical testing as applied to Duesenberry's model may be considered separately. First, the experience of the past. Which provides an apter picture of what has happened: Duesenberry's model or one in which population growth and technical progress are more directly responsible for the growth of demand, as they are in the models of a number of other recent writers? So long as the economy enjoys fairly steady growth, the outline result yielded by the two types of model will not be very different. Demand and productivity will be rising, the average proportion of the labour force unemployed will be relatively small and there will be no systematic reason for changes in the capital-output ratio, the proportion of income saved or the distribution of income. Neither type of model is therefore inconsistent with what Mr. Kaldor has called the "stylised facts" of economic growth, and more refined tests need to be applied to discriminate between them.

In some respects the reaction of the system to a change in circumstances may be more instructive than its behaviour when growing steadily. An example may be quoted which is interesting but which unfortunately serves to underline the difficulty of reaching a definite answer. Since the First World War the rate of growth of the adult population of the United States has been markedly lower than it was previously, largely because of the introduction of legal restrictions on immigration. If the rate of growth of income is unaffected by the supply of labour, as the simplest version of Duesenberry's model would imply, one would expect to find the fall in the rate of growth of population to be offset by a rise in the rate of growth of production per head. If, on the other hand, the ultimate determinant of the rate of growth of output is the rate of expansion of factor supply, one would expect there to be a fall in the rate of growth of total output, and there would be no particular presumption of an offsetting increase in the rate of productivity growth. In fact, productivity in the United States has risen decidedly faster since the First World War than it did before, though the change has not been quite enough to prevent a fall in the rate of growth of total output.¹ This would appear on the face of it to be a distinct point in favour of a demand-dominated theory of the Duesenberry type. But it is not really as simple as this. Duesenberry himself maintains that the underlying forces making for growth on the demand side were seriously weakened for various reasons after 1919. He would not therefore be able to attribute the speeding up of productivity growth to the persistence of the old underlying growth equation in face of a slowing down of population growth. He puts forward a number of special causes to explain the rapid growth of production in the 1920s (speculation in real estate, the stock-market boom, etc.), and since he does not suggest that the underlying forces making for growth have become significantly stronger again since then, he would

¹ See S. Fabricant, *Basic Facts on Productivity Change*, Occasional Paper 63, National Bureau of Economic Research (1959).

presumably also seek special explanations (such as government spending) for the vigorous growth of the 1950s. So his theory is not really much more successful in providing a built-in explanation of the speed-up of the rate of growth of productivity than the alternative type of theory is.

When it comes to prediction of the future, Duesenberry's model is capable of yielding spectacularly different conclusions from other types of model. For although his theory is optimistic in one respect, in that it denies the inevitability of cyclical depressions, in another respect it is pessimistic, in that it represents the whole growth process as highly precarious. An unfavourable change in the parameters, such as an increase in the relative importance of industries with low capital-intensity, is liable on his model to cause a complete breakdown of growth, not merely to cause growth to proceed with a rather lower average level of employment and/or profits, as in other models. In this it is akin to the stagnation thesis in its most extreme form. Unfortunately from the scientific point of view, though fortunately from others, this predictive test is not likely to be carried out, since governments now assume responsibility for preventing severe slumps. Indeed, the role of the Government in controlling the volume of demand, and also its composition to some extent, serves more generally to destroy the usefulness of predictive capacity as a test of growth models. Although many of the concepts and relationships involved are likely to retain their relevance to the contemporary scene, the devising and testing of self-contained models of capitalistic growth should perhaps be regarded as essentially a historical study, relating to an epoch now closed.

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REVIEWS

Theory of Public Finance. By R. A. MUSGRAVE. (New York and London: McGraw-Hill, 1959. Pp. xvii + 628. 97s.)

MUSGRAVE's book has been long-awaited and long-heralded. Let it be said at the beginning that it will immediately take its place as one of the latest works in its chosen field. In erudition, theoretical insight and encyclopædic knowledge it ranks with great treatises in other fields such as Humpeter on *Business Cycles* or *The History of Economic Analysis*. It would seem that the author has been engaged on the work for twenty years; it is quite safe to predict that it will be widely read twenty years from now, and most probably a great deal longer.

Having paid tribute to the stature and intrinsic worth of the book, I would like to work off my irritation straightaway about a minor but irritating defect. There is an extremely large number of misprints in the book, some of which are very minor (such as names of authors misquoted), but some of which do affect the sense of the argument. It is understood that any of these have now been spotted and are to be corrected in the second edition (and there certainly will be a second edition), but in the meantime it is quite impossible just to hand the book as it stands to any student. It is simply astonishing that an author so well versed in the marginal principle could have failed to apply it to himself. A very small additional effort at the final manuscript or galley-proof stage would have brought in vast gains in accuracy and clarity.

The title of this book is, for once, a close guide to the contents. Musgrave's approach is nakedly and unashamedly theoretical; he is concerned with both normative and positive aspects of public finance, but has no truck with such matters as institutional backgrounds, actual tax arrangements and so on. Part I ("Statement of Issues") and Part II ("The Satisfaction of Public Wants") are largely concerned with the normative side. Musgrave argues that the budget of the public sector has three branches—allocation, distribution and stabilisation. The most original feature of this part is the distinction between two functions of the allocation branch—the satisfaction of *social wants* (public goods which must be consumed in equal quantities by all) and *merit wants* (the conventional social/private discrepancies). A good deal of the subsequent argument turns on this distinction. The benefit and ability to pay theories are then critically examined; and both are rejected as being incomplete solutions to the determination of the budget plan. The fundamental defect of the benefit theory is characterised (p. 134) as being that it provides no way of ascertaining the true benefits; and in the ability to pay case there is no

visible means of ascertaining social preferences (p. 134). In the end, therefore, Musgrave argues that the provision of social wants, merit wants and the determination of the income distribution pattern (and also the issues of employment and price stability?) are matters to be resolved by voting through ballot boxes rather than voting through purses. At the normative level economics has something to say about avoiding excess burdens (Chapter 7) and the appropriate index of equality (Chapter 8) but precious little else. A final chapter in Part II is an interesting but somewhat incongruous excursion into national-income literature.

Part III ("Adjustments to Budget Policy: Classical Aspects") deals with the traditional positive problems: the relative effects of different tax and expenditure systems on work effort, saving, relative prices and outputs, investment and the distribution of income. Part IV ("Compensatory Finance") accounts for about one-third of the book. The author weaves into the narrative some discussion of growth problems and a lengthy discussion of the theory of debt policy as well as the subjects more usually covered by this heading.

This very brief summary may give some idea of the width and depth of the work. It may be most helpful now to use the remaining space to explore, first, some of the book's outstanding merits and, second, some of the sins of commission and omission.

The merit *par excellence* of this book is the high quality of the theoretical reasoning. Without recourse to anything more than elementary mathematics Musgrave succeeds in cutting through a vast amount of tangled top-growth and sorting out the main stems of argument clearly and concisely. Thus the excess burden of indirect taxation argument (pp. 140–55), the effects of various taxes on the division of personal income between consumption and saving (Chapter 12) and the ramifications of the balanced budget multiplier theorem (pp. 432 ff.) are all set out so explicitly that there is no longer any excuse for the old fallacies to crop up again. There are many innovations in the book as well as solutions to old problems: a good example is the insistence (e.g., pp. 243–6) on considering group reactions as well as those of the representative individual. The essential point here is that if we substitute a progressive income tax for a proportional tax we must look at the relative changes in marginal and average rates for each income group and the marginal rates of substitution of leisure for income for each group separately if we are to have any hope of sorting out the answer.

It cannot be denied that the author's willingness, and indeed anxiety, to explore these theoretical issues fully leads him to consider many unusual and indeed queer cases (e.g., the "57 varieties" of assumptions about the possible shapes of marginal utility of leisure and marginal utility of income curves in Chapter 11 and the manifold ways (Chapter 18) in which compensatory tax adjustments may influence consumption). Although one can have some reservations about the methods of treating these problems

children, there can be no question that it is extremely valuable to have the answers all on tape, as it were.

To have explored some of these issues thoroughly would have been no mean feat; to keep up the performance over 600 pages is an intellectual achievement of great distinction.

If there were nothing to disagree about in a book of this length, it would be a matter for condemnation of the author, or the reviewer, or both. The first general point is really one of methodology. As it stands, the book is likely to be found overwhelming by most readers. Case after case is considered, every possibility is explored, no stone left unturned; and so it is hardly surprising that one has no clear impression when one gets to the end of which things are important, which results are the more likely and so on. Fundamentally, I think, this springs from the author's predilection for abstract theoretical reasoning and his reluctance to cite empirical evidence. There is nothing wrong in being a tool-maker, especially when the maker does clearly understand, as in this case, that his tools are meant to be used. But it is surely reasonable to expect some indication about whether any particular implement is likely to be in more general use than others. Thus in Chapter 11 we have a long refined analysis about the effects of taxes on incentives to work followed by a rather feeble single page on secular evidence. There have been a number of recent inquiries on the effects of taxation on incentives to work; discussion of these findings, their merits and demerits, would surely have enriched the argument. Similarly in Chapter 12 the point is (quite properly) made that the substitution of a consumption tax for an income tax will be more favourable to saving, the more is saving for the purpose of accumulation than for future consumption. But there is no word on which is or is likely to be the more realistic case. Musgrave would, one feels, be a superlative general in a mopping-up campaign remorselessly rounding up pockets of enemy resistance, but perhaps not quite so outstanding leading the charge towards a single strong point.

The second point is one of arrangement. Obviously the book has grown piecemeal over the years and suffers some of the consequences. We have already remarked on some incongruity in Part II. Similarly, it is not entirely obvious why problems of growth should all be discussed in Part IV on Compensatory Finance. Nor are topics always co-ordinated as they should be. There is some overlapping discussion of effects of taxes on the work/leisure choice and on the consumption/saving case in Part II (Chapter 7) and Part III (Chapters 11 and 12). Capital-gains taxation, business-income taxation and depreciation provisions are all discussed at various stages. But it would need an extremely perspicacious reader to divine that these problems hang together and that, for instance, the inclusion of capital gains in the personal income tax net is to a large extent an alternative to the differential taxation of undistributed profits.

Thirdly, and perhaps inevitably, the book is unquestionably uneven. There is no clear distinction in logic between unevenness in the sense of spending too much time on one subject at the expense of another and unevenness in the sense of one page being more difficult than another. As the first is a matter of judgment and the second one of fact, one cannot be dogmatic on this general point. Nevertheless, it does seem odd to find some twenty pages on voting theory, drawing on, *e.g.*, Arrow and Black, but only four pages on Federal Finance without any reference to Wheare or similar authorities. There are twenty-four highly compressed pages on the effects of taxation on risk-bearing (Chapter 14) but no connected discussion at all on the principles of minimising the costs of administering taxes. A whole chapter (Chapter 15) deals with the complexities of price and output adjustments by business firms to various taxes, but the overlapping jurisdictions of income taxes across territorial frontiers and the problems of property taxes scarcely get a mention. But, to repeat, matters of opinion cannot readily be disentangled from matters of fact on such points as these.

Finally, there are points at which Musgrave's analysis seems dubious or obscure. This again is inevitable in a book of such proportions, but perhaps one example may be given of each. On pp. 249-50 the author argues that an equal yield proportional consumption tax is likely to induce a larger amount of work effort than a proportional income tax, principally because of group effects. It is argued that as the average propensity to spend rises with income, the imposition of a proportional spendings tax will be equivalent to substituting a regressive tax on income for a proportional tax, and that therefore work incentives will be greater. However, the problem is not one of the relation of marginal rates to average rates of spendings tax but rather of that of marginal spendings tax rates to marginal income tax rates. If one allows for the fact that the expenditure tax rate must be greater than the equivalent income tax rate (by a factor depending on the national savings ratio), the fact that tax on saving for future consumption is postponed rather than abolished and the further point that the marginal propensity to save is usually found to be fairly invariant to income, the argument becomes much more open than appears from the text.

As an example of obscurity, the discussion on p. 168 about the measurement of depreciation in real terms may be cited. Quite apart from the (obviously erroneous) transition from \$ to million \$ magnitudes and the inadequacy of the statement that in times of inflation "original cost does not permit the investor to recover the real cost of his outlay," the general argument in terms of real value of net worth is not likely to be found enlightening by many.

But compared with the intellectual achievement of incorporating all these ideas and arguments within two covers, these criticisms are of small

importance. The note on which one must end is that from now on anyone interested in the theory of public finance can simply be told, "It's all in Musgrave."

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Theory and History. By LUDWIG VON MISES. (London: Jonathan Cape, 1958. Pp. ix + 384. 30s.)

THIS new volume by Professor von Mises has the same texture and tone as his earlier books on *Human Action* and *Socialism*. There are frequent cross-references, and both the methods of argument and the conclusions are perfectly consistent. The defence of the liberal market economy and of economics and "praxeology" as *wertfrei* intellectual studies with "unique epistemological character" will be familiar to all his readers. The defence, of course, is most frequently a scathing attack. Never can the market economy have been handled so stridently or the history of ideas so brusquely. The book is littered with adjectives such as "absurd," "preposterous," and "pernicious," and nouns such as "pseudo-economics," "fantasy" and "slavery." Professor von Mises attacks Marx for refusing to argue with his opponents and assuming that it was sufficient to unmask their bourgeois background and thereby the necessarily "ideological" character of their doctrines. Nonetheless, he often follows this procedure himself. He assumes that all who disagree with his version of liberal economics are "driven by an idiosyncratic abhorrence of the market economy and its political corollary" (p. 242), that all recent socialists base their hopes not "upon the power of their arguments but upon the resentment, envy and hatred of the masses" (p. 65), that Marxists "cling to historical materialism and stubbornly refuse to listen to its critics because they want socialism for emotional reasons" (p. 158), and that people who "support policies that aim at the substitution of planning by an authority for autonomous planning by each individual" are "longing for slavery" (p. 376). In a more restricted context he claims that "it is only the passionate pro-socialist zeal of mathematical pseudo-economists that transforms a purely analytical tool of logical economics into a utopian image of the good and most desirable state of affairs" (p. 367).

It is impossible to dismiss comments of this kind as disagreeable tricks of style. There are other comments which betray a tendency to sweeping generalisation as well as passionate feeling. "Marx misrepresented the operation of the capitalist system in every respect" (p. 117). "What prompted those who suggested the substitution of the social sciences for the sciences of human action was, of course, a definite political program"

(p. 259). "The religiousness of these sincere believers had nothing in common with the established system of devotion" (p. 339). Remarks of this kind suggest not liberal openness of mind but a refusal to leave familiar grooves of thought or, to use Professor von Mises' more technical language, too uncritical reliance on "thymological" explanations for shifts and diversities in ideologies. Whatever might be said about the differences between his approach to economics and the approach of economists who do not agree with him, it is clear that there would be an even bigger gulf between what he considers to be "thymological experience" and what other people would consider to be such.

Four points of more particular criticism raise important general issues. First, Professor von Mises, who disposes of many arguments, does not dispose of the doubts, insights and suggestions about value theory which made Gunnar Myrdal's recent collection of essays so illuminating. He writes too confidently about "ends" and "means" in his first few pages, and repeats the word "*wertfrei*" frequently in the remaining pages. It is neither malice nor evil intention which has made people doubt his version of value theory but genuine intellectual uneasiness. Second, as a historian Professor von Mises is less convincing than he is as a writer about history. He makes many extremely suggestive general remarks about history and the methods historians should use, but he grossly over-simplifies both the actual history of ideas and the history of institutions. It is not true to say, for instance, that "no proletarian or son of a proletarian contributed any substantial idea to the socialist ideology" (p. 121) or that "workers were never enthusiastic about socialism." Interesting historical questions are side-tracked in this kind of dogmatic account of what happened in the past. Third, in his account of the operation of the economic system at the present time he does not examine carefully either legitimate discussions of "weaknesses" in capitalism or such interesting phenomena as rates of growth in socialist countries. Of the capitalists and entrepreneurs of a market economy, whose role has changed in different periods of economic history, he writes generally "they acquire and enlarge their property through the services they have rendered to the consumers, and they can retain it only by serving daily again in the best possible way" (p. 116). No attempt is made to distinguish between the theory of a market economy and the actual working of "free economies" in practice both in the past and at the present time. Fourth, he uses terms which provide props for his argument but never receive careful enough attention. The most important of these terms is "masses." He rightly accuses Marx of not having clearly defined what he meant by "class," but he tends to use the word "masses" himself as a very general term which some of his readers will query. There is a considerable truth in a comment by Mr. Raymond Williams in his *Culture and Society* that "there are in fact no masses; there are only ways of seeing people as masses." Indeed, Mr. Williams' book, which deals with the history of the use of

words in changing social contexts, brings out clearly why the kind of arguments advanced by Professor von Mises—many of them echo (often eloquently) old controversies—have not received the assent of a considerable number of intelligent people over the last hundred and fifty years.

It is a tribute, of course, to Professor von Mises that his trenchant exposition provokes vigorous dissent. How useful argument at this level really is must, however, be open to question. One reason why Professor von Mises' eloquence will not convince all his readers is not that they are stubbornly cocksure but, to use a phrase of Gunnar Myrdal, that they are "more modest" than he. They will doubt whether what the blurb of *Theory and History* calls the author's "frequent tilts at other experts" will generate more light than heat.

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ASA BRIGGS

The British Budgetary System. By SIR HERBERT BRITAIN. (London: Allen & Unwin, 1959. Pp. 320. 25s.)

It is hardly accidental that it is quarter of a century since the last edition of a comprehensive account of the British budgetary system appeared, bearing in mind the preoccupation of writers in public finance with the more fashionable and possibly more exciting study of the use of the budget as a stabiliser and redistributor of income. However, this very change in the policy uses of the budget has brought into being major alterations in the structure of the system of public finance, notably in the financial relations between central government, local government and nationalised industries. Sir Herbert Britain's study is very welcome, all the more so because it is written with the inside knowledge of a former senior administrator of the Treasury.

There are two noteworthy features of this book. We would expect from Sir Herbert an authoritative account of the whole budget "cycle" from the preparation of estimates to the deliberations of the Public Accounts Committee. What is particularly useful in his survey is the documentation. This means that the book is more than a handbook for students and civil servants, but also a starting-point for detailed research into budgetary matters. The second feature is that the budget is explicitly regarded as an instrument of economic policy. It is obviously impossible to expect a book of this kind to preface description of our financial administration with a complete treatise on social accounting and the theory of fiscal policy. Sir Herbert has managed to summarise the rationale of fiscal policy in a few pages in Chapters II and III very well indeed, and as a result, the more sophisticated reader, at least, will discern more clearly the links between theory and policy.

One regrets that Sir Herbert has chosen to avoid controversy. Hilton

Young's book seemed to gain by its obvious bias and occasional stylistic flourishes; remember his nostalgic reference to Hume's and Gladstone's concern for public spending—"now in Elysium it is theirs bitterly to lament the wastage of Ambrosia." We must respect the author's views on this matter, but he invites criticism on two counts in this connection. First of all, having laid emphasis on the budget as an instrument of economic policy, he nevertheless deliberately avoids discussion of various important proposals for budgetary reform. Secondly, we are told in the Introduction that where controversial matters arise, the author has confined his comments to what he conceives to be the justification of existing practice, but a little later we are told that he does not purport to express "what may have been official views on the points under discussion." The reader may be excused if he has difficulty in identifying what is exactly the Treasury view. For example, is Sir Herbert's view on modern debt theory (pp. 53-4) or his discussion of the necessity for more information on the distribution of National Debt holdings (pp. 183-4) shared by to-day's Treasury officials? It would be interesting to know.

It is praise enough for a work of this kind that it makes the subject palatable. Sir Herbert Britain's book does just this, but at the same time it preserves enough detail to make it an essential work of reference as well as a text-book.

ALAN T. PEACOCK

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Studies in the Quantity Theory of Money. Edited by MILTON FRIEDMAN. (Chicago: University of Chicago Press (London: Cambridge University Press), 1957. Pp. 265. 37s. 6d.)

THE behaviour of monetary systems provides one of the most rewarding fields of empirical study to be found in the whole of economics, and this collection of four such studies, with an introductory essay by the editor, is to be warmly welcomed as a set of new explorations, or of extensions of previous explorations, in that field. At first sight the studies have little common character; Mr. Cagan's long article on the monetary dynamics of hyperinflation and Mr. Selden's on monetary velocity in the United States are attempts to establish econometric models explanatory of the courses of events: Mr. Klein's investigation of German money and prices from 1932 to the end of the War is more concerned with the establishment of the statistics required for calculating velocity, and with qualitative explanations of the changes in velocity: Mr. Lerner provides something more like a straightforward chronological account of monetary policy and inflation in the Confederacy. One common feature of these studies is, however, their use of the concept of velocity of circulation—a useful tool of analysis which has ately, for no very good reason, been out of fashion. Here it is introduced

along with the concept of demand for money with a commendable absence of inhibitions.

Mr. Cagan's model of the process of hyperinflation, which he tests with data from Germany, Poland, Austria, Hungary and Russia after the First World War and from Greece and Hungary during and after the Second, is somewhat similar to a less-formal one which the present reviewer used in an earlier work; it consists essentially of a relationship between the rate of increase of prices and the subsequent value of the velocity of circulation, or real value of the money stock. In the Cagan model the rate of increase of prices, which is taken as determining current expectations about the rate at which prices will continue to rise, is an average, with exponentially decreasing weights, of the rates of increase experienced right back to the start of the inflationary period. Having obtained generally close relationships between this and the real value of the money stock (which would, of course, be proportional to the inverse of velocity if real income remained constant), the author shows that unless prices are very sensitive to their past rates of change, hyperinflation will not be a continuously self-generating process without a continuous increase in the money supply. It seems that only in the German and Russian inflations was this sensitivity of prices to their past rate of increase sufficient for hyperinflation to continue indefinitely after a stabilisation of the supply of money.

This is an extremely interesting result. Mr. Cagan's attempts to go beyond it are, however, less convincing. His attribution of the increase in the supply of money to the desire of the authorities to obtain revenue by this means, and his denial that it is to any major extent due to pressure upon them to finance rising factor payments (rising because of expectation-inspired wage demands or the depreciation of the currency on the foreign exchanges) would be difficult to justify in regard to, at any rate, the German hyperinflation, and he presents no evidence that appears to justify it.

Space does not permit any comment on the very useful contributions of Mr. Klein and Mr. Lerner, except that it is a little surprising that even in Chicago the experience of the Second World War should not throw more light than the former writer, and the Editor, appear to have perceived upon the process by which really savage wartime controls depress the velocity of circulation. Mr. Selden's attempts to explain the course of velocity in the United States present, however, some very interesting features. Perhaps the chief of these, at first sight, is the discovery that the difference between long- and short-term interest rates is much better correlated with velocity than is either of these rates by itself, and that real income per head also seems to be a useful explanatory variable. The author's treatment of the excess of long over short rates as the "cost of holding money substitutes" (*i.e.*, short-term securities) constitutes an ingenious analogy with the Keynesian treatment of any interest-rate as the "cost of holding money."

To argue in this way is, however, to treat the velocity of circulation as if

it were determined by interest rates (or differences between them) acting as an independent variable. It is, perhaps, helpful to reflect that when money is scarce (velocity high) there is pressure to realise both short- and long-term securities, but more especially the former, because of their greater saleability and better maintenance of capital value; hence high velocity goes with relatively high short rates in relation to long rates. The interesting discovery that this difference is better correlated with velocity than is the *level* of rates, either short or long, could, nevertheless, hardly have been made if the author had, like many of his predecessors, been intent mainly on "explaining" the course of rates of interest.

The significance of real income per head in this connection is apparently greater for long-term trends than for short-term changes. It is, however, not by any means clear how far the relationships dealt with by Mr. Selden subsist merely between simple trends in the variables. Indeed, it should perhaps be added that the statistical basis for the deductions in this study is open to some suspicion. The degree of inter-correlation between some of the variables which are treated as independent is not stated, but it may well be so high as to place the validity of the multi-variate analysis in doubt.

A. J. BROWN

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Trade Balances during Business Cycles: U.S. and Britain since 1880. By ILSE MINTZ. (New York: National Bureau of Economic Research, 1959. Occasional Paper 67. Pp. x + 99. \$1.50.)

THIS is an interesting and careful study of a subject which the events of the recent recession have made of topical interest. It is easy to make a list of the factors which in theory will determine the cyclical behaviour of a country's balance of trade: the relative magnitude and timing of cycles in it and abroad, relative price- and income-elasticities of demand, relative elasticities of supply and so on. These are complex and would permit almost any result. The empirical data, however, viewed over a long period, show a clear-cut pattern, which is all the more remarkable for having undergone, in the British case, a complete reversal after 1914. Before 1914 the British balance of trade rose in cyclical expansions and fell in cyclical contractions, whereas since 1914 the opposite has been the case. In the United States the normal rule has always been for the balance to fall in expansions and rise in contractions.

The American experience invites explanation in terms of a high income-elasticity of demand for imports. Dr. Mintz shows that this is only part of the truth and that a falling off in American exports towards the end of the expansion, followed by a recovery when the recession has begun, has been equally if not more important. The British experience since 1914 has been similar to the American in general result, but it has been customary to

explain the two rather differently, with less emphasis in the British case on the income-elasticity of demand for imports and more emphasis on the terms of trade. Dr. Mintz attributes the reversal in the cyclical behaviour of the British balance after 1914 to the corresponding reversal in the cyclical behaviour of the terms of trade, already noted by Martin and Thackeray and others.

An important feature of Dr. Mintz's study is the technique of distinguishing "co-phases," in which the level of activity in the country concerned is moving in the same direction as that in the rest of the world, and "counter-phases," where the opposite applies. This distinction yields some interesting contrasts. Thus in Britain since 1914 the favourable movement of the balance of trade in contractions has been more pronounced in co-contractions than in counter-contractions, whereas in the United States the reverse has been the case. This confirms the notion that what mainly matters for Britain is the change in the terms of trade brought about by differing elasticities of supply of different classes of goods in face of an overall world fall in demand, whereas for the United States it is the *relative* levels of demand at home and abroad that are most important.

The phase technique does not apply quite so well to Britain before 1914. For this period a British boom is not something that can be thought of as an essentially domestic phenomenon and set in contrast to a world boom. It is shown that before 1914 Britain's balance of trade fell more in counter-contractions than it did in co-contractions, the reason being a less favourable movement in the terms of trade in counter-contractions. This is on the face of it a rather troublesome conclusion, since it appears to suggest that a world expansion was, after all, bad for Britain's terms of trade before 1914 as well as after. But inferences about the effects of world expansion drawn from occasions when world expansion coincided with British contraction are misleading, since such expansions were by their nature untypical. Britain's unique position as supplier of heavy industrial goods and as lender meant that as a rule a world boom meant an intense demand for British exports and a rise in their prices; a British co-expansion thus resulted and an improvement in Britain's balance and terms of trade. Occasionally, however, there were expansions abroad which, because they did not extend to our chief markets or for some other reason, did not raise the demand for our exports much but did raise the prices of our imports. These would presumably be the world expansions associated with British counter-contractions, but they would not give a representative picture of the normal effect on Britain of a world expansion. The loss of Britain's special position as a supplier of capital goods and capital after 1914 was no doubt an important reason for the reversed cyclical behaviour of the balance: a world boom no longer meant such a concentration of demand on British industry.

One interesting finding of the study is that cyclical movements in the balance in both countries were much stronger around the peak of the cycle

than at other phases. Significant non-linearities in supply functions would appear to be present. In Britain before 1914 this assumed a peculiar asymmetrical form: before the peak exports rose *more strongly* than imports; at or after the peak exports fell *sooner* than imports. Dr. Mintz regards this as a lag of imports after general business activity. Others have interpreted it as a lead of exports.

The paradoxical deterioration of the American balance of trade in the recession of 1958, which finds a parallel in the depression of the 1930s but in very few other cases, occurred too late to receive more than a postscript footnote from Dr. Mintz. Dr. Mintz is disposed to regard it as an abnormality rather than as a sign that changes in import structure or a tendency for American exports to become a marginal source of supply are causing her position to resemble that of Britain in the days of her world leadership.

Dr. Mintz's concluding speculations on the impact of balance-of-trade movements on the level of activity through their effects on foreign exchange reserves seem premature in the absence of an equally systematic study of other items in the balance of payments, especially capital movements. The latter are also given rather less consideration than they might seem to deserve in the explanation of the behaviour of the balance of trade itself, especially for pre-1914 Britain.

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Expectations, Uncertainty, and Business Behavior. Edited by MARY J. BOWMAN. (New York: Social Science Research Council, 1958. Pp. 202. \$2.00.)

It is probably generally agreed amongst economists that the theory of business decisions is in an unsatisfactory state. Indeed, a good case could be made for the view that the theory is held to be much more useless than it really is. In any event, this collection of papers and the conference which occasioned them are clearly inspired by the belief that a new and determined attempt should be made to understand the nature of business decisions. Moreover, it is held that such an attempt to be successful must involve a number of disciplines.

Almost all the papers here printed are interesting, some are extremely well argued, and the Conference was clearly worthwhile. On the other hand, it must be admitted that nothing that could be called a "new theory" emerges, that the psychologists speak a language which does not seem of immediate use to economists (and no doubt *vice versa*) and that when one has read them all one is not a great deal farther towards an acceptable method of analysis than one was before. However, since most issues ever raised are competently discussed in one or other of the papers, they should certainly be read by all who work in this field.

The book is divided into four parts: "Theories of the Nature and Structure of Expectations" (Georgescu-Roegen, Shackle and Ward Edwards); "Psychological Approaches to the Study of Expectations and Business Behavior" (Herbert Simon, Katona, Meredith, William E. Henry and Albert Lauterbach); "Effective Expectations and Time Horizons with Special Reference to Practices in Selected Industries" (Ruth Mack, Merton J. Peck and Hans Brems) and "Forecasting and the Role of Expectations in the Economy of the Firm" (Modigliani, Kalman J. Cohen, Robert Eisner, Irwin Friend and William Darcovich). It is clear that a comment cannot, in the space of this review, be offered on all papers.

Georgescu-Roegen's essay entitled "The Nature of Expectations and Uncertainty" combines a lucid and critical account of a number of probability and expectation theories with a very persuasive exposition of his contention that "the real number system does not suffice to represent all expectations and that those expectations which can be meaningfully connected with a real number form only a special class." This paper should be widely read in particular for its criticism of Savage's well-known book. To a non-specialist, in this field, Roegen's arguments are pretty convincing.

Professor Shackle's "Expectation and Liquidity" is a beautifully written account of his theory, together with an attempt—alas unsuccessful—to apply it to the demand for assets. The rather *ad hoc* machinery constructed for this purpose will not do. Indeed, I think one must recognise that the Shackleian apparatus, while it affords considerable psychological insights, is no more a complete theory of expectations and decisions than any other and is open to a number of rather important logical objections—some of which become particularly insistent when the demand for assets is considered.

Herbert Simon's paper "The Role of Expectations in an Adaptive or Behavioristic Model" is really excellent. Nothing that he says is startlingly new (partly because he himself has already argued on similar lines on earlier occasions), but it is always to the point and very convincing. He draws the by now familiar distinction between "programmed" and "non-programmed" decisions, argues that the two types must be treated differently, shows that there are good grounds for believing behaviour to be "adaptive" rather than "optimising" and makes excellent use of the notion of "aspiration levels." Anyone on the way to firms armed with questionnaires will do well to read this paper first.

The psychologists' contributions cannot be readily assessed by an economist. However, the paper by Meredith, "The Surprise Function and the Epistemic Theory of Expectations," certainly holds the non-specialist reader's attention. He regards the individual in all his activities, as constructing models (expectations) of what the external world is like and presents (rather elliptically) a neuro-physiological account of this. From our point of view, the interest of this theory lies in the importance which it attaches to what Meredith calls "biography": that is, the kind of model of the

external world someone constructs will depend on the kind of man he is, and this in turn will depend on his past history. The reason why this view seems to me interesting is that it faces us again with a well-known problem: is it ever possible to construct a general theory of expectations and decisions, or must we recognise the overriding importance of the personal (biographical) element in these processes? One suspects that the answer would be that while biography is vital to our understanding of any particular decisions, social institutions and the like, assure that many individuals share similar biographies. One would like to hear the sociologists on this. (Lauterbach's paper is of some use here.) It may well be that a classification of businesses by "biographical" types would be more illuminating for some purposes than the usual "industry." Mr. Meredith seems to me unduly optimistic about the willingness of businessmen to allow economists or others to sit on important boards for research purposes.

The empirical papers are all interesting and well worth reading. Both Ruth Mack and Merton J. Peck suggest that uncertainty (in the Knightian sense) is probably less important in inventory decisions than has hitherto been supposed, and they examine a number of ways in which the businesses they have studied organise their information concerning the future. Hans Brems constructs a model on the basis of which he concludes "that the timing of expected rival response to price changes is radically different from the timing of expected rival response to changes in the non-price variables . . . (and that) in such circumstances, profit maximisation will turn the odds overwhelmingly against price competition." He illustrates his contention with reference to the automobile industry. Robert Eisner uses known plans (*ex ante* data) and actual sales to test a sensible and fairly traditional theory of investment decisions. His conclusions are tentative, but nonetheless very interesting, particularly since they are not in noticeable conflict with current theory. Modigliani and Cohen provide a theoretical discussion of "The Significance and Uses of *Ex Ante* Data." This is an important topic at a time when we can look forward to an increasing flow of such data.

All in all, a most interesting collection, of which a review can only give the most imperfect of accounts.

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Micro-economic Theory: A Mathematical Approach. By J. M. HENDERSON and R. E. QUANDT. (New York and London: McGraw-Hill, 1958. Pp. xii + 291. 58s.)

THIS is a pleasant, well and succinctly written text-book suitable for beginners in mathematical economics. Besides an introduction, there are seven chapters and an appendix on mathematical techniques. These are

elementary determinants, calculus and difference equations. The chapters are entitled: "The Theory of Consumer Behaviour," "The Theory of the Firm," "Market Equilibrium," "Multi-market Equilibrium," "Monopolistic Competition," "Welfare Economics" and "Optimisation over Time." It is seen that the book covers a wider field than its title would lead one to believe. Indeed, a major criticism of an otherwise laudable volume is that far too much ground is covered in a superficial way and too little in depth.

The superficiality is of two kinds: lack of precision at some important points of the argument and a failure to follow on discussion to its proper conclusions. A few examples of the first kind are:

(i) The student is given the impression that utility maximisation assures that indifference curves are everywhere convex (p. 14), while, of course, they only assure convexity at the chosen basket. The same mistaken impression is given with regard to isoquants (p. 51).

(ii) The impression is given that utility maximisation implies *strict* convexity of indifference curves, and hence single valued demand functions (p. 21). This is not so.

(iii) It is argued that the concept of consumers' surplus requires cardinal utility measures (p. 22 footnote), which is not true. (The author gives only the Marshallian definition.)

(iv) The symmetry of the off-diagonal elements in the well-known Hicksian bordered determinant is used in discussion but neither proved nor commented upon.

(v) Houthakker did not prove that, given his axioms, the consumer must possess an indifference map (p. 33). The frontier of all points not inferior to some collection x and all points not superior to that point is not necessarily a collection of points between which the consumer is indifferent.

(vi) The term "gross substitutes" is used (p. 123) and not defined anywhere.

(vii) The integration of money into a general equilibrium analysis is carried through faultlessly, but the whole section is quite spoiled by the nonsensical conclusion that the quantity of money *determines* the level of money prices *because* it can be shown that equilibrium money prices vary directly with the stocks of money (p. 145). In the model the authors use the equilibrium rate of exchange of any good for money cannot be determined from a knowledge of the stock of money alone. Nor could an index of such exchange rates. It is simply a wrong and misleading use of the word "determine."

(viii) The marginal rate of substitution between any two items consumed in any one "week" must in equilibrium equal the relative prices of these two items ruling in that week. Hence this equilibrium

marginal rate of substitution, in a multi-week model, is independent of interest rates. From this, the authors conclude that the utility function $U(x_1 \dots x_T)$, (where x_i is a vector of items consumed in week i) may be written as $V(c_1 \dots c_T)$ (where c_i is total consumption outlay in week i). This, however, is only true if the marginal rates of substitution between any two items in any one week are independent of the amount consumed of any item in any other week (separability).

These matters are perhaps not too serious. Indeed, there are few books in this field where a similar list could not be made. But the failure to pursue any one subject to its proper conclusion and depth is a different matter. Examples of these are:

(i) There is no discussion of consumers' surplus (except the footnote already mentioned), and no discussion of index numbers. Nothing on the Leontief-Hicks theorem on aggregation. Nothing on the Le Chatelier principle (which the mathematics used could easily have dealt with). Nothing on the application of programming to utility analysis (although programming is discussed elsewhere). Nothing on liquidity preference and the demand for assets, although the Von Neumann index is described. Nothing on insurance and gambling. Nothing on bilateral exchange.

(ii) The discussion of production makes no mention of the elasticity of substitution and the uses to which it has been put. There is no mention of innovations or their possible classification. Separable production functions and their uses in defining "factors of production" are absent from the chapter.

(iii) The chapter on market equilibrium makes no use, and contains no discussion of, the correspondence principle, although both stability and the effects of taxation are discussed separately.

(iv) No connection is established between the discussion of community indifference curves, etc., in the chapter on Welfare Economics and the uniqueness of equilibrium analysis of an earlier chapter. There is nothing on the role of "income effects" either in stability or in welfare economics. Too little is said on the economics of "free goods." There is no "comparative statics" analysis of the multi-market model, although the mathematics used would have been perfectly adequate for this.

(v) Only one kind of monopolistic discrimination is discussed. There is nothing on advertising or product differentiation. Although six theories of duopolistic behaviour are given, they are neither evaluated, criticised nor compared.

(vi) There is no discussion of the valuation of capital goods and intermediate products. Nothing on the definition of capital. A snippet on the period of production. No mention of some of the more important recent theories of savings.

Clearly there is much to be said for a short book, and if the authors had done all or some of the things mentioned they would have had to leave out others. But so they should have done. Micro-economics is really quite large enough a topic without discussing welfare economics and general equilibrium analysis as well. Instead, a chapter on decisions under uncertainty, one on savings and one quite separate one on production and programming would have been made better fare.

When all this has been said, one must hasten to add that, in spite of all this, the book is an impressive achievement. The presentation is everywhere clear and concise, and the authors are thoroughly at home with their subject. The teacher, provided he is willing to supplement the gaps in lectures, can certainly recommend the book to his students.

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The Challenge of Development. (Jerusalem: The Eliezer Kaplan School of Economics and Social Sciences, the Hebrew University, 1958. Pp. 233. 7s.)

THIS symposium, held in Jerusalem in 1957, includes contributions to the sociological, political and demographic as well as economic aspects of development. Much the most provocative and stimulating of the specifically economic papers is Professor Kahn's "The Pace of Development." Concentrating on societies in which the labour force is far from being fully utilised, he makes a strong case for the adoption of a bold strategy in making the steep ascent from backwardness, justly emphasising that "a high rate of development from the outset involves current benefit as much as current hardship, quite apart from all-round benefit which gradually results from the improvement in the means of production." There is a salutary plea for exploiting traditional methods of construction on a greater scale (compare the Chinese irrigation record in the winter of 1957-58) and an equally salutary warning of the use of global investment coefficients.

Given an abundant supply of unused or only partially or "imperfectly" used labour, Professor Kahn is surely right in inverting the conventional formulation by claiming that it is "as true, and perhaps more illuminating, to say that the proportion of the current income of the community devoted to capital formation depends in the rate of economic development." Much of his essay is devoted to the optimum methods of securing a greater redistribution of consumption in order to facilitate a higher volume of employment. It must be added that recent experience serves to confirm Professor Kahn's case for the setting of ambitious targets and for fuller utilisation of the labour reservoir, which if properly tapped constitutes perhaps the greatest single asset of a large under-developed country bent on rapid economic growth.

Other contributions include papers by Professor Kuznets on "Economic Growth of Small Nations" and by Professor Patinkin on "Secular Price Movements and Economic Development." In his essay on "Monetary Policy in Underdeveloped Countries," David Horowitz, the Governor of the Bank of Israel, reviews the dilemmas of a central bank in combating inflation when state enterprise is substituted for private initiative and "inflationary saving" for the formation or influx of capital. Professor Bachi's "Trends of Population and Labour Force in Israel" provides the basic demographic background to Israel's economic problems.

S. ADLER

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Business Budgets and Accounts. By H. C. EDEY (London: Hutchinson, 1959. Pp. 174. 18s.)

THIS book, by providing an introduction *appropriate to economists* to the principles and practices of business accountancy, makes good a serious deficiency in economic literature. For though we economists have by now become quite sophisticated about the preparation of statistical statements, of both the *ex post* and the *ex ante* variety, relating to an industry or an economy, we have remained very ignorant about the equivalent procedures within the individual firm. We can find our way through the complexities of the Blue Book on *National Income and Expenditure* but are helpless when faced with a corporate profit and loss account; we analyse with confidence a consolidated balance sheet of "the Banking Sector" but stare impotently at the balance sheet of the Bowater Paper Corporation; we are at home with the *Economic Survey* and have a contemptuous familiarity with the antiquated budgetary procedures of H.M. Government, while being scarcely aware of the existence of business budgets; we have learnt from Professor Devons and Mr. Nove about production programmes in the M.A.P. and the U.S.S.R. but not about "the physical production plans that decide," within an individual firm, "what work is to be done, and in what manner." Such, at any rate, is the ill-balanced state of the English economist's competence: I am not sure that our colleagues in America and on the Continent would be so uniformly open to the same criticism.

Now, however, we have Mr. Edey to guide us into the unfamiliar territory. His book is, of course, only an elementary introduction, but this is perhaps what we most need: a sympathetic lead into the existing specialist literature. For the accounting profession has its own tasks to perform, its own concepts, its own way of looking at things, its own convenient fictions, its own mumbo jumbo; and all are different from the economist's. In this foreign land, where fact and fiction seem to be inextricably intermingled, we need at first the help and advice of a fellow economist who already knows the local language and customs, and this is precisely what Mr. Edey offers us.

He explains in his introduction that the business accountant has to satisfy different masters: the shareholders, the directors, the tax authorities and (in case of liquidation) the courts. Rightly, in this book, he concentrates on the role of accountants in assisting the directors: this they do (he explains) by facilitating the setting and the achievement of three "targets": a target for revenues and costs, a target for the availability of money and a "target pattern of resources other than money" (p. 12).

These targets are commonly recorded in three principal budgets, each further subdivided and each covering every month in the forthcoming year: the general profit and loss budget, the finance budget (recording the planned flows of money into and out of the business) and the budgeted balance sheet. In a passage which might equally serve as an apologia for the *Economic Survey* or a war-time aircraft programme, Mr. Edey explains that—

"the preparation of the budget imposes a discipline on all members of the organization, in that it causes them to consider and reflect on what they intend to do. Furthermore, the budgeting procedure, if properly carried out, provides a safeguard against different departments of the business having mutually contradictory plans. And once the budgets have been established and approved they provide an operational plan of action for the guidance of all concerned. The checking of the actual results (the figures for which are provided by the accounting system) against the budgeted figures provides information on the course affairs are taking, and this may lead to changes in the way things are being done, or in the plans and budgets themselves, or in both. The comparison may also help to improve budgeting procedure in later periods."

"The year seems to be a natural period for human planning and most businesses that have budgets prepare them annually." This surely means that the figures relating to the later months of the forthcoming year will, when this time arrives, already be months out of date as forecasts and perhaps quite inappropriate even as targets. One is therefore surprised to learn that relatively few businesses adopt "a continuous process of annual budgeting at . . . monthly or quarterly intervals": nor (even more surprisingly) does Mr. Edey believe that they should: "There is something to be said for getting a definite plan for a definite period into one's mind and keeping it there until the end of the period, even though you know that adjustments will have to be made to it as time runs on" (p. 46).

The passage I have just quoted is the only case where I found Mr. Edey's views less than completely persuasive: otherwise he manages not only to anticipate all the queries which occur to an economist-reader but also to provide answers which struck me as well balanced and reasonable. I therefore commend his book "not merely to newcomers to economics but also to old hands seeking an introduction to Business Budgets and Accounts."

BRIAN TEW

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Introduction to Operations Research. By C. W. CHURCHMAN, R. L. ACKOFF and E. L. ARNOFF. (New York: John Wiley (London: Chapman & Hall), 1957. Pp. 645. 96s.)

ECONOMISTS have become accustomed to the rate of growth in terminology exceeding that of the subject-matter with which it purports to deal. Small wonder, then, that one approaches a book on "Operations Research" with some misgivings. But reassurance comes quickly, for these two words singly or in combination stand not for yet another theoretical variation but for an attempt to apply existing techniques, suitably modified, to the practical solution of business, military and government problems. As befits such inquiries, the approach is eclectic. More significantly the emphasis is on research by teams composed of a number of specialists. The potential gains from specialisation are applied to the discipline of research itself.

The training grounds of operations researchers would appear to be mathematics, engineering, natural science, psychology and economics, together with the administrative units to which the study refers. If this book is representative of the field of operations research, there is a clear indication that mathematics has an important part to play.

The feature common to all the problems is that of adapting available means in the best manner to attain the specific ends of the interest group concerned. An economist might therefore be surprised that there is no discussion in terms of utility, preference systems, opportunity cost and the like. But the three authors of this volume do not include an economist within their number though one is well aware of much economic sense and judgment. This is particularly apparent in the chapter on "Weighting Objectives," which overlaps the problems economists have been grappling with in welfare economics.

The writers recognise that these objectives are largely non-economic and often non-quantitative and go on to suggest that since, for useful solutions, some weights must be attached this is best done clearly and specifically. But at this point the argument becomes blurred. Methods for the derivation of weights are advanced which could lead the less-discerning reader to conclude that these weights are a good deal more objectively based than in fact they are.

There is also too great a confidence in the ability to quantify such aims as "retention of family control in a business." The problem is correctly framed, but there is too much impatience to produce answers. Fortunately in practice the problems with which operations researchers have been most successful are those where it has proved possible to minimise or eliminate the problem of weighting. Attention is focused on the choice of optimum methods of organisation within firms rather than within society or industry.

Analysis accompanied by practical illustration is provided for five

different classes of problem: inventory models and the choice of optimal inventory policy; linear programming and allocation policy; queueing models, an aspect of the theory of stochastic processes, and their application to traffic and pipeline congestion; replacement in theory and practice; games theory and its relation to the explanation of competitive bidding. In each case the treatment is well graduated and the argument lucid, though the non-mathematician will rather quickly find the going heavy. This is no bad thing. The tools used and the clarity and relevance of the many practical examples are such that some investment in the necessary mathematics will prove worthwhile.

Of more immediate concern are the conclusions that may be drawn by the more mathematically minded. To illustrate, in the chapter on replacement models the correct timing of replacement is analysed in terms of the minimisation of cost under conditions of known life and effectiveness, on the one hand, and on the other, in terms of the probability distribution of the effective lives of machines. At no point is any attention given to expectations of demand, which may have marked effects on the choice between machines with different lifetimes. Hence, it is not surprising to find the name of F. A. Lutz does not figure in the extensive bibliography.

What, then, does this add up to?

This is an important book, full of fruitful ideas, and useful techniques with a growing field for application. But two things are clear; the economic student must be well-versed in his discipline before digesting the contents, and the operations research team on any project should most certainly contain a well-trained economist.

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MALCOLM R. FISHER

The Motor Industry. By G. MAXCY and A. SILBERSTON. (London: Allen & Unwin, 1959. Pp. 245. 25s.)

THIS is a book of much wider interest than its title indicates. It is not only that everyone can claim some familiarity with the industry and its products so that it provides acceptable illustrations for text-books. The motor industry is, in a sense, the archetypal industry of the early twentieth century in much the same way that the railway mirrored the forces of industrial development in the mid-nineteenth century and cotton those in the late eighteenth. The motor car is the typical durable consumer-good and is of special interest, therefore, in demand analysis. It is produced by techniques extending from the craftsman and the small workshop to the automatic factory, and illustrates both the progress of technique and the influence on it of scale of production. Since technical change is rapid and producers are few in number, it also provides a useful case-study in oligopoly.

The demand for motor cars is already the subject of a considerable

literature, but there is a lack of studies concentrating on the circumstances governing supply. The authors furnish a very readable account of the structure of the industry and of the workings of competition in it. But they do much more: they attempt to give quantitative expression to such concepts as the technical optimum and the short- and long-run cost curves of the firm. Both curves, although downward sloping, show too gradual a fall in relation to the presumed elasticity of demand to make price-competition a worth-while gambit. In the short run, prices tend to be kept stable or to be adjusted in line with the variable cost of labour and materials; in the long run, competition takes the form of trying to hit the public taste more successfully with a new model at the going price.

The authors suggest that, in the short run, variations in output have relatively little effect on unit costs; for a weighted average of cars, vans and trucks, fluctuations between 20% above and 40% below "capacity" are unlikely to cause standard costs to vary by more than 5% either way, although for a single model the curve may show a slightly steeper slope. This is taken at other stages in the argument to show that fixed costs are not particularly heavy; they come to only 16%, at standard volume, for the firm supplying the particulars, and to about 13% at full capacity. It is, however, a little misleading to quote ratios of this kind, since the variable costs of the car manufacturer are equally low, and nearly 70% of total factory cost in the example given represents bought-out parts and components. Suppliers have their own fixed costs; but do they quote lower prices when demand falls off? The authors hint, when discussing the advantages of vertical disintegration, that they may do so; they do not raise the issue in relation to the short-period cost curve.

The long-run cost curve seems to fall more steeply than the short at outputs under 100,000 a year and less steeply at outputs above 200,000. The authors argue that there are technical economies in body pressings (and perhaps some components) up to outputs of 1 million a year, in machining up to the half million mark and in assembly up to about 100,000. They give figures, supplied by one of the larger firms, that imply a fall in unit costs of only 13% through a fourfold expansion from 100,000 to 400,000 a year. But this estimate is certainly too conservative: for example, it shows no fall in fixed overheads after 300,000, no reduction in variable indirect costs (supervision, maintenance, etc.), and takes no credit for economies accruing to suppliers as a result of the expansion. It is not altogether clear to the reader whether the curve is supposed to relate to an integrated firm making its own bodies, an ambiguity of no little importance when the cost of the body is one-third of the factory cost of the car and the major economies of scale at volumes in excess of 100,000 are in body-building. If the curve does relate to an integrated firm it is difficult to fathom what is included in the 70% of total cost that goes on components and materials; but if it is not, then the curve seriously underestimates the technical economies of scale.

The authors themselves suggest that doubling output to 200,000 units would achieve a saving of 10%, and a further doubling to 400,000 would increase this to 15%: if the output of the *industry* were expanded fourfold they would expect a reduction in costs of one-fifth to one-quarter. It is not altogether easy to reconcile these estimates with the fall in real costs per vehicle between 1930 and 1937, which they put at 35% (for a doubling of output), or the subsequent fall of perhaps 20–25% between 1950 and 1955 (when output increased by 50%). No doubt, as the authors emphasise, the average size of car was declining in both periods and there was also some progress in the technique of manufacture independently of the effects of expansion. Nevertheless, the reader may feel that on one of the major issues the authors are under-stating their case. He may also feel that they leave him with a very vague idea of the proportion of British output consisting of models in the 100,000-a-year class, and no idea at all that the scale on which the Volkswagen is manufactured is 3,000 a day.

There are a number of other questions to which one would like a more definite answer. If a firm produces a successful model, why does this not give it a cumulative advantage, even at moderate demand elasticities? Why do some models succeed while others fail? Why do the Volkswagen and Dauphine occupy a place in their own countries that no similar car occupies in Britain? If variety is costly, what elements of variety cost most, and are they the elements that most enhance the appeal of the car? It is possible to assemble 1,000 cars a day, each different from the others, at no appreciable extra cost, provided they use the same jigs, spraying sheds and so on; but it is not possible to give them all a different silhouette. Could standardisation be carried further without injury to sales? Or are manufacturers the prisoners of their own marketing organisation, forced to imitate their rivals and yet offer something recognisably different? At what rate is the technical optimum expanding from one decade to another, and is the lag of British firms behind the optimum growing or contracting?

If these questions are not answered it is not because they have escaped the authors. They have done a workmanlike job and give to the non-technical reader a vivid picture of the things that matter to those who manage the industry.

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A. K. CAIRNCROSS

Effects of Mergers: Six Studies. By P. LESLEY COOK with the collaboration of RUTH COHEN. (London: Allen & Unwin, 1958. Pp. 458. 42s.)

Most studies of industrial combination are concerned with a description of the processes of change and of the forces that give rise to them. The authors of this book, while providing a useful account of the organisation of six industries over a long period, also try to estimate what might have happened in the absence of the mergers they describe and, on the basis of

this judgment, to reach a conclusion about whether the public interest would have been better served by any likely alternatives. Investigators of this type of problem usually limit themselves to a judgment of whether, *at a particular point of time*, the organisation and behaviour of an industry are open to objection on the grounds of public interest. They may extend their judgment beyond the present and speculate about the future effects of the monopolistic organisation under review. In doing so they may have to take account of operations in the recent past. But they are not usually called upon to consider how a situation, which is the result of a chain of decisions in the past, might have differed if the initial decisions had not been taken. Indeed, since such decisions are irrevocable, the alternative situations that might have arisen are irrelevant to any criticism of the present organisation. However, the task which the present authors have set themselves is much more ambitious than that of inquirers into contemporary industrial organisation, for they have sought to reconstruct a series of historical situations and to judge successive decisions over a long period of time. This approach brings a lively interest to their study, even though doubts may sometimes be entertained about the validity of particular reconstructions.

In the case of the cement industry, the authors conclude that, in the absence of mergers, concentration in relatively few firms would ultimately have occurred by the expansion of the more efficient, but that the elimination of other firms would have been long delayed because of market imperfections. This would probably have kept costs higher than they actually were, and prices would have fluctuated more widely. Profits would have been lower if competitive conditions had prevailed, but in all probability restrictive cartels would have been established from time to time with unfavourable effects on both costs and prices. On balance, it seems unlikely that any alternative course of development to that actually followed would have yielded substantial advantages from the standpoint of the public interest.

For calico printing the same conclusion is reached, though with some hesitation. Again, the authors are impressed by the fact that competition (in the absence of the combine) would have been too imperfect to eliminate quickly the less efficient firms. They seem to give insufficient weight to the industry's arrangements for curbing price competition and sharing output, arrangements which might have been less effective in the absence of the C.P.A. The authors' conclusions about the soap industry, where "bigness benefited the consumer," are also debatable and do not emerge clearly from their description of the course of events. It is shown that for many years the Lever combine failed to take advantage of its power to rationalise production and sales, and that selling costs, in consequence, were unnecessarily high. They refer to the merits of size in connection with research and development, but they fail to observe that many innovations in soap-making came from the Italian industry, which is composed of a large number of competitive firms. In the case of the glass and motor industries, mergers

played a minor part in the evolution of a highly concentrated structure, and the discussion, therefore, resolves itself largely into an examination of the merits of the economies of scale in those trades.

In general, the authors find that the disadvantages of mergers lie in the short-run effects on prices, and that these are offset by the elimination of inefficient firms and capacity, by the reduction of the waste that attends imperfect competition and by the technical and commercial economies which large firms can promote. Mergers, they say, are unlikely to be successful unless there are considerable economies of scale, and they are unlikely to take place unless these are difficult to achieve without mergers. In other words, mergers can be regarded as a method of bringing about a "higher organisation" in an industry (to use Professor D. H. McGregor's term), and in the industries under review the authors think that the mergers have achieved this result. Their conclusion, however, does not mean that competitive forces have been inconsiderable factors in promoting progress, for "long-period price competition" has seldom been "stifled to any important degree." It may be thought that the survival of competitive pressures in these industries was a condition of their continued economic efficiency, and that if attention were to be concentrated on the short periods in which the industries were able to escape from these pressures the verdict would be less favourable. This is borne out by the authors' criticisms of the mergers at certain stages in their growth. It is not to be expected that outside criticism can often strike at the foundations of a long-established industrial organisation. Such criticism can be most usefully applied to an industry at a moment when its organisation is in process of change and to particular aspects of its behaviour. Probably the chief value of the book lies in the chapters where these detailed criticisms are proffered rather than in the final verdicts.

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G. C. ALLEN

Malaya: A Political and Economic Appraisal. By L. A. MILLS. (Minneapolis, Minn.: University of Minnesota Press (London: Oxford University Press), 1958. Pp. xi + 234. 30s.)

THE author of this book, who is Professor of Political Science in the University of Minnesota, is already well known for his writings on South-East Asia; this addition to them is to be welcomed.

Professor Mills' first five chapters are mainly in narrative form; and the story that they tell is primarily that of recent political and constitutional developments in Malaya and Singapore, but at intervals there are comments on economic topics. Many of these are interesting and refreshing: Professor Mills is, for example, more critical than most authors of the role of the rubber companies and of the planters during the 1920s. His economic

paragraphs briefly sum up the story of the growth in Malaya of commercial agriculture and of a rich entrepot trade, and show the effects of these developments on a Malay population who had traditionally been subsistence rice farmers and fishermen.

The first of these five narrative chapters takes the story up to the outbreak of the war with Japan. The second is concerned with the "change and reconstruction" following the Japanese surrender: here Professor Mills handles critically, but fairly, the astonishing episode of the MacMichael mission and its attempt to combine all the Malay States into a Malayan Union; and he rightly points out that it was this episode more than anything else that "wrought the miracle of making the Malays lose their indifference to politics." Chapter 3 is entitled "The Communist Rebellion"; it starts with an account of the first, infiltrating phase of Communism in post-war Malaya, and passes on to the emergency of 1948, to the Briggs Plan, and to the Templer régime. In the case of the last two topics there is a tendency to stick a little too closely to narrative; Professor Mills does not allow himself to be enticed sufficiently far into comment and criticism, and his treatment of the "New Villages" hardly lives up to the promise in the publisher's blurb that "it may well be controversial." He is, however, interestingly critical about the virtual suppression of the Secretariat for Chinese Affairs, an action which is said to have deprived the Government at a vital moment of invaluable links with Chinese opinion, feeling and sources of information. Chapters 4 and 5, on "Communalism and Self-government" and "*Merdeka*" respectively, deal with moves towards political independence in post-war Malaya against the background of a plural society in which political parties tend to be communal in character; it takes the story up to the formation of the Malay-Chinese Alliance party and the coming of independence to the Federation of Malaya on August 31, 1957.

Chapter 6 departs a little more from strict narrative form than its predecessors, and considers the special economic, social, political and strategic problems of Singapore. Chapter 7 follows naturally, since it is concerned with Malaya and Singapore in Commonwealth defence.

Professor Mills concludes with two chapters on economic affairs, which give a very useful and readable summary of the present state of the rubber-planting industry in relation to such considerations as the probable future demand for natural rubber, the tendency for capital to be discouraged by the political climate, and the taxation structure in Malaya. The discussion of other facets of the Malayan economy is rather less adequate, and there is hardly any mention of the problems of rice production and peasant farming (apart from rubber small holding).

Professor Mills' book is a useful summary of the recent political and constitutional change in Malaya and Singapore, though it stops just short of very recent events in the latter. His account is fair, readable and generally

accurate. But it is not in the nature of this book to be profound or analytical, or to discuss such general issues as the suitability of representative government in a plural society or the impact of social change on politics. His economic comments are often shrewd and interesting, and his book can be read with profit by anyone seeking an introduction to Malayan economic affairs. But it is hardly a book for the expert on Malayan economics or on the economics of an under-developed society.

B. H. FARMER

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Land Reform in Japan. By R. P. DORE. (London: Oxford University Press, 1959. Pp. xvii + 510. 55s.)

Essays on Japanese Economy. By SHIGETO TSURU. (Tokyo: Kinokuniya Bookstore, for the Institute of Economic Research, Hitotsubashi University, 1958. Pp. 24.)

THE dispossession of landlords, especially absentee landlords, and the transformation of tenants into peasant proprietors are regarded by many people as the solution to rural problems and the key to greater agricultural output. There may be excellent social and political reasons for land reform of this type. It is much more doubtful whether it will lead to greater efficiency and increased productivity. Japanese experience provides a useful testing ground for the student of land reform. Before the War Japan had a mixed system of tenants and peasant-owners under which considerable agricultural progress was made. But there was increasing dissatisfaction among the tenants in the inter-war period that they were receiving less than their due share of the national bowl of rice. Measures of rural relief by a sympathetic government in the 1930s reduced peasant discontent but left unsolved the basic tenancy problem. The post-war occupation authorities, with the support of many Japanese, implemented their policy of "democratisation," not only by dispersing the *Zaibatsu*, deifying the Emperor and encouraging the trade unions, but also by carrying out a widespread reform of the land-tenure system. The background, implementation and effects of this revolutionary institutional measure is the theme of Professor Dore's scholarly study.

The author has used Japanese written sources and, equally important, personal observation in the villages, to give us an exhaustive and authoritative account of this subject. Although his sympathies generally lie with the tenants, he discusses with balance and insight such emotive topics as the contribution of landlords to agricultural development. Among them were enough "energetic and keen agriculturalists . . . to contribute a great deal to the increases in productivity which took place in Japan after the Meiji restoration." He makes clear that the system of land tenure was only one

among many factors responsible for rural distress and is equally careful to avoid dogmatic statements on the causal relations between rural distress and military aggression. Much of the book is devoted to the social and political structure of the villages. His detailed case studies of households and hamlets at times interrupt the flow of the narrative, but they enliven the text and provide a salutary reminder of the dangers of generalising about Japanese rural life.

The changes in land tenure have been hailed as the most successful reform in Japan since the War. One can justly criticise many writers, including Professor Dore, for not stating rigorously the criteria by which this success is gauged. The impression I get is that a politically quiescent peasantry was one of the major reasons behind, and one of the main beneficial effects of, the reform. The economist will judge its success in terms of, *inter alia*, utilisation of resources, productivity and the contribution of agriculture to general economic development. That there have been remarkable improvements in agriculture and in rural living standards in recent years is clear. To a large extent these are to be attributed to government support and abnormal post-war conditions rather than to changes in tenure. The reform has not removed the basic problem of scarce land and capital relative to labour. In fact, it has reduced the average size of holding. In his chapter entitled "Permanent Reform or Temporary Interlude" the author also discusses the difficulties of preventing land alienation and the revival of tenancy in a future period of depression.

Professor Dore's book must certainly rank as one of the most significant contributions in English to Japanese economic history. It is to be hoped that in a few years time, when the new system has been operating longer and when the effects of cheaper food imports and reduced government aid may be felt, the author will give us a further assessment of the role of land reform in the Japanese economy.

Professor Tsuru is one of Japan's best-known American-trained economists. This volume is a collection of eight articles previously published in Western and Japanese journals and, like the curate's egg, it is good in parts. Part I contains six essays on post-war problems of the Japanese economy. Although these were important and topical at the time of writing, many of them are now dated. The most valuable is the article on "Employment in Japan: Problems and Prospects," which focuses attention on the difficulties of absorbing the rapidly growing labour force and stresses the need to increase exports if full employment is to be achieved and maintained. There is also a useful study of macro-economic planning in Japan, illustrated with many statistical tables. In Part II there is a long essay, written in 1941, on "Japan's Economy under the Strain of the China Incident." This was admittedly penned without adequate statistical data. It illustrates the dangers of making economic prophecies, for the author believed that by 1941 the China war had almost exhausted the Japanese

economy. The second historical essay on "Economic Fluctuations in Japan 1868-1893," reprinted from the *Review of Economic Statistics*, Vol. 23, No. 4, is the best known and most important in the book. It is a concise, well-illustrated analysis of the cycles which occurred during the vital formative period of Japanese economic growth.

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Furness and the Industrial Revolution: An Economic History of Furness (1711-1900) and the Town of Barrow (1757-1897). By J. D. MARSHALL. (Barrow-in-Furness: Barrow-in-Furness Library and Museum Committee, 1958. Pp. xxii + 438. 23s.)

FOR much of the period covered by this study, Furness lay outside the main stream of industrial development, although its coastal position and the early development of the charcoal-iron industry meant that it was subject to a variety of external influences. In the mid-nineteenth century, however, the coming of the railway and the establishment of the Hindpool iron works transformed the area. For twenty-five years the history of Furness, and more particularly the town of Barrow, is an admirable example of break-neck industrialisation. Then, in the last quarter of the century, the growing use of non-phosphoric and imported ores, and the subsequent exhaustion of the hæmatite mines, brought economic difficulties from which the area was rescued only by the development of the naval shipbuilding yards at the very end of the century.

There is much in this volume to interest the student of railway history and of the general industrial history of the nineteenth century: Dr. Marshall tells his story with a wealth of detail and makes full use of the available statistical material, and he certainly casts his net unusually wide. For example, he traces the consolidation of landownership in the eighteenth and early nineteenth centuries and the increasing social stratification which accompanied it—a topic of peculiar interest in an area which at the beginning of the eighteenth century could still boast a predominantly peasant society. Nor does he neglect the human aspects of his subject: considerable attention is devoted both to the part played by individuals in the development of the district—though the author is certainly more critical of the activities of the great industrialists than is at present fashionable—and to the effect of social change on the life of the people.

On the whole, Dr. Marshall takes a decidedly pessimistic view of the human consequences of industrialisation, and this reviewer, at least, finds much of his argument compelling. It must be confessed, however, that his account rests heavily on evidence connected with housing and educational facilities.

general social conditions and mortality rates, and rather less is said about purely economic desiderata. The local materials for a study of wages and prices are admittedly scanty, but what Dr. Marshall has to say on this score is not entirely above criticism. He points out, for example, that the deflation of local wage-rates by Silberling's price index gives a spurious impression of rising real wages after the Napoleonic Wars, and I am not altogether clear, therefore, why the fact that wages lagged behind the index during the war period should apparently be accepted at its face value, despite the evidence of increasing expenditure on the poor. (Incidentally, the author's comparison of wages in eighteenth-century Furness with those in other districts is somewhat misleading: the reference on p. 40 to the movement of wages in the "country at large" should be to the *county* (of Lancashire) at large, and the error is repeated in a quotation from Mrs. Gilboy's study of eighteenth-century wages in a footnote on the same page.)

Some of the most interesting, and perhaps also the most controversial, pages of this study are those devoted to demographic questions. Dr. Marshall thinks that the population upsurge in the late eighteenth century should be mainly attributed to a steadily depressed death-rate. It is difficult to demonstrate the point statistically, and the figures he quotes in fact suggest that the birth-rate rose and the death-rate fell by roughly the same amounts. This is not surprising, since the population estimates on which the rates are based were apparently derived from the figures of baptisms and burials on the assumption that changes in birth- and death-rates tended to cancel each other out. In so far as there was a decline in mortality, however, he evidently considers that it took place in all age-groups, and not primarily among juveniles, because burials of "sons" and "daughters" constituted roughly a third of the total number of burials recorded in the registers throughout the century. But he has surely overlooked the probable effect of increasing numbers on the age structure of the population. During the eighteenth century, the average number of baptisms more than doubled, and unless we assume that the total population grew equally fast—which would imply that the rate of growth in Furness was as high as in the rest of Lancashire, and very much above the national average—the figures would certainly suggest that there was a *relative* decline in mortality among the younger age groups in the population.

This point illustrates both the strength and weakness of the volume. For it could be argued that by casting his net so wide, Dr. Marshall has attempted too much. Those who are less interested in the detail of local history may wish that he had concentrated more on a selective and comparative analysis of some of the outstanding problems of general interest suggested by the history of the area, even at the expense of a fully comprehensive chronological treatment. But few will doubt the importance of the evidence provided by the local investigator in contributing to the solution of wider questions, and the Barrow Committee is to be congratulated on its enterprise.

in making available a valuable addition to the stock of local studies which are helping to enrich our knowledge and understanding of the past.

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History of the Brewing Industry in England, 1700-1830. By P. MATHIAS.
(London: Cambridge University Press, 1959. Pp. xxviii + 596.
85s.)

THIS is a work which deserves praise from every point of view—as a piece of sustained, critical research, as a valuable contribution to the study of the Industrial Revolution, as perhaps the first scientific investigation of the development of the brewing industry in this country. In this latter context its value is greatly enhanced by the fact that the author has had access to unpublished material and to the papers and records of some of the most famous brewing firms which tradition and caution have hitherto kept hidden—and it is to be suspected largely forgotten—in the companies' vaults. Even from this point alone the book marks a stage in the study of this industry in providing so much material for further use. How much more does the author achieve and where do we arrive? The book, for all its 600 pages, is a section only of the problem, a section in time and a section in place; that is to say about a hundred years in time, and virtually restricted to London in place. There are obvious reasons for this restricted approach, and it is in the pattern of modern historical research; it may indeed be argued that anything else is virtually impossible. But it has to be realised that such an approach must involve the great danger of over-emphasis and of distorted perspective.

Very briefly the theme of the study is that "porter" was invented at the beginning of the eighteenth century; that porter was particularly and uniquely suitable for large-scale brewing; that the problem and cost of transport and the poor keeping qualities of ale or beer as then known restricted large-scale brewing to areas where a large market was immediately at hand; and that therefore London was the obvious place in which large-scale development could alone and did first take place. In due course, a century later, improvements in knowledge and technique made it possible to treat ale on the same lines as porter; and the revolution in transport in the 1840s changed the whole market position. In other words, in the author's views porter and porter alone made possible that expansion of large-scale brewing that started and developed in the eighteenth century. It is an interesting suggestion, but it is doubtful if it can be substantiated. That large-scale development and all that goes with it occurred is undoubted—but can one name porter as the cause rather than the subject?

It is here that we see clearly contrasted the advantages and the disadvantages of taking a limited period in such a survey. The author is between a dilemma of his own creation. The germs of expansion in the brewing industry were in fact already there and working; new markets were inevitably developing that not only justified but also required wholesale production; an ingenious brewer produced the new "porter," a more uniform and better product than much that was then being sold; and it caught the public taste. At once others followed, and porter became the rage. One can perhaps see the first stages of this same thing happening to-day in regard to lager beer.

This in no way minimises the value of the author's meticulous account of how large-scale brewing developed in the eighteenth century; the problem first of all of a seasonal industry with its subsidiary problems of the financing of heavy outgoings at one period in the cycle and the handling of heavy accumulations of cash at another period. The growing needs of an expanding industry, the over-optimism of individual brewers, the effects of normal trade fluctuations and of abnormal crises due to wars and scarcity; all would have brought in the problem of finance and the need of the banker. But in this case the position was different from other industries developing in this same period, in that the brewing industry provided surprising amounts of largely short-term cash for use elsewhere. It was this that above all produced that close link between banking and brewing that marked the latter half of the eighteenth century and the early years of the nineteenth.

Equally noteworthy, as the author emphasises, was the interdependence of brewing and two important agricultural activities—barley-growing and hop-growing. No one has indeed ever devised any other use for hops—apart from the old-fashioned sleep-producing pillows of our grandmothers; and brewing came in time—but not in the period of this study—to dictate to a large extent the types of barley to be grown, and to provide a most profitable and assured barley market. It is, however, only in this century that in fact the brewing industry has materially contributed to the scientific side of agriculture.

The author is wholly right in claiming brewing as an important item in the industrial revolution; but it is difficult to accept his view that it led the industrial revolution. It is certain and interesting that it was perhaps the most notable example of that revolution in the south of England, and in particular that it linked London with the Industrial Revolution in a way that has not been recognised. The position of London as the centre and heart of the new industry of brewing is indicated by the fact that, although brewing was spread all over the country, it was not till 1822 that the country brewers organised themselves as a representative body; and then it was due to the fact that London brewers were having an altogether disproportionate effect on legislation and administration that affected the whole country,

where there were rapidly developing markets of their own in the Midland and the North.

The author gives some very interesting figures of the early growth of the tied-house system. The policy is seen to date from a much earlier period than the ordinary man in the street or the present-day critic realises. Barclay Perkins, for instance, had over 500 tied houses in 1830, the end of the period under survey, and the three biggest London brewers had then about 1,400 tied houses between them. This pattern was at this period probably largely limited to London, but even so, it would be difficult to think of any other industry in this or any country where this type of direct link between supplier and retailer had proceeded so far. What is interesting is that in all this period and indeed during the whole of the nineteenth century this "tie" went largely unnoticed and uncriticised; it is only in this generation that it has attracted so much notice, due no doubt to the fact that now something like 95% of ordinary public-houses are "tied." The author records the developments and gives the motives and reasons that influenced this policy of the big London brewers; he does not attempt, except perhaps by remote implication, to express any view on the soundness of the policy, nor does he debate it from the broader economic point of view. Neither aspect can be ignored.

The system of controlled outlets is curiously enough the only direction in which integration has played a major part in the brewing industry. The author devotes chapters to malting, barley growing and hop growing. In malting practice varied greatly, some brewers did a large part of their own malting, others did none; but over the whole industry malting remained very largely in independent hands, while barley growing has always remained almost wholly independent; and hop growing has been indulged in by only very few brewers.

Beer has always—at least in the last three centuries—played a part in national life that in two ways differentiated it from other industrial activities. In the first place, as the author points out, its contribution to the national exchequer has always been important and at times fundamental. In the mid-1730s the malt tax and the beer tax between them contributed a quarter of the total national revenue from taxation. In 1806 malt beer and hop excises brought in almost one-fifth of the total revenue of Great Britain drawn from taxation. Why beer and alcohol have always been a major objective and victim of the Revenue may be a matter of some wonder, but it is not irrelevant, as the author points out, that the brewing industry was found to "tolerate increments in the rate of excise more successfully than any other commodity." Beer still contributes very substantially to the national revenue and still fairly successfully survives the heavy taxation load put on it.

The other point the author rightly makes is that beer has always had a too conspicuous position from the social and moral point of view. In the

latter part of the period covered by this book beer had become the temperance reformers' most effective weapon, both in this country and in Ireland, in their efforts to stem the growth of gin drinking. Since then we have seen the reformers attack the stalking-horse itself; and whatever individual views may be, it has until comparatively recently been the apparent accepted public view that there must be strict, and ever stricter, control on the retailing of beer. This is not an aspect to which the author devotes direct consideration, but he points out the economic fact that the excise duty had in the period under review, as it has always had, an important and a disproportionate effect on the pricing policy of brewers and publicans. It has tended to obscure the competitive aspect and certainly to obstruct pricing policy.

All this but tends to illustrate what a complicated subject the history of brewing really is; this book, long as it is, has not perhaps been able to do more than set the stage for a detailed and critical consideration. When that is done it would seem to the reviewer that a clearer and a truer picture will be obtained by eliminating to a large extent from the account the ancillaries—the brewing materials, the incursion of individual brewers into politics and the like; and at the same time lengthening the period of review. There is really no argument for stopping at 1830, except the untenable argument that there was a recognisable line between the "Age of Porter" and the "Age of Ale." But while making that comment one must again say what a remarkable achievement this book is and what a valuable contribution it is to a hitherto largely unstudied subject.

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NOTES AND MEMORANDA

A CASE STUDY OF RESPONSE TO PRICE IN AN
UNDER-DEVELOPED COUNTRY

I

THE extent of the response of agricultural producers in under-developed countries to changes in prices and price relationships bears on a wide range of issues of analytical and practical significance, including among the latter problems of public finance and of price and income stabilisation.

There are, however, well-known and serious difficulties in measuring the degree of responsiveness of producers to price changes. There are the familiar problems arising from the usual absence in the real world of anything resembling closely the *ceteris paribus* of the theoretical formulations of functional relationships in economics. There are further difficulties created by the time lags between changes in agricultural capacity and changes in output; and also by the effects of uncertainty about the permanence of absolute and relative price changes. The problems of testing a hypothesis or of measuring the strength of a functional relationship make it difficult to reach objective assessments, and rival hypotheses are likely to flourish side by side, often deriving from opposing policy preconceptions and sometimes giving rise to opposing policy prescriptions.

Though certainly not unknown, situations exhibiting sharply the response of producers to a change in an economic variable are infrequent, and have rarely been described. Situations deliberately induced by the contrivance of changes in the variable, that is, where, in effect, an hypothesis has been postulated and put to the practical test, are even more unusual. There is, therefore, some general interest in the policy of some of the statutory marketing authorities in Nigeria of raising the quality of certain agricultural produce by offering price incentives to peasant producers. This policy was explicitly premised on the hypothesis that peasant producers respond to price differences, and would appropriately adjust their behaviour as producers to the incentives provided by the authorities. The results of this experiment supported the hypothesis in that the average grade produced has risen greatly. The explicitly formulated hypothesis, the introduction of the measures and the consistent results closely resemble the methods of observation and experimentation in the natural sciences. This note describes the measures taken by the authorities, and the response of producers; thus, in effect it reports on the process and the results of a series of contrived experiments.

II

All major agricultural exports of Nigeria are handled by statutory export monopolies (marketing boards) established during and after the Second World War. Two of the marketing boards, the Nigeria Cocoa Marketing Board and the Nigeria Oil Palm Produce Marketing Board, prescribed wide differentials in the prices paid to producers of cocoa and palm oil for various grades of these products in order to encourage the production of higher grades.¹ These differentials in producer prices greatly exceeded the grade differentials in the markets in which the Boards sold the produce.² The producer prices and grade differentials were announced before the opening of each season, so that there was no price uncertainty; and the agents of the board had to buy all supplies offered to them at not less than the announced prices. The producer therefore knew the additional returns for improving his output by more careful tending of the trees, gathering of the produce and preparation for sale to the boards.

The purpose of the prescription of wide grade price differentials and the results of this policy are frequently referred to in the annual reports of the two boards. Here are two extracts from the annual reports of the Nigeria Cocoa Marketing Board:

"It is quite certain that, given an adequate cash incentive and the necessary instruction, the Nigerian farmer is capable of producing well-fermented cocoa. That he has not always done so in the past is attributed largely to the fact that the premia offered were too low, for it should be remembered that the fermentation process involves a loss in weight in comparison with beans which are dried only and not fermented" (Nigeria Cocoa Marketing Board, *Second Annual Report*, Season 1948-49, p. 22).

"That the Board's policy of offering a premium on first grade quality cocoa has resulted in an ever-increasing effort to produce good

¹ The Nigeria Cocoa Marketing Board was established in 1947 and the Nigeria Oil Palm Produce Marketing Board in 1949. They succeeded the West African Produce Control Board established in 1942, which also had a statutory export monopoly. In 1954 the boards were placed on a regional as distinct from a product basis. The regional boards, each of which handles several crops but only part of the total export of each crop, continued the policy of their predecessors in paying wide price differentials for different grades with the same result as experienced by their predecessors. For convenience of presentation we have confined the discussion to the period before the regionalisation of the boards in 1954. The tables in this paper cover the entire period of operation of the two national boards, spanning the interval between their predecessor, the West African Produce Control Board, and their successors, the various regional boards.

Measures to improve quality by the offer of price inducements were consistently applied only in respect of cocoa and palm oil.

² We are not concerned here with the merits of these policies, which are wasteful, since they encourage uneconomic expenditure of effort, discourage economic expenditure of effort and also waste output already produced. This is particularly evident in the refusal to buy so-called sub-standard produce even when this has a positive commercial value. We have analysed the economic implications of these policies in an article "The Economics of Marketing Reform," *Journal of Political Economy*, June 1954.

2. Grade differences: Grades were differentiated on the basis of the proportions of unfermented or insufficiently fermented beans and of defective beans.
3. The producer prices in this table refer to those paid for main-crop cocoa which accounted for over 90% of total supplies. The small quantity of light-crop cocoa, harvested later in the season, was subject to a discount of £5 per ton for all grades (somewhat smaller in 1947-48), until parity of treatment was introduced in 1953-54.

TABLE II
Nigeria Oil Palm Produce Marketing Board Producer Prices for Palm Oil and Composition of Purchases by Grades, 1949-54

	Special Grade.			Grade I.			Grade II.			Grade III.			Grade IV.			Grade V.			Total pur- chases, '000 tons			
	A £.	B £.	C %.	A £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.	A £.	B £.	C %.					
1949	No special price			42.75	66.4	37.13	—	5.62	14.5	33.0	—	9.75	12.7	29.63	—	13.12	6.0	26.25	—	16.5	0.4	161.5
1950	53.0	+10.25	0.2	42.75	61.3	37.13	—	5.62	17.8	33.0	—	9.75	14.4	29.63	—	13.12	5.8	26.25	—	16.5	0.5	138.7
1951	71.0	+16.0	6.3	55.0	70.8	43.0	—	12.0	11.4	34.0	—	21.0	7.5	30.0	—	25.0	4.0					135.2
1952	80.0	+19.0	29.6	61.0	56.3	47.0	—	14.0	7.4	35.0	—	26.0	5.2	30.0	—	31.0	1.5					178.5
1953	75.5	+17.5	50.4	58.0	38.0	45.0	—	13.0	7.2	34.5	—	23.5	4.4									211.6
1954	65.0	+15.0	60.8	50.0	29.9	38.0	—	12.0	4.4	33.0	—	17.0	4.9									205.0

Notes

1. Abbreviations:

A. Producer price of grade, in £ per ton.

B. Difference between price of grade and price of Grade I, in £ per ton.

C. Purchases of grade as a percentage of total purchases made by the Board.

2. Grade differences: The original five grades were based on the free fatty acid (f.f.a.) content of the oil, ranging from 0 to 9% f.f.a. content for Grade I by equal steps to 36-45% f.f.a. content for Grade V. This classification was maintained, except that in 1952 the permissible range for Grade IV was narrowed to 27-33%. The Special Grade refers to "edible" (as distinct from the other "technical") oil, with a low f.f.a. content (up to 4½%).

3. The table refers only to palm oil produced by smallholders, and not to plantation palm oil.

Special Grade of oil at a special premium price.¹ The extent of the improvement in the produce bought by the Board is apparent in the statistics. Thus in 1954 purchases of the Special Grade constituted as much as 60% of all purchases, while, as for cocoa, purchases of the two lowest grades were discontinued after they had fallen to small quantities.

Another experience of the Boards' operations, which cannot be presented in the same way, also shows clearly the awareness of producers of economic opportunities and their readiness and eagerness to take advantage of them. Whenever higher prices were announced or even generally anticipated for the following season, producers and intermediaries withheld supplies in the closing months of the previous season, while supplies were rushed forward when a reduction was announced or anticipated. This is reflected in statistics of monthly purchases by the Boards, and was sometimes mentioned in their annual reports.²

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WELFARE AND EXPANDED CHOICE— PROOF OF THE GENERAL CASE

MR. NEWMAN's comment³ upon my earlier article⁴ includes a request for a proof of the optimality of free exchange in the many-person, many-commodity case. The chief aim of this note is to comply with Mr. Newman's request, after first giving a more general and rigorous statement of my welfare criterion than in my original article.

My original welfare criterion was conceived in terms of, and is entirely satisfactory for, straightforward situations of choice subject to a budget constraint. It was, however, pointed out by correspondents⁵ that my criteria were not satisfactory for some situations (in particular, those involving non-connected sets of opportunities). The present statement is couched, I believe, in both the most exact and the most general form of which the proposition is capable of so being.

¹ Price differentials in favour of the Special Grade were introduced towards the end of the previous year, in which purchases of this grade constituted 0.2% of total purchases.

² The willingness of peasants to take risks on anticipated price changes is nothing new in West Africa. Early in this century it was well known that producers withheld supplies of oil nuts when stocks at the processing plants were known to be too low. Cf. Charles Wilson, *The History of Unilever* (1954), p. 182.

³ Peter Newman, "Mr. Lancaster on Welfare and Choice," *ECONOMIC JOURNAL*, September 1959.

⁴ "Welfare Propositions in Terms of Consistency and Expanded Choice," *ECONOMIC JOURNAL*, September 1958.

⁵ Especially E. J. Mishan and T. Majumdar.

Let X be a set of points a in commodity space, and let X' be the set of all points a' such that $a' \leq a$.¹ Then to any individual who chooses point a in X the set $\mathcal{I}$ represents:

- (i) an expanded choice if there exists in $\mathcal{I}$ a convex subset which includes both the originally chosen point $\bar{a}$ and at least one point not in X' ;
- (ii) a contracted choice if $\bar{a}$ is not in $\mathcal{I}$, but $\mathcal{I}$ is a subset of X' .

Expansion or contraction of choice is taken as the criterion of an increase or decrease in the individual's welfare, and the usual Paretian individualist criterion for an unambiguous increase in group welfare is adopted.

In order to show the optimality of free exchange in the general case, it is necessary to show, for N individuals and M commodities, that, commencing from an equilibrium reached with different individuals facing different prices, there exists at least one set of prices, the same for all individuals, for which some choices are expanded and none contracted.

Let p_{ij}^1, x_{ij}^1 be the prices faced, and quantities chosen, respectively, of the i th good by the j th individual, in a marketing situation in which the total quantities of the goods are fixed. Then we wish to show that we can always find some set of prices p_i^2 , common to all individuals, such that

- (1) $\sum_i p_i^2 x_{ij}^2 = \sum_i p_i^2 x_{ij}^1$ for all j (the budget constraints)
- (2) $\sum_i p_{ij}^1 x_{ij}^2 \geq \sum_i p_{ij}^1 x_{ij}^1$ for all j
 $\sum_i p_{ij}^1 x_{ij}^2 > \sum_i p_{ij}^1 x_{ij}^1$ for at least one j } (the criterion for increased welfare)
- (3) $\sum_j x_{ij}^2 = \sum_j x_{ij}^1$ (fixed total quantities for the goods)

for some set of quantities x_{ij}^2 not identical with the original set x_{ij}^1 .

Writing $\Delta p_{ij} = p_{ij}^2 - p_{ij}^1$, $\Delta x_{ij} = x_{ij}^2 - x_{ij}^1$, then (1) – (2), and (3) become, respectively

- (4) $\sum_i \Delta p_{ij} \Delta x_{ij} \leq 0$, all j ; $\sum_i \Delta p_{ij} \Delta x_{ij} < 0$ at least one j
- (5) $\sum_j \Delta x_{ij} = 0$, all i ; not all Δx_{ij} to be zero.

From (5), at least one of the Δx 's must be positive and at least one negative. Provided not all the Δp 's are of the one sign, we can always choose the Δx 's so that we pair a non-zero Δx with a non-zero Δp of the opposite sign and satisfy (4). That is, so long as there is at least one commodity for which there are at least two different prices, we can always choose a price between the two which gives an expanded choice to at least one individual without

¹ That is, X' includes X plus any "empty" spaces either between two points in X , or between points in X and the co-ordinate axes. For the ordinary budget-line situation X, X' are identical.

contracting the choice of any other. Conversely, if the initial equilibrium has been reached with uniform prices there is no new set of prices which will enable expansion of one individual's choice without contraction of another's.

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SOME MARSHALLIAN CONCEPTS: A COMMENT

PROFESSOR MAXWELL has suggested¹ that I should have followed the approach of Professor Ragnar Frisch to the concept of the representative firm² in writing my 1954 paper.³ It seemed to me, however, that Professor Frisch's paper was somewhat confused. Some of the objections to Professor Frisch's doctrine have already been pointed out by Professor Robertson.⁴ While Professor Maxwell believes he has avoided some of these difficulties, his re-interpretation of Frisch appears to me to leave the theory of the representative firm in a rather unsatisfactory state.

(1) Professor Maxwell assumes that the firms in the industry selected for study are not in perfect competition.⁵ But he also appears to assume, and without explanation, that there is a single supply price for the industry's output.⁶ He thus ignores all the problems connected with the supply curve of an imperfectly competitive industry, problems with which my paper was designed to deal explicitly.

(2) Professor Maxwell seems to wish to define the representative firm as that firm which is at any time operating at the minimum point of its average cost curve (p. 693)⁷ and equating marginal cost to price (p. 695). We are not told how this situation is to be made compatible with profit maximisation under imperfect competition.

(3) Professor Maxwell may, however, wish to abandon profit maximisation in favour of full cost pricing.⁸ In that case it is not made clear why there would be a single price in the industry.⁹

(4) These problems are not, I think, insurmountable. My difficulty is

¹ J. A. Maxwell, "Some Marshallian Concepts, especially the Representative Firm," this JOURNAL, Vol. LXVIII (December 1958), pp. 691–8.

² Ragnar Frisch, "Alfred Marshall's Theory of Value," *Quarterly Journal of Economics*, November 1950.

³ J. N. Wolfe, "The Representative Firm," this JOURNAL, Vol. LXIV, June 1954.

⁴ Sir Dennis Robertson, *Economic Commentaries* (London: Staples Press, 1956), pp. 11–25.

⁵ Maxwell, *op. cit.*, p. 694.

⁶ Maxwell, *op. cit.*, p. 693.

⁷ On p. 693 Maxwell says: "The rising firms will, for a time, be able to lower unit costs by enlarging their scale of plant until they reach an equilibrium point; they will then be representative, and no more internal economies will be open to them." The discussion on p. 696 appears to support and repeat this argument.

⁸ This appears to be confirmed by the description on p. 694.

⁹ Professor Maxwell nowhere states the assumption of a single price for the product of an industry explicitly. But it appears to be implied by all of his argument, and is almost stated in footnote 1 of p. 694.

that Professor Maxwell simply ignores them. A more serious complication arises, however, when we ask what Professor Maxwell wishes to do with the concept of the representative firm. The answer is by no means clear, but the quotations from Marshall cited on pp. 691-2 suggest that he wishes to argue that the costs of production of the representative firm will determine price. Unfortunately Professor Maxwell's basic assumption that there is a whole range of possible values for the minimum points of the cost curves of the various firms making up the industry¹ makes this impossible. If there are a large number of firms in the industry and a wide range of values of minimum cost points, there will presumably be a number of firms whose cost curves reach a minimum at any particular value selected. Out of this group there should be at least one firm which is producing at a minimum cost output. If all we know about the representative firm is that it is producing at an output which equates both marginal and average cost to price then there will be a different "representative firm" for each price we select. The representative firm is price-determined rather than price-determining.

(5) Professor Maxwell may not, however, have intended to use the costs of the representative firm as the determinant of price. It may have been his intention to use this device simply as a means of focusing attention on the existence of a range of costs. Such concentration would be more understandable if some use was to be made of the fact. Unfortunately, Professor Maxwell tells us nothing about the characteristics of this size distribution, or of the forces which determine it. One possibility which might bear examination is this: suppose that for any output, the costs of firms whose cost curves reach a minimum at that output, form a size distribution. We might formulate a hypothesis that the price which would prevail in the long run in this industry would bear some relation to this size distribution. It might, for example, tend to equal the mode, or the median, or some other measure of this distribution. Such an hypothesis would at least give us a starting-point, even if the causal forces behind it remained unknown.

(6) It seems to me a shortcoming of Professor Maxwell's model that it offers little explanation of what appears to be the chief behavioural characteristics with which Marshall endowed the representative firm—its tendency to grow with a growth in the demand for the products of the industry of which it composed a part, or with an increase in the price-cost ratio of those products. Professor Maxwell's observations on this question are very sketchy. He says (on p. 697) that: "In all cases the external economies which come to the representative firms in the original (fishing) industry, may, with a lag, open to them the chance of new internal economies."²

¹ See the discussion and diagram on pp. 694-5. On p. 691 Maxwell answers the question: "The representative firm is representative with respect to what?" by saying that "Confirmation of the view that unit cost is the key can be found in many statements in the Principles."

² This topic is discussed, somewhat darkly, on p. 697. Maxwell argues that an increase in demand for the products of an industry may open up new external economies if the supplying industries were not initially in equilibrium.

Professor Maxwell does not tell us why external economies should be translated into internal economies, but his reasoning may be as follows: the external economies take the form of the reduction of the price of some input to each firm in the industry. This will, in the short run, lower the costs of all the firms in the industry, but this lowering of costs must be described as an external economy. The lowering of the price of this input may, however, allow some modification of production plans, with some substitution of the input whose price has been lowered for other inputs. The reduction in costs which results from this reorganisation might conceivably be regarded as an internal economy. Depending on the technical characteristics of production, this reorganisation may increase, decrease or leave unaffected the output at which minimum cost is attained. If there were a single firm which reached minimum cost output at any price, and if price fell to exactly the same degree as the average cost of the firm that was operating at minimum cost before the change, this would specify the direction and extent of change in size of the representative firm in Professor Maxwell's model.

No doubt other interpretations of Professor Maxwell's argument are possible. I would myself prefer an entirely different approach, however, one which treats "the representative firm" as an abbreviated notation for the size distribution of firms, and which makes this size distribution the result of a process of chance. The analysis of the size distribution of firms, and with it price and output determination, would then centre on the study of the conditions which make the growth of firms more or less likely. Such a theory is hinted at in my 1954 article, and it is to be developed rigorously and in much greater detail in a forthcoming paper.

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INDUSTRIAL CAPITAL STOCKS AND ENERGY CONSUMPTION—A COMMENT

MR. FRANK's article¹ in the March 1959 number of this JOURNAL may mislead readers into thinking that he has established more than he has, in fact, done. In his article he gives indices of capital stock and energy consumption, with the same year taken as 100, both for the United States and for the United Kingdom, and shows that the correlation coefficients between the indices, for each country, are very large indeed. But it does not follow from this that energy consumption may be used as an index of the stock of capital. A further examination of the statistics adduced shows that, in one of the three cases which he quotes, serious errors would result from doing so.

If, wise after the event, one wanted to use one of Mr. Frank's indices of

¹ A. G. Frank, "Industrial Capital Stocks and Energy Consumption," *ECONOMIC JOURNAL*, March 1959, pp. 170-4.

energy to estimate capital stock, the high correlation coefficients establish that, and only that, a linear function of an index of energy can be used as an index of capital. That is, writing x_t, z_t for capital and energy in the year or period t , a function of the form $\alpha + \beta z_t$ can be discovered which estimates x_t with considerable precision. It would not be correct to use z_t without further ado for this purpose; this assumes that $\alpha = 0$ and that $\beta = 1$. There are three pertinent questions: can α reasonably be taken to be 0, can β reasonably be taken to be 1 and would it be reasonable to take α as 0 and β as 1? Supposing that the discrepancies from the linear function of z_t are independent and normally distributed, these questions can be answered from Mr. Frank's figures.¹

For the United States, a function of the required form, fitted by least squares, is

$$x_t = 1.1298 + 0.9873 z_t, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and it appears *prima facie* that α and β might well have the values 0 and 1, the discrepancies being due to unrepresentative data. This is in fact so. With 95% confidence it may be asserted that, irrespective of β , α lies in the range $-7.00-9.26$. Irrespective of α , with the same confidence it may be asserted that β lies in the range $0.95-1.02$. Testing whether the real relation could be $x_t = z_t$ is a little more complex; it amounts to testing whether the point (0, 1) lies within a certain ellipse in the α, β plane. In fact, it does so in this case, so the observed statistics might well be represented by the function $x_t = z_t$. This confirms the impression produced by Mr. Frank's Fig. 1.

It is reasonable to ask what loss of precision would be involved by taking z_t as an estimate of x_t rather than using the better-fitting least-squares line (1). The answer is, very little. The greatest discrepancy between the two series of index numbers is 16, whereas the greatest discrepancy of the value estimated from (1) from the actual value is 14.46. The root mean square deviations are, respectively, 6.07 and 5.69. The improvement resulting from the use of (1) is slight, particularly compared with the inaccuracies of the data.

Using the Giffen and Stamp series of all capital, the regression for the United Kingdom is

$$x_t = 0.4855 + 1.0209 z_t, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Again it turns out to be the case that the discrepancies from $x_t = z_t$ may reasonably be due to sampling errors alone. The confidence intervals for α and β , disregarding the other, are $-38.82-39.79$ and $0.80-1.24$ respectively (the comparative wideness of the intervals reflects both a smaller correlation coefficient and fewer observations). Once more, the point (0, 1)

¹ See, for example, A. M. Mood, *Introduction to the Theory of Statistics*, Ch. 13 (New York, 1950.)

is found to lie well within the appropriate ellipse in the α, β plane, so the observed statistics might well be represented by the function $x_t = z_t$.

In this case, the greatest discrepancy between the two series of index numbers is 14, whereas the greatest discrepancy of the value estimated from (2) from the actual value is 17.39. The root mean square deviations are 9.37 and 8.36 respectively. Again, (2) hardly represents an appreciable improvement.

The Douglas series of industrial capital for the United Kingdom gave Mr. Frank's lowest correlation, 0.9619. In this case, the results of the closer analysis are rather different. The regression is

$$x_t = -51.7178 + 1.4841 z_t, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The confidence intervals for α and β , disregarding the other, are $-118.75-15.32$ and $1.11-1.86$ respectively. The former barely includes the value 0, indicating that this value is not implausible. But $\beta = 1$ is not a good approximation at all. Further, the ellipse in the α, β plane does not, this time, come anywhere near to including (0, 1) (in fact, $F_{(2, 5)} = 50.48$), so it is highly unlikely that $x_t = z_t$ could have led to the observed data.

The greatest discrepancy between the two series of index numbers is 64, whereas the greatest discrepancy from (3) is 27.81. The root mean square deviations are 41.81 and 14.26 respectively. It follows that (3) is a great improvement over $x_t = z_t$, as the statistical analysis indicates.

It is not possible to determine whether the radically different results of the two United Kingdom analyses are due to faults in the basic data or to a breakdown in the model. Mr. Frank argues that energy consumption should be a better indicator of industrial capital than of total capital, whereas this is the opposite conclusion to that reached from the figures. But an examination of (3) does indicate a possible explanation of the observed result. The regression indicates that a rise in energy consumption of one unit is associated with a rise in industrial capital of about one and a half units, where in each case the "unit" is 1% of the 1865 figure. This is in accordance with the argument that newer capital is a more efficient user of energy than older, at any rate in the industrial sector. The series for total capital, and for industrial capital in the United States, do not, however, reflect this.

The formal properties of the regression are as follows. In all cases the absolute rates of increase of the two series are in proportion and, in general, estimates of capital relating to two periods are needed to enable the series of energy to be translated into one of capital. If $\alpha = 0$, so that the regression is $x_t = \beta z_t$, the absolute rates are in proportion, as before, and in addition the percentage rates of increase are equal. One estimate of capital stock (which might well be 100 for a suitable base year) suffices to estimate the relationship. If $\beta = 1$ but α is not equal to zero, so that the regression is $x_t = \alpha + z_t$, the absolute rates of increase of the two series are equal, but the

percentage rates are not. Again, one estimate of capital stock suffices. If, as was the case in two of the three instances considered, the regression is effectively $x_t = z_t$, the energy series can be used immediately for the capital series. The high correlations indicate that a linear regression is a good model; before supposing that $x_t = z_t$ further investigations would be necessary in particular cases.

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A NOTE ON PROFIT SHARING IN MIDDLE EAST OIL

PROFESSOR PENROSE's closely reasoned article in the *ECONOMIC JOURNAL*,¹ though it does not support a commonly held view that the Middle East oil countries are exploited "because of the monopolistic organisation of the industry," takes the position that the oil companies possess "superior economic power . . . arising from their ability to inflict a disastrous economic loss on the producing country" and that this power "gives them a bargaining position which *holds down* the share of the profits the producing country can obtain in an oil agreement. Thus the producing countries can still complain of exploitation in the sense that their resources are being used to earn large profits which do not accrue to them."²

The post-war record in the Middle East clearly supports Mrs. Penrose in some cases, notably that of the serious Anglo-Iranian Oil Company-Iranian Government dispute of 1948-53. But other very significant parts of the record are not explicable in terms of her analysis. The purpose of this note is to call attention to such parts of the record, and particularly to the important cases in which the evidence strongly indicates that the Government and not the Company enjoys superior economic power, especially the power in given situations to increase substantially its share of company earnings.

The first case occurred during the period of the Iranian dispute, and, given interdependence in oil matters and the sellers' market at the time, was doubtless influenced, at least in part, by the course of that dispute. The case in question concerns the action of the Saudi Government in 1950. Though the Company (Aramco) fits Mrs. Penrose's model—in that it (1) was at that time the only company in the country, and (2) is owned by some of the biggest international oil companies—the Government was able to take *unilateral* action which more than *doubled* its share of net earnings from Saudi production. The Company was not happy about the Government's decision, but it acquiesced in it and recognised that the Saudi Government

¹ E. T. Penrose, "Profit Sharing Between Producing Countries and Oil Companies in the Middle East," Vol. LXIX, June 1959, pp. 238-54.

² P. 251 (italics added).

could not legitimately be denied formula parity with Venezuela, which had pioneered the 50-50 sharing arrangement over the preceeding seven years.¹

More recent, and in some ways even stronger, evidence also indicates the superiority of the Government's position in important situations if not generally to-day. Beginning in 1958, for example, the discounting of oil in world markets reached such proportions that Middle East posted prices—which are used by the Governments of that area to compute the sharing base—led to a marked overstatement of the "net earnings" of the Companies. The result was that, despite the buyers' market, the Government share in important cases was further increased by some 20%, without as much as a sign of company reaction of the general type suggested by Mrs. Penrose's analysis.² Moreover, it is relevant to point out that at about the same time the Venezuelan Government unilaterally increased its share by decreeing a new tax formula which reduces the company share approximately a fifth.

The dynamics of the international oil industry, and especially the large Middle East component thereof, appear to be generating forces producing an increase in the secular trend of the Government's share. Though this is not the place to go into details, considerations along the lines of Ricardian rent analysis and the development of Arab nationalism within the general context of Soviet-United States political relations, rather than oligopoly analysis, would appear to loom large as explanatory factors.

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THE MARGINAL EFFICIENCY OF CAPITAL

MESSRS. PITCHFORD AND HAGGER³ show that, for the Marginal Efficiency of an asset to be defined uniquely by the expression,

$$S = \sum_{t=1}^{t=m} \frac{Q_t r}{(1+r)^t} \cdot \cdot \cdot \cdot \cdot \quad (A1)$$

(where S is the current replacement cost of the asset, $Q_n \dots Q_1$ are the

¹ See Aramco's testimony before Congress, *Joint Hearings before Subcommittees of the Committee on the Judiciary and Committee on Interior and Insular Affairs*, Senate, 1957, Part 2, pp. 1428-30.

² The adverse impact on Middle East oil companies was so heavy that one study even reports it in terms of a negative "indicated nonproducing income" in 1958 of \$260.1 million in a year when adjusted company payments to producing governments amounted to \$1,237.0 million, 18% greater than in the preceding year. See W. S. Evans, *Petroleum in the Eastern Hemisphere*, First National City Bank of New York, April 1959, pp. 12-13. Though this study includes areas outside the Middle East, the data in it may be used as Middle East figures.

³ J. D. Pitchford and A. J. Hagger, "A Note on the Marginal Efficiency of Capital," *ECONOMIC JOURNAL*, September 1958, pp. 597-600. C. S. Soper, "The Marginal Efficiency of Capital: A Further Note," *ECONOMIC JOURNAL*, March 1959, pp. 174-7.

expected net returns during the periods of its life and r its marginal efficiency), it is necessary for

$$\sum_{t=1}^m t \cdot Q_t \cdot p^{t-1} > 0 \quad . \quad . \quad . \quad . \quad . \quad (A2)$$

(where $p = \frac{1}{1+r}$). If (A2) is not satisfied for all types of possible project it will not be possible to determine exactly the level of aggregate investment at a given rate of interest. It is concluded that the economic importance of this depends on how prevalent negative Q 's are: "it is of considerable importance if negative Q 's are at all common and can be largely ignored if, as may be the case, they are something of a rarity."

It may be noticed immediately that the existence of negative Q 's will not necessarily cause (A2) to fail. It is the contention of this note that while Q 's may often be negative, it is much rarer for them to be sufficiently negative for the marginal efficiency of the asset to be multi-valued.

The expectation of negative Q 's by the investor is not rare; indeed, if periods of a sufficiently short length are taken, it will be unusual for some Q or other not to be negative. There are many cases where "follow-through" investment is necessary in the early years of a plant; and the initial investment itself is likely to be spread over a period of time. In other cases the necessity of very substantial repairs part way through the life of the plant can be foreseen with certainty. And ventures are frequently undertaken where there is a high probability that some year or other will produce substantial running losses—for example, in retaining a labour force through a recession or in carrying a mine through a wash-out.

Nevertheless, although an entrepreneur may face the prospect of some running losses when he is considering investing in a particular venture, he will, *if he is free to terminate the venture at any time during the physical life of the equipment*, contemplate carrying on through the period of losses only if he expects the losses to be succeeded by more than compensating profits. If, looking into the period of the physical life of the assets associated with a particular venture, a point is viewed beyond which losses over-balance profits, then this point is either the limit or beyond the limit of the economic life of the plant.

This proposition may be put in a much simpler way: from the streams of returns corresponding to the possible lengths of life the entrepreneur will select that with the greatest discounted value. Then if i is the rate of discount, $x = \frac{1}{1+i}$, the corresponding "discounting factor," and m the economic life of the plant, we have

$$\sum_{t=1}^m Q_t x^t \geq S \quad . \quad . \quad . \quad . \quad . \quad (B1)$$

which is the condition for the project to be economic; and the $m-1$ conditions,

$$\sum_{t=1}^m Q_t x^t \geq \sum_{t=1}^{m-1} Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad (B2)$$

$$\sum_{t=1}^m Q_t x^t \geq \sum_{t=1}^{m-2} Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad (B3)$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B4 \text{ to } B_{m-1})$$

$$\sum_{t=1}^m Q_t x^t \geq \sum_{t=1}^1 Q_t x^t \quad . \quad . \quad . \quad . \quad . \quad (Bm)$$

which are necessary conditions for m to be the economic life of the project.¹ (B2) to (Bm) can be rewritten in the forms,

$$\sum_{t=m}^m Q_t x^t \geq 0 \quad . \quad . \quad . \quad . \quad . \quad (C2)$$

$$\sum_{t=m-1}^m Q_t x^t \geq 0 \quad . \quad . \quad . \quad . \quad . \quad (C3)$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C4 \text{ to } C_{m-1})$$

$$\sum_{t=z}^m Q_t x^t \geq 0 \quad . \quad . \quad . \quad . \quad . \quad (Cm)$$

In this form they restate the argument of the last paragraph. Adding (B1), (C2) to (Cm) we obtain,

$$\sum_{t=1}^m t \cdot Q_t \cdot x^t \geq S;$$

or, dividing both sides by x ,

$$\sum_{t=1}^m t \cdot Q_t x^{t-1} \geq \frac{S}{x} \quad . \quad . \quad . \quad . \quad . \quad (D)$$

If (D) is true, (A2) is true and i is single-valued. And this will be true in the limiting case where $\sum_{t=1}^m Q_t x^t = S$ and $i = r$ or the marginal efficiency of the project.

Moreover, in proving (D) for cases of *costless extricability* we have proved (A2) with a margin to spare. It seems to follow that in some cases, even where the assumption of costless extricability fails, (A2) may still be satisfied. These are the cases where the unavoidable loss later in the life of the plant is small in relation to the initial capital outlay.

The Pitchford and Hagger case seems unlikely to occur, because it is surely true that costs of extrication are relatively small except where they are voluntarily incurred because of offsetting salvage values (*e.g.*, taking

¹ That the argument here is mathematically identical with that of Mr. Soper is evident from the fact that these are the conditions which he derives for the roots of the marginal-efficiency equation to be positive.

out the pillars of a mine). But the case cannot be proved impossible, and it might even come into its own if there was an increase in the extent of legal and social obligations placed on entrepreneurs to compensate their redundant labour forces and to restore the sites they abandon. If, for example, the Electricity Authority were compelled to restore perfectly the sites of obsolete nuclear power-stations it is possible that, because the "final" outlay would be of a size comparable with the initial outlay, it would react perversely to changes in the rate of interest.¹

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COMPLEMENTARITY AND FUTURE TIME: A NOTE ON THE MARGINAL EFFICIENCY OF CAPITAL

IN a Note in the September 1958 issue of this JOURNAL Pitchford and Hagger² explained that the marginal efficiency of capital could not always be given an unambiguous value, for a given replacement cost of capital and a given series of yields, if some of the yields were negative. In March 1959 Soper³ gave an elegant analysis of the conditions in which negative yields could in fact give multiple values; and went on to explain in economic terms why these conditions would not prevail under profit-maximising conditions.

Unfortunately the latter part of this proof is fallacious. It is a variant of a faulty theorem used in Hicks' *Value and Capital* to establish a different proposition.⁴ Nevertheless, the derivation of the very limited conditions in which multiple values can occur certainly adds to our knowledge. When the technical conditions are set out in terms of actual economic possibilities they can be judged to be infrequent. The fallacy may, however, have some limited methodological importance.

Soper demonstrates that there cannot be more than one real and positive root to the equation $\sum_{i=1}^n Q_i p^i - S = 0$ (where Q_i is the output at time i

¹ What would happen in a case like this is that the revenue of the first few years of the station's economic life would pay for its initial cost, while the income of subsequent years would accumulate to provide for the "final" outlay. Thus for part of its life the authority would be a borrower and for part a lender, and if the latter was more important it would regard an increase in rates of interest favourably!

² J. D. Pitchford and A. J. Hagger, "A Note on the Marginal Efficiency of Capital," *ECONOMIC JOURNAL*, September 1958.

³ C. S. Soper, "The Marginal Efficiency of Capital: A Further Note," *ECONOMIC JOURNAL*, March 1959.

⁴ J. R. Hicks, *Value and Capital*, 2nd Edition, pp. 201, 221-2; T. H. Silcock, "A Note on a Theorem in *Value and Capital*," *Review of Economic Studies*, XVIII (2), 1950-51.

and $p = \frac{1}{1+r}$) when the value of S , the replacement cost, is continuously greater than $\sum_{i=1}^k Q_i p_j^i$ for all $k = 1, 2, \dots, n-1$ the term p_j being one real and positive root of the original equation. He then illustrates, by Fig. 1,

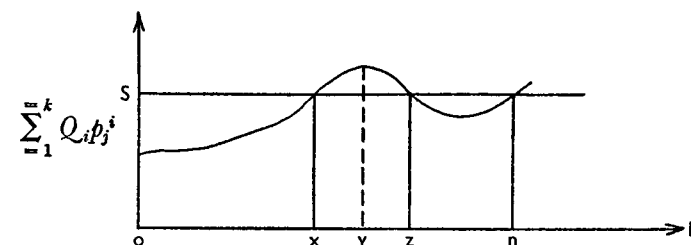


FIG. 1

the conditions in which there can be more than one real positive root to the equation. Multiple roots can only occur when the curve of the value $\sum_{i=1}^k Q_i p_j^i$ against time intersects more than once the horizontal line representing the replacement cost S .

From this he goes on to show that if the investment plan were discontinued at the point y the aggregate value, if discounted at the given rate p_j would be greater than the replacement cost. A smaller value of P , which would make the aggregate value equal to the replacement cost, implies a higher value of r , or in other words a higher yield. Thus, by discontinuing the investment plan at y the entrepreneur would secure a higher yield than in the original completed plan.

Since multiple values are only possible when the curve intersects the S line more than once, there must always be at least one point such as y at which, by stopping the investment plan, a higher yield could be secured than by carrying it through to completion.

In effect, this is a demonstration that there can only be more than one positive value for the marginal efficiency of capital in an investment plan, if there are not merely individual negative yields but whole sectors at the end of the plan, the addition of which reduces the aggregate discounted value below what it would be if the plan were discontinued earlier.¹

The proof goes on to argue that, in the process of maximising profits, these sectors of the plan would be abandoned as unprofitable. A true maximum profit plan would therefore always have a uniquely definable marginal efficiency of capital.

¹ The passage in the text does not specify the rate at which the discounting must be done. It is in dealing with this nice logical point that Soper's proof is most elegant; but the matter does not lend itself to verbal exposition, and is not important for the present argument.

It is not, however, always possible to discontinue a plan at any point in time. Debts must be repaid, tin-tailings rendered harmless, building structures demolished and radio-active waste buried deep, at considerable cost. In making a plan in advance one may often have to face later, negative yields, as a condition of securing the earlier, positive ones.

In interpersonal relations an earlier consumption experience followed by a later outlay is fairly common. Most of us, who can get personal credit, have this experience every month! Is there any good reason for supposing that the pattern is uncommon in which the positive yield precedes some part of the negative yield which is its cost?

There is, of course, a difference in this respect between our dealings with nature and our dealings with other men. In our dealings with nature our plans consist in modifying, on the basis of more or less accurate knowledge, a part of the infinite series of events in an environment basically neutral or mildly hostile to our own ends. It is usually possible to ensure, at no great cost, that the series of events continuing after the planned satisfaction of our wants will be at least no more hostile to our own ends than if we had not intervened. If we know enough to make some events in the series markedly positive we shall normally know enough to keep the others neutral, ensuring that worn-out tools are not dangerous, and formerly cultivated land at least no more hostile or infertile than jungle.

This is the normal pattern, but unfortunately it is not invariable. Every plan we initiate now has an infinite series of positive and negative outputs as its consequences; and occasionally it is not possible, or not worth while, to take the positive action before the desired consequences, which will forestall the need to take remedial action afterwards, or suffer adverse effects.

Because of the normal pattern, however, our institutions are designed on the basis of physical property with a continuing positive value. Our economic morality is based on the assumption that it is wrong or irrational to enjoy something in advance of paying for it. Probably a number of technically feasible and economically rational possibilities of consuming in advance are ruled out on these grounds.¹

More important is the fact that our normal conduct of economic life, and hence of economic analysis, is based on the assumption that bygones are bygones. Both obligations and assets surviving from the past are treated as institutional facts in the midst of which an essentially new decision has to be made. Investment, like all other economic decisions, begins anew at each instant.

From this point of view, if the entrepreneur has actually reached y , and if some of the negative yields beyond y are consequences imposed on him by his efforts and satisfactions up to y , then these negative yields will not, at y , be treated as part of his investment plan at all. They will be an institutional

¹ Cf. Joan Robinson, *The Accumulation of Capital*, Chapter 4.

hangover, unrelated to his future plans; and in planning for the future they will be ignored and treated as zero, once y is reached.

This is the reason why the fallacy in Soper's proof has some methodological importance. Indeed, this is the main significance of the present note. (Actual cases in which the latter end of an investment plan has negative value are unlikely to have much economic importance.) The significant point is that we cannot, when we are discussing economic time in mathematical symbols, treat it as if it were continuous without specifying our assumptions about the planning process much more carefully than we normally do.

Viewed from y , any negative yields which are merely a consequence of earlier investment are not part of the new investment plan at all. They have become simply a part of the environment; and to neglect this aspect of them would be to sacrifice the characteristic economic insights based on the essential newness of choice at each instant of time. But viewed from time 1, or the initial point of the whole investment plan, some of the negative yields may be technically complementary with some of the earlier positive ones.

Because it is easy and natural to treat time as a mathematical continuum, and because, in a continuing economic plan, the relations between anticipated time and realised time are complex, and are often the essence of the economic problem, it would appear to be desirable to translate mathematical reasoning as Soper did at this point, into verbal economic expressions, so as to facilitate intuitive checks on its plausibility.¹ If, in the proof under reference, the approach to maximisation by discarding any negative remainder of an investment plan after time y had been expressed in terms of a generalised mathematical statement, *e.g.*, that for at least some positive value P_j of P , the expression $\sum_{i=k}^{i=n} Q_i p_j^i$ must be positive for any value of k , and if this plausible condition, which would be valid for dealing with new investment but invalid for dealing with phases in an investment viewed from time 1, were then applied as a constraint in a general system of mathematical reasoning about the problem, it would be very difficult to detect that a flexibility rightly postulated at the initial point of an investment process had been wrongly assumed as between two future points of a present plan.

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¹ Cf. T. H. Silcock, "Mathematics in the Teaching of Economics," *Malayan Economic Review*, I, 1, 1956.

DIFFERENTIAL POPULATION GROWTH AS A FACTOR IN INTERNATIONAL ECONOMIC DEVELOPMENT¹

The following note arises from Mr. P. T. Bauer's article on "International Economic Development."² In this article Mr. Bauer discusses Gunnar Myrdal's views on the divergence between, on the one hand, the increase in *per capita* national income shown, or assumed, in the great majority of countries *considered individually* and, on the other hand, the median income of the average world citizen. In this connection he refers to an article which I had published in 1949,³ in which attention was drawn to this divergence. The reason for this divergence is, of course, the differential rate of population growth in the poorer and richer countries respectively. Since this matter is of some general interest, and there seem no real disagreements as to the points of substance—as distinct from the best way of expressing them and perhaps their quantitative importance—it may seem worth while setting out the points involved more clearly than has perhaps been done, and at the same time showing their quantitative importance in the light of recent figures.

The following table is based on U.N. data for *per capita* national income and growth of population respectively.⁴ It shows countries grouped into four income classes, and the expected rate of growth of population 1955–65 for each class separately.

Income per annum <i>per capita</i> , \$.	Number of countries.	Population 1955, millions.	Population 1965, millions.	Percentage population increase * 1955–65, %.
Under 100	51	1,437	1,713	19
100–599	73	835	1,001	20
600–999	9	199	211	6
1,000 and over	8	197	227	15
Total	141	2,668	3,152	18

* Data take account of international migration.

This table shows clearly that there is a generally negative correlation, although by no means a perfect one, between *per capita* income and rate of population increase. The rank correlation coefficient for 100 countries for

¹ The author, who at present is a staff member of the United Nations, is personally responsible for the opinions expressed, which do not represent the official views of the United Nations.

² ECONOMIC JOURNAL, March 1959.

³ "Economic Progress in Underdeveloped Countries," *Social Research*, March 1949.

⁴ I am indebted to my colleagues Mr. Grauman and Mr. Khan for the data included in this note.

which data were available individually is -0.33 . (The Pearson coefficient of correlation is also -0.32 , significant at the 1% level.)

Each of the two poorest categories of countries has a higher rate of population increase than either of the two wealthier categories. The quantitative importance of differential rates of population growth can also be readily calculated from this table. Other things being equal, *i.e.*, *per capita* income within each of the four categories of countries remaining constant,¹ differential population growth would reduce the world average income from about \$341–342 in 1955 to about \$333 in 1965. This is a decrease of just under 0.3% per annum.

The same result of a 3% decline in world *per capita* incomes as a result of differential population growth, other things being constant, can also be obtained by using the data to produce a simple "two sector model" in which the countries with a *per capita* income of less than \$600 per annum/*per capita* form one group, and the countries with over \$600 the other. In this simple model countries under \$600 have six times the population of the others (1955 data). Applying to these two groups the expected rates of population growth 1955–65, we note that the average income of the two groups combined also falls by 3% within the decade.

This rough calculation is an under-statement of the downward pull exerted by the differential population growth in the poorer countries, since we have not made allowance, so far, for the differential growth of population *within* each income category. More detailed calculations relating to 100 countries suggests that the downward pull of differential population growth is 4.3% for the 1955–65 decade, or 0.4% per annum.

Now perhaps we may put the matter under debate as follows:

(1) To begin with, taking industrial output, agricultural output and other output separately there seems little observable tendency for output in the under-developed countries generally to grow less than output in the more developed countries generally. To repeat, that is the situation *as long as we consider the various sectors separately*.

(2) However, throughout the world there is a tendency for agricultural output to grow more slowly than industrial output. Since agricultural output is more heavily weighted in the national incomes of the poorer countries, this results in a general picture of national averages growing more slowly in the poorer countries, although there are numerous exceptions to this general tendency. Thus, the different weighting of the different sectors of output and its correlation with *per capita* income levels exerts a downward pull on the *relative* rate of progress of the under-developed countries, compared with others. *

¹ For purposes of this rough calculation, \$75 has been assumed to be the average *per capita* income in the lowest income class, of under \$100 per annum *per capita*.

(3) Furthermore, and additionally to the downward pull mentioned under (2) above, there is differential population growth and its generally negative correlation with *per capita* incomes. This further downward pull reduces the world average income *per capita* at a present rate of 0.4% per annum. This means that if average *per capita* incomes in the individual countries increase by, say, 1¼% per annum—1% for the poorer countries and 2% for the other countries—the differential rate of population growth would offset, for the world as a whole considered as a statistical unit, about one-third of the progress suggested by the analysis of individual countries (0.4% out of 1¼%). Thus the offsetting factor is certainly significant, and it may well be that at times it has offset most or perhaps the whole of the progress observed in individual countries.

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GALBRAITH ON COUNTERVAILING POWER: A REPLY

THE difficulty in replying to Professor Galbraith's comment (March 1958) on my critique of countervailing power is to know his present position. Is his book on countervailing power intended as enlightened comment on the intellectual parochialism of professional economists, does it provide an interesting qualification of existing theory or is it an important theoretical innovation? I am puzzled to know at which level he is commenting.

On the first interpretation I do not feel competent to judge except to agree that the profession, all too frequently, seems to lose sight of the ends of our society in its preoccupation with the intricacies of analysis; or, worse, develops a mania for methodological problems. A sharp reminder of current realities from someone of Professor Galbraith's talents is always salutary. I myself confess to enjoying, and profiting from, his excursions into social and political-economy matters.

Secondly, if he means that countervailing power is a force to be reckoned with in market organisation and social and political life no one can possibly disagree. It may even be an important force. I myself adverted to its use, *à la* Galbraith, in legislation to control monopoly. It is now so incorporated into the 1956 Restrictive Trade Practices Act (Section 21 (d)). But it is not a new idea. Marshall, ever with us on these retrospective exercises, noted the possibility, indeed the probability, of a development of countervailing power in his time (*Principles*, 8th ed., p. 493). Nor is it easy to assess the place of such a notion in economics. Can it rank with what may be called the important secondary concepts of economic analysis—for

example, uncertainty, non-competing groups, increasing returns, the compensation principle, net advantages and other ideas associated with various names in economic thought? I would hesitate to attempt an answer.

However, in my innocence I took the third view that Galbraith was advocating, albeit on an intuitive level, a theoretical approach for market economies analytically superior to the orthodox competitive approach (see Chapters IV and IX of his *American Capitalism*). It was an attractive idea and seemed important. I therefore endeavoured systematically to test it by examining, not merely the structure of distribution as he implies, but also the interrelationships of manufacturing industry, the trading activities of government agencies, the habits of the co-operative movement and the development of the British trade-union movement (pp. 90-1, 93-5 and 95-8 this JOURNAL, March 1958). I found that competition was still the predominant force and that countervailing power made no systematic appearance in any sphere except, oddly, the buying activities of nationalised industries and government departments. The analytic deficiencies of competitive theory, of which Professor Galbraith makes so much, are irrelevant to this crucial point. Further, analysis revealed inconsistency in his selection of material and error in the principal welfare propositions of his theory (pp. 99-102). I see no reason now in the light of further thought, or his comment, to withdraw any of these conclusions. If Professor Galbraith proposes new theoretical constructions he must expect to have their operational value scrutinised; and for them to be shot at if found inadequate. This may be negative, but it is also an essential aspect in the advancement of economic thought—as Professor Galbraith must know.

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NOTE ON THE RELATION BETWEEN SHARE OF WAGES IN NATIONAL INCOME AND TERMS OF TRADE

"THERE seems to be some empirical support, therefore, for the view that movements in the terms of trade and in the share of wages are related (inversely), although it must be remembered that the number of countries we have considered is limited and the period with which we have been concerned short."

Thus Mr. Maynard¹ summarises his conclusions based on an analysis of eight countries over a limited number of post-war years.

¹ G. Maynard, "Inflation, the Terms of Trade and the Share of Wages in National Income," ECONOMIC JOURNAL, December 1958.

It is true that since 1939 there does appear to have been this inverse correlation; a regression line drawn on Chart 1 would proceed from north

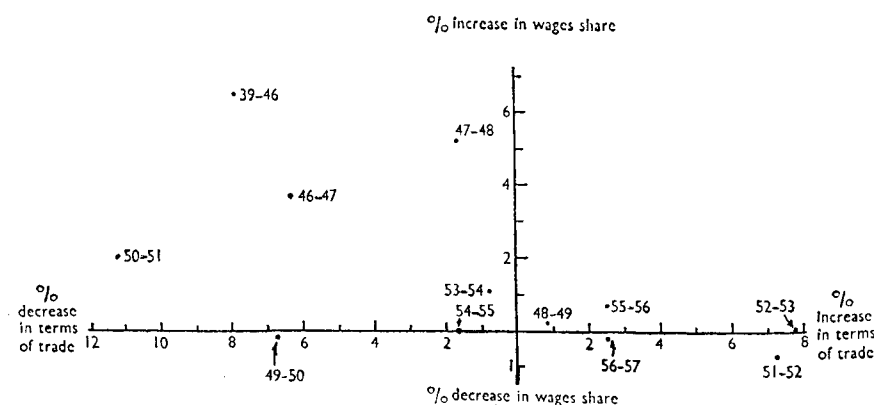


CHART 1.—Relation between proportionate change in wages share of national income and proportionate change in terms of trade (increase represents improvement), United Kingdom 1939-57.

west to south-east. There are, however, still three years when this negative correlation did not hold.

The scatter diagram in Chart 2 plots the relationship for seventy-one

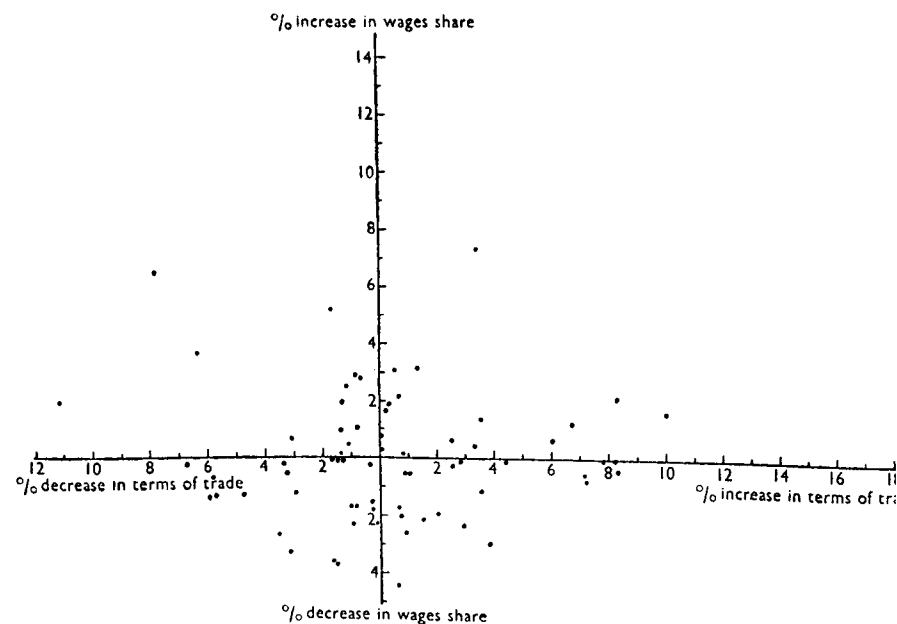


CHART 2.—Relation between proportionate change in wages share of national income and proportionate change in terms of trade (increase represents improvement), United Kingdom 1870-1957.

years or periods from 1870 to 1957 and it is obvious that there is no consistent long-run association between the two variables.

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*Changes in the Share of Wages in the National Income and in
Terms of Trade—United Kingdom 1870-1957*

	% change in share of wages in national income.	% change in terms of trade.		% change in share of wages in national income.	% change in terms of trade.
1870-71	+1.3	+6.1	1906-07	+1.9	+0.3
1871-72	+7.4	+3.4	1907-08	+0.3	0.0
1872-73	+1.4	+3.5	1908-09	-1.3	-4.8
1873-74	-3.3	-3.2	1909-10	0.0	-1.4
1874-75	+2.9	-0.9	1910-11	0.0	+4.4
1875-76	-1.4	-6.0	1911-12	-2.1	-0.1
1876-77	-0.7	-5.9	1912-13	-1.1	+3.5
1877-78	-2.9	+3.8	1913-24	+14.5	+18.2
1878-79	+2.5	-1.2	1924-25	-0.2	-3.4
1879-80	-3.6	-1.7	1925-26	+0.5	+3.3
1880-81	-0.5	-3.3	1926-27	-1.7	-0.9
1881-82	+2.8	-0.7	1927-28	-3.6	-1.7
1882-83	-1.7	+0.6	1928-29	+0.8	0.0
1883-84	0.0	+2.8	1929-30	+2.2	+8.3
1884-85	-0.5	+0.9	1930-31	+1.7	+10.0
1885-86	-2.0	+0.7	1931-32	-0.5	+1.0
1886-87	+3.1	+0.5	1932-33	-1.9	+2.0
1887-88	0.0	-1.5	1933-34	+0.7	-3.1
1888-89	+3.2	+1.3	1934-35	-1.7	-1.0
1889-90	0.0	+8.2	1935-36	-1.2	-3.0
1890-91	+0.5	-1.1	1936-37	-1.3	-5.8
1891-92	+0.2	-1.4	1937-38	-0.3	+8.3
1892-93	+2.2	+0.7	1938-39	-2.3	-1.0
1893-94	-2.3	+2.9	1939-46	+6.5	-7.9
1894-95	-2.6	+0.9	1946-47	+3.7	-6.4
1895-96	+1.7	+0.2	1947-48	+5.2	-1.7
1896-97	-1.5	-0.3	1948-49	+0.2	+0.8
1897-98	-0.2	-0.4	1949-50	-0.2	-6.8
1898-99	-0.5	+7.1	1950-51	+2.0	-11.2
1899-1900	+0.7	+6.0	1951-52	-0.7	+7.2
1900-01	+1.0	-1.4	1952-53	0.0	+7.8
1901-02	-2.7	-3.6	1953-54	+1.1	-0.8
1902-03	+2.0	-1.4	1954-55	0.0	-1.7
1903-04	-4.4	+0.6	1955-56	+0.7	+2.5
1904-05	-1.8	-0.3	1956-57	-0.2	+2.5
1905-06	-2.1	+1.5			

Terms of Trade: The relationship between Import prices and Export prices; (a positive sign indicates an improvement in the terms of trade).

1870-1913 Imlah, "Terms of Trade of the United Kingdom 1798-1913," *Journal of Economic History*, November 1950.

1913-1957 *London and Cambridge Economic Bulletin*.

Sources: *Wage Share:*

1870-1945 E. H. Phelps Brown and P. E. Hart, "The Share of Wages in The National Income," *ECONOMIC JOURNAL*, June 1952.

1946-1957 Calculations from Table 2 of 1955 and 1958 Blue Books on National Income. Same definitions of National Income as used by Phelps Brown and Hart, i.e., total personal income before tax minus pay of armed forces and employers' contributions. For comparison, the Phelps Brown and Hart figures appear below together with the figures calculated from the more recent sources:

Wages as % of National Income

	Phelps Brown and Hart.	New estimates.		Phelps Brown and Hart.	New estimates.
1938	39.2	39.3	1952	—	45.1
1946	40.5	40.8	1953	—	45.1
1947	40.8	42.3	1954	—	45.6
1948	41.7	44.5	1955	—	45.7
1949	42.3	44.6	1956	—	46.0
1950	41.9	44.5	1957	—	45.9
1951	—	45.4			

THE THEORY OF SELLING COSTS: A CORRECTION

IN the paper of this title in the June issue of the ECONOMIC JOURNAL I argued that the second order conditions of maximum profits require the return to selling outlay (at constant prices), to be diminishing. This argument is wrong. In correcting the proof I noticed that something was amiss and inserted a footnote (p. 296), in which diminishing returns to selling outlay became an assumption rather than a proposition. In the appendix, however, I allowed the mistake to stand.

If x is demand, $x_s = \frac{\delta x}{\delta S}$, $x_{ss} = \frac{\delta^2 x}{\delta S^2}$ (here S is selling outlay), then if the elasticity of demand is constant throughout, a second order condition for a maximum profit is $\left[\frac{x_{ss}}{x} - \left(1 + \frac{1}{e} \right) \right] < 0$, where e is the elasticity of demand. Hence $x_{ss} < 0$ is necessary only if $|e| \leq 1$.

The argument of the article is quite unaffected provided we treat $x_{ss} < 0$ as an *assumption*. If $x_{ss} > 0$ the equation (6a) of the appendix can be used to find the new shape of the PS curve which *may* now be positive, even if e is constant throughout. Since the case of a positively shaped PS curve is considered in the article (although I then believed it could only occur if e were not constant), the analysis can be corrected in an obvious way.

Birmingham University.

F. H. HAHN

CURRENT TOPICS

THE International Economic Association held a Conference at Elsinore, Denmark, during September 1959 on the subject of "Inflation." Among those who contributed papers were G. Haberler, H. S. Ellis, E. H. Chamberlin, J. S. Duesenberry, W. R. Gardner, A. W. Marget, R. Triffin (U.S.A.), J. C. R. Dow (U.K.), E. Lindahl, E. Lundberg (Sweden), E. James, J. Rueff (France), F. Neumark (Germany), P. Baffi (Italy), M. W. Holtrop (Netherlands), E. Gudin (Brazil). The papers and a summary of the discussion will be published for the International Economic Association by Messrs. Macmillan. The Conference was preceded by the triennial meeting of the Council of the Association. Professor Austin Robinson was elected to succeed Professor Lindahl as President for 1959-62; Professor Gudin was elected Vice-President; Professor Neumark as Treasurer.

Houblon-Norman Awards. The trustees of the Houblon-Norman Fund invite applications for Fellowships of Grants in aid of Research into the working of industry and finance in Great Britain and elsewhere and the economic conditions affecting them. Fellowships, which are intended for experienced research workers, are awarded for full-time research for one year, which may be renewed for a second year. Grants are awarded for either full-time or part-time work. The amount of the awards, which will take effect from October 1, 1960, will depend upon the circumstances of the candidate and the probable expenses of his work. Applicants must be British subjects normally resident in the United Kingdom. The Trustees are prepared to relax this rule only in exceptional circumstances. Forms of application, which may be obtained from the Secretary of the Fund, c/o the Bank of England, London, E.C.2, should be returned not later than March 1.

THE following have been admitted to Life Fellowship of the Society:

Agbi, B. O.	Kinoshita, Prof. K.	Muscatt, J.
Carney, D. E.	Machado, Dr. A. G.	Panas, E. G.
Katayama, S.		

THE following have been admitted to Fellowship of the Society:

Adediran, G. E. T.	Atcherley, P. N.	Beesley, Dr. M.
Ahmad, J.	Atkins, A. S.	Bennett, R. C.
Antony, T. J.	Aweh, F. O.	Britt Mother, E.
Arowolo, E. A.	Bailey, K. H.	Broughton, D. J.

Brown, G. R.
 Butt, S. W.
 Carneiro, D. A.
 Chance, D. J.
 Childs, A. E.
 Clack, O. L.
 Cohen, M. A.
 Coker, C. H. H.
 Collery, A.
 David, C. A.
 Davies, J. Rees
 De Jong, H. W.
 Dharchoudhury, N. R.
 Dougan, K. A.
 Elkan, W.
 Enriquez, C. G.
 Felicietti, A.
 Ferguson, J. M.
 Ghevaria, V. D.
 Gogna, P. R.
 Gordon, A.
 Hall, C. A. Jnr.
 Hambleton, H. G.
 Harmon, Dr. E. M.
 Higgins, V. E.
 Hilmy, Dr. Y.
 Hoare, D.
 Hoare, G. E.
 Hope, C. J.
 Hotchkiss, W. E.
 Houghton, R. W.
 Hudson, M. W.
 Hutton-Jamieson, N.
 Johnston, J. S.
 Jones, D.
 Jones, O. V.
 Jossa, Dr. B.
 Kennerley, A.
 Kenyon, C. G.

Khaliq, M. A.
 Kim, J.
 Kooraram, M.
 Labsvirs, Dr. J.
 Lamb, K. R.
 Levi, L.
 Lumb, D. M.
 MacCallum, D.
 Madan, J. N.
 Manu, J. E. A.
 McDonald, H.
 McKenzie, J. C.
 McSwiney, M.
 Miller, S.
 Miranda, R. A.
 Mishan, E. J.
 Mitter, D. K.
 Mitter, P. B.
 Monaghan, J. L.
 Monaghan, P. A.
 Monday, H. R.
 Morgan, W.
 Muir, I.
 Nagpure, N. P.
 Narain, D.
 Nasir, Prof. M. S.
 Newton, M. O.
 Ojikutu, W. A. O.
 Olaoshun, D. A.
 Park, S.
 Paterson, R.
 Pearmain, K. C.
 Phillips, G. F. G.
 Pinkwater, M. B.
 Pownall, H.
 Pratt, S. A. J.
 Purkis, M. A.
 Qadir, A.
 Ramnath, Mrs. L.

Rao, G. S.
 Regoczy-Ritzman, E.
 Rose, H. B.
 Salgado, M. R. P.
 Sampson, P. M. J.
 Samuels, M. I.
 Santarelli, Prof. A.
 Satyamurthy, G. V.
 Schwartz, H.
 Sekoni, J. O.
 Sey, S.
 Shantharam Rao, N.
 Sharma, Prof. H. S.
 Sharma, K. P.
 Shukla, Prof. D. K.
 Sillah-Fosonah, S.
 Sinclair, W. A.
 Singh, Prof. A.
 Slade, A. T.
 Stout, Prof. D. E.
 Sur, S. P.
 Sweet, A. J. E.
 Tanourella, A.
 Taylor, D. W.
 Thompson, M. A.
 Tompsett, A. J.
 Torimiro, Miss F. T.
 Trainor, D. R.
 Tuckerman, J. R.
 Turtle, J. E.
 Udofiah, E. E.
 Watson, D. J.
 Wei Chi-Hua
 West, Miss C. H.
 Wijesinghe, S.
 Williams, C. E.
 Winrow, L. P.
 Yonren, E. H.

THE following have been admitted to Library Membership of the society:

Antioch College, Yellow Springs, Ohio, U.S.A.
 Barnard College, New York, U.S.A.
 Bhavan's College, Bombay, India.
 Central Electricity Generating Board, London.
 College of Commerce, Kolhapur, Bombay State, India.
 College of Commerce, Seoul University, Seoul, Korea.
 Franklin & Marshall College, Lancaster, Pennsylvania, U.S.A.
 Fresno State College, California, U.S.A.
 Institut für Agrarpolitik und Marktlehre, Kiel, Germany.
 Iron & Steel Board, London
 New School for Social Research, New York, U.S.A.
 New York Public Library, P.S., Grand Central Station, New York, U.S.A.
 Sri P.D.M. College of Commerce, Rajkot, India.
 Torry Research Station, Aberdeen.
 Upper Lakes Shipping Ltd., Toronto, Ontario, Canada.

Correction. See June Journal page 398. Kenzo, K. should be Kobayashi, K.

RECENT PERIODICALS AND NEW BOOKS

Applied Statistics

VOL. VIII, No. 2, JUNE 1959. *Matching and Prediction on the Principle of Biological Classification*: W. A. BELSON. *Deferred Sentencing Schemes*: I. D. HILL, G. HORSNELL and B. T. WARNER. *The Length of Cigarette Stubs*: P. G. GRAY and E. A. PARR. *The Use of Edge-punched Cards in Statistical Computation*: D. H. WARD. *Designing a Budget Survey*: W. F. F. KEMSLEY. *Pseudo-random Elements for Computers*: E. S. PAGE.

Economica

VOL. XXVI, No. 103, AUGUST 1959. *The Contribution of Economic Theory to Economic Prognostication*: R. C. TRESS. *Economic Growth and the Problem of Inflation*: I. N. KALDOR. *Advertising Appropriation Policy*: W. TAPLIN. *Ricardo on Factor Prices and Income Distribution in a Growing Economy*: H. BARKAI. *Stock and Flow Quantities: A Common Fallacy*: R. W. CLOWER. *The Rate of Interest and Walras' Law*: H. ROSE. *Reply to R. W. Clower and H. Rose*: D. PATINKIN. "Coles": *Weights and Measures, with Special Reference to Abraham Darby and the Coke-smelting Process*: R. A. MOTT.

The Review of Economic Studies

VOL. XXVI, No. 71, JUNE 1959. A "Value and Capital" Growth Model: J. R. HICKS. *Cardinal Utility for Even-chance Mixtures of Pairs of Sure Prospects*: G. DEBREU. *The Case for "Revealed Preference"*: H. M. WAGNER. *The Marginal Productivity Theory of Wages and Disguised Unemployment*: D. MAZUMDAR. *An Afterthought on the "Real Cost-Opportunity Cost"* Dispute and some Aspects of General Equilibrium under Conditions of Variable Factor Supplies: J. VANEK. *Comments on the General Theory of Second Best*: M. McMANUS. *McManus on Second Best*: K. LANCASTER and R. G. LIPSEY.

Oxford Economic Papers

VOL. 11, No. 2, JUNE 1959. *Mrs. Robinson on Simple Accumulation*: G. D. N. WORSWICK. *Exercises in the Analysis of Growth*: R. F. KAHN. *Some Problems of Definition and Measurement of Capital*: J. ROBINSON. *Unbalanced Growth*: P. STREETEN. *Arguments for Tariffs*: J. BLACK.

Yorkshire Bulletin of Economic and Social Research

VOL. 11, No. 1, JULY 1959. *Economic Theory and Policy*: J. C. GILBERT. *The Structure of World Export Trade, 1926-1953*: F. H. AWAD. *Montagu Norman and Banking Policy in the Nineteen Twenties*: D. WILLIAMS.

London and Cambridge Economic Bulletin

NO. 31, SEPTEMBER 1959. *The Economic Position. The United States Economy at Mid-year*: M. W. REDER. *The Reorganization of the Cotton Industry*: J. WISEMAN. *Prices and Wages*: H. S. BOOKER. *Quarterly Income and Expenditure*: C. H. FEINSTEIN. *The U.K. External Position*: S. J. WELLS. *Home Finance*: R. F. G. ALFORD and H. B. ROSE.

National Institute Economic Review

NO. 4, JULY 1959. *The Economic Situation: Recovery at Home; Recovery Abroad*.
NO. 5, SEPTEMBER 1959. *The Economic Situation. The Radcliffe Report: A Short Guide. The Demand for Industrial Materials, 1950-57*.

The Journal of Industrial Economics

VOL. VII, No. 3, JULY 1959. *Pricing for Profit in Retailing*: W. G. McCLELLAND. *Economy of Scale in the Metal Removal Industry*: S. B. ALPERT. *What is Relevant?*: P. L. SMITH. *Productivity in Shipbuilding*: J. R. PARKINSON. *Motor-vehicle Transport Pricing in Lebanon*: R. N. FARMER. *The Allocation of Goods Traffic between Road and Rail*: C. H. SHARP. *Price Discrimination at Retail: a Comment*: W. D. MAXWELL; *A Reply*: R. H. HOLTON. *An analysis of the Man-made Textile Fiber Industry*: E. E. MUNTZ.

The Economic History Review

VOL. XII, No. 1, AUGUST 1959. *The Stages of Economic Growth*: W. W. ROSTOW. *The Expansion and Decline of York in the Later Middle Ages*: J. N. BARTLETT. *English Merchant Capital in Bohemia in the Eighteenth Century*: A. KLÍMA. *Growth and Progress: The Nineteenth-century View in Britain*: S. G. CHECKLAND.

The British Journal of Sociology

VOL. X, No. 3, SEPTEMBER 1959. Special number on Industrial Sociology. *The Need for a Sociology of Labour*: V. L. ALLEN. *The Aims of Industrial Sociology: Some Reflections*: W. H. SCOTT. *The Dynamics of Joint Consultation*: F. FUERSTENBERG. *Technical Organization as a Factor in Supervisor-Worker Conflict*: J. H. GOLDTHORPE. *Veblen and Industrial Sociology*: J. A. BANKS. *New Ways in Industrial Sociology*: J. H. SMITH. *Status, Mobility and Anomie: A Study in Readiness for Desegregation*: M. M. TUMIN and R. C. COLLINS.

Population Studies

VOL. XIII, No. 1, JULY 1959. *The Growth of the Australian Population with Particular Reference to the Period since 1947*: W. D. BORRIE. *Married Women and Employment in England and Wales*: R. K. KELSALL and S. MITCHELL. *Some Social Characteristics of Indian Immigrants to British Guiana*: R. T. SMITH. *Changing Features in the Soviet Population Census Programme*: G. V. SELEGEN. *Fertility Trends of the Asian Population of Tanganyika*: J. G. C. BLACKER. *A Demographic Study in an Area of Low Fertility in North-east Tanganyika*: D. F. ROBERTS and R. E. S. TANNER. *Swedish Population Thought in the Eighteenth Century*: E. P. HUTCHINSON.

The Sociological Review

VOL. 7, No. 1, JULY 1959. Special number on Urbanism in West Africa, introduced by K. LITTLE. *West African Urbanisation: A Geographical View*: R. J. HARRISON CHURCH. *Urbanism as a Traditional African Pattern*: W. BASCOM. *The Yoruba Town Today*: P. C. LLOYD. *Some Urban Patterns of Marriage and Domesticity in West Africa*: K. LITTLE. *Urbanisation and Religion in Eastern Ghana*: D. K. FIAWOO. *Urbanisation and the Position of Women*: T. BAKER and M. BIRD.

Journal of Agricultural Economics

VOL. XIII, No. 3, JUNE 1959. *Seasonality and Elasticity of the Demand for Food in Great Britain since Derationing*: J. A. C. BROWN. *A Theoretical Approach to the Profit Maximisation Problems in Farm Management*: G. B. CLARKE and I. G. SIMPSON. *Some Further Results of Production Function Analysis of the Farm Management Survey Data*: R. W. M. JOHNSON. *Economic Structure of the Food Trades in the United Kingdom*: I. O. H. LEPPER. *The Supply of Concentrated Feeding-stuffs in British Agriculture*: G. HALLETT. *Restrictive Trading Agreements in the Dairy Industry*: F. D. MILLS. *Agricultural Subsidies and the Hill Farmer*: E. A. ATTWOOD.

The Farm Economist

VOL. IX, No. 4, 1959. *Some Implications of the Common Market for European Agriculture*: D. T. HEALEY. *A Study of the Dissemination of Information about a New Technique in Dairy Farming*: M. SMITH and D. SHEPPARD. *Subsidies to Meat Production in the United Kingdom*: G. HALLETT. *Improving the Agriculture of Southern Italy*: M. CHISHOLM.

The Banker

VOL. 109, No. 402, JULY-AUGUST 1959. *Reflation Takes Hold. Fuel Strategy in Fragments*: J. E. HARTSHORN. *Credit Since the Freeing. America's Bursting Grannary*: B. MORTIMER. *Lombard Street Builds up its Resistance. Expanding Ireland's Economy*: J. MEENAN.
VOL. 109, No. 403, SEPTEMBER 1959. *The Radcliffe Report: No Verdict on Money; Full Analysis and Commentary; Statistical Highlights. India Approaches the Third Plan*: G. TYSON. *Insurance: the Facts Behind the Mergers. Red Gold: Its Challenge to the West*: T. BALOGH.
VOL. 109, No. 404, OCTOBER 1959. *The City and the Election. Radcliffe under Scrutiny: What Role for Interest Rates?*: E. V. MORGAN; *What is this Liquidity?*: F. W. PAISH; *A Post-mortem on "Freedom"*: T. BALOGH. *Take-over Bids and the Law*: D. G. RICE. *Chile Changes Course*.

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- VOL. CLXXXVIII, No. 1385, AUGUST 1959. *The Banking Half-year. Credit Transfer Scheme*: R. HINDLE. *Bank Rate Vindicated?—Evidence Before the Radcliffe Committee*: W. H. WHITE. *Life Assurance Summarised*: P. STEBBINGS. *Survey of Economic Conditions in the U.S.A.: The Economy at Mid-year. Foreign Bankers in London*: A. H. SMITH.
- VOL. CLXXXVIII, No. 1386, SEPTEMBER 1959. *The Radcliffe Report: A Preliminary Survey*: SIR R. HAWTREY. *Cohen Council Mark III*: C. T. SAUNDERS. *Survey of Economic Conditions in the U.S.A.: Statistical Growth in the Economies. Some Characteristics of Banking in Belgium*: D. FIERENS. *U.K. Finance Companies: A Study of Profits, 1949–1958*: N. RUNCIE.

Barclays Bank Review

- VOL. XXXIV, No. 3, AUGUST 1959. *France's Economic Rehabilitation. The Bases of Commonwealth Trade. Britain's Forests*.

District Bank Review

- No. 131, SEPTEMBER 1959. *Money, Trade and Employment*: A. N. SHIMMIN. *Management on the Farm*: L. EASTERBROOK. *The European Free Trade Association*.

Lloyds Bank Review

- No. 53, JULY 1959. *Progress Without Tears*: HONOR CROOME. *The British Tourist Industry*: J. HENNESSY. *The Role of the Economist in Public Affairs*: E. DEVONS.

Midland Bank Review

- AUGUST 1959. "Below the Line" in Britain's Budgets. *A Glossary of "Common Market" Terms*.

National Provincial Bank Review

- No. 47, AUGUST 1959. *British Railways—Their Re-emergence*: SIR P. WARTER. *Compensation under Part I of the Town and Country Planning Act, 1959*: D. HEAP.

The Three Banks Review

- No. 43, SEPTEMBER 1959. *The Control of Public Expenditure*: R. C. TRESS. *Air Transport—The Next Decade*: P. MASEFIELD. *A Visit to Peking 1959*: J. KESWICK.

Westminster Bank Review

- AUGUST 1959. *Three American Recessions*: W. M. SCAMMELL. *Recent Trends in British Retailing*: S. POLLARD and J. D. HUGHES. *Some Aspects of Broiler Production*: B. J. F. JAMES. *Is Taxation a Deterrent?*: L. BUCK and S. SHIMMIN.

British Tax Review

- MAY–JUNE 1959. *Finance Act, 1959: Income Tax and Profits Tax*: J. S. HEATON; *Estate Duty*: J. G. MONROE. *The Regulating Power of Taxation*: C. F. CARTER. *If I May be Pardoned for Adding . . . II*: W. PHILLIPS. *Share Premiums Again*: R. INSTONE. *Succession to a Trade*: G. B. GRAHAM. *European Fiscal Systems*: HOLLAND: E. B. NORTCLIFFE.
- JULY–AUGUST 1959. *Eating One's Cake and Having It*: H. H. MONROE. *Feather-bedding Death Further Deliberated*: G. S. A. WHEATCROFT. *Schedule A Additional Assessments*: P. WHITWORTH. *United Kingdom–Eire Double Taxation Agreements and Assessments*: G. C. MASON. *European Fiscal Systems*: FRANCE: E. B. NORTCLIFFE.

Journal of the Institute of Actuaries

- VOL. 85, PART I, No. 369, 1959. *Provision for Old Age: The Future Development of the National Insurance Scheme*: I: *The Financial Structure of the Proposed Scheme*: C. M. O'BRIEN; II: *The Effect on Occupational Schemes of the Proposals for Contracting Out*: W. G. BAILEY. *The Administration of the Institute of Actuaries*: C. F. WOOD. *Continuous Mortality Investigation: Enquiry into the Desirability of Constructing a Light Table of Mortality*.

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- VOL. XXV, No. 435, AUGUST 1959. *The Work of Newspaper Trusts*.
- VOL. XXV, No. 436, SEPTEMBER 1959. *European Unity—A Review*.

Which?

- JULY 1959. *Reports on Insect Repellents, Travel Sickness Preventatives, Seals of Approval*.
- AUGUST 1959. *Reports on Spin Driers, Tumbler Driers, Aired Driers*.
- SEPTEMBER 1959. *Reports on Detergents, Canned Peaches*.

The Journal of the Institute of Transport

- VOL. 28, No. 5, JULY 1959. *Aspects of Railway Management*: D. BLEE. *Transport Costs*: H. E. OSBORN. *Road Finance*: SIR H. OSBORNE MANCE.
- VOL. 28, No. 6, SEPTEMBER 1959. *Some Impacts of Air and Road Transport in Railway Economics and Practices*: C. E. WHITWORTH. *The Expeditious Movement of Passengers and Baggage Before and After Flight*: J. W. SWANN. *Pooling Track Costs*: SIR H. OSBORNE MANCE.

Progress (Unilever)

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- No. 5, 1959. *The Provisional Results of the All-Union Population Census of 1959. Successfully Execute the Re-valuation of Fixed Assets*: A. BELYAKOV. *On the Question of Analysing the Dynamics of Labour Productivity by Index Numbers*: R. KRESIN. *On the Question of the Comparison of Regional Indicators of Quality*: V. PEREGUDOV. *On the Relations Between the*

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- No. 6, 1959. *The Development of Soviet Trade in the New Seven-year Plan and Tasks of Statistics:* S. PARTIGUL. *Some Problems of the Statistics of Productive Co-operation:* P. ZELTSER. *New Statistical Reporting for Agriculture and Procurements:* N. DYADYUNOV. *From Experience in Coding and Card-perforating Population Census Data in the Russian Federation:* I. LOGACHEV and M. MIRKIN. *Questions of the Study of the Local Experience of Economic Construction in 1921-22 (In Memory of V. I. Lenin):* V. SMOLYANINOV. *On the National Accounts in the Official Statistics of the German Federal Republic:* L. NOCHEVINKA. *On Compiling Inventories of Fixed Assets in Connection with Their Revaluation:* F. PANKROTOV. *A Meeting of Heads of the Statistical Administrations of Union-Republics:* S. KUNIN. *A Briefing Conference on the Revaluation of Fixed Assets in the U.S.S.R. Economy:* A. RIZNIK. *A Meeting on Statistics of Labour and Wages:* P. L.
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- No. 6, 1959. *The Struggle for a Balanced Composition of Domestic Trade:* M. LESZ. *The Improvement of Quality—A Major Economic Problem:* J. KOPINSKI. *Comparison of the Capital Requirements for Economic Development in Poland and the U.S.S.R.:* A. MARKOWSKI and M. RAKOWSKI. *Better Utilisation of Production Capacities—a Means to Diminish Investment:* J. CIAHUNOWICZ. *Investment and Employment in Building:* L. KALKOWSKI. *Trends in the Economic Development of the Western Territories in 1959-65:* F. KOBYLECKI. *The Economics of the Polish Transatlantic Shipping Line:* I. TARSKI. *Prospects of Private Motor-vehicle Ownership in Poland:* A. ROSTOCKI. *Some Organisational Problems of Home Trade:* B. WARZECHA. *The Results of the First Chinese Five-year Plan and Current Tasks of the Chinese Economy:* J. WERNER. *Changes in Planning and Management in the German Democratic Republic:* E. SZYMAŃSKI. *Physical Planning—a Subject of Study in Universities in Capitalist Countries:* J. BORON.
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- No. 1, 1959. *Economic Competition between the U.S.S.R. and the U.S.A. in the Production of Staple Animal Products per Head of Population:* I. RUBÍK. *Iron-ore Prices and Mining Rent:* M. KOCMAN. *Outline of a Theory of Production Costs and Prices:* C. KOŽUŠNÍK. *Methods of Calculating Labour Productivity in Terms of Foreign Exchange and the Profitability of Foreign Trade:* M. CHURANEK. *An Important Contribution of Scientists to International Understanding (the U.N.E.S.C.O. Meeting of Social Scientists in Prague):* B. URBAN. *A Seminar on Demographic Studies:* M. STÁDNÍK.

- No. 2, 1959. *Some of the Basic Features of the Development of Capitalist Industry in the Czechoslovak Republic:* M. SVANINER. *Characteristics of the Contemporary Tax System in the U.S.A.:* M. PETR. *Methodological Problems of Investigating Levels of Living in the U.S.S.R.:* I. PISAREV. *Production and Distribution in Co-operative Farms from the Viewpoints of the Economist and the Enterprise:* A. HUTNÍK. *Monopoly Profits in the U.S.A.:* V. ŠILDKRUT. *Conference of the Institutes of Economics of Socialist Countries:* V. L.
- No. 3, 1959. *The Programme for Building Communism in the Soviet Union. The Post-war Evolution of Land Tenure and Ownership in the German Federal Republic:* I. STANEK. *Technical Obsolescence, Revaluation of Fixed Assets and Depreciation Charges:* A. EMEL'YANOV. *Financial Capital and Financial Oligarchy:* L. URBAN. *A Scientific Conference on Economic Relations Between Socialist States:* J. MERVART. *The Berlin Conference on European Economic Trends:* J. CHLUMSKÝ, R. WAGNER and M. HIRŠL. *Debates of a Conference on Investment in Building:* V. ŠILHÁN.
- No. 4, 1959. *The Ratio Between Increments of Productivity and Earnings with Piece-rates under Socialism:* Z. MOŠNA. *Characteristics of World Trade in the Inter-war Period:* R. OLŠOVSKÝ. *On Economic Theory in the Programme of the Union of Communists of Yugoslavia:* F. OLIVA. *The Laws of Price Formation in the State Socialist Sector:* J. VEJVODA. *New Forms of Economic Management in the German Democratic Republic:* P. HESS and G. MANZ.

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- No. 7-8, 1959. *The Importance and Function of the New System of Agricultural Procurements:* L. ŠTROUGAL. *The Economics of the New Procurement Prices:* J. BARTUŠEK. *Balancing the Earnings of Co-operative Farms in Various Districts:* J. KOŘÁLKA. *Changes in Agricultural Planning:* J. KŇÁKAL. *Equipping Co-operative Farms with Their Own Machinery:* Z. ŠIMEČEK. *The Main Trends in the Development of Engineering Technology:* M. VÍTEK. *The Improvement of Price Formation for Non-standard Engineering Products:* V. KNOBLOCH. *The Improvement of Factory Transportation:* J. SMRČEK. *On Norm-fixing for Material Incentives in Enterprises:* O. KISS and J. LIBUS. *The Concept and Function of the Turnover Tax:* J. ROSA. *Certain Problems Relating to the Efficiency of Technical Development:* J. HON. *Methods of Calculating the Wage Fund:* F. HONZÍK. *Methods of Analysing Retail Turnover:* M. DOHNALOVÁ. *Experiences of District Planning Commissions in Disaggregating the 1959 Plan:* Z. ALOY. *Rebates and Surcharges of Sales and Supply Organisations:* L. KODET. *The Pattern of Fuel Supply and the Role of Thermal Electric Stations in the Soviet Seven-year Plan. The Application of Automatic Computers to Planning:* V. HAVLÍN. *The Current Discussion of the Reproductive Process.*

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- No. 4, 1959. *The Economic Achievements of Bulgaria in Fifteen Years of Popular Power:* A. PANOV. *Soviet Economic Aid—an Important Factor in Bulgarian Socialist Construction:* G. PEEVA. *The Permanent Improvement of Personal Welfare in the Years of Popular Power:* D. BALEVSKI. *Our Achievements and Prospects in the Chemicals Industry:* I. CHOLAKOV. *Scope for Increasing Tobacco Production in the Kurdzhali District:* K. KALCHEV. *More Quick-growing Trees Should be Planted:* I. ZHELEV. *The Fundamentals of Plan-fulfilment Analysis in an Industrial Enterprise:* G. VAKILIEV.

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- No. 1-2, 1959. *Methodological Problems of Establishing "Export Coefficients" for Industrial Products*: L. NOVAK. *Uneven Distribution of Income and Instruments for Remuneration According to Productivity*: V. PELAJIĆ. *Survey of the Influence of Instruments for Distributing the Profit in Private Enterprises*: M. UJDENICA. *Some Problems of Communal Finance*: R. DUNMANIĆ. *Organisation of Output Calculation on the Basis of Economic Units*: R. MIHELIC.
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- MAY 1959. *A Revision of the Index Number of Net Industrial Output*: O. LUKÁCS and Z. ROMÁN. *Sample Surveys in Agricultural Statistics*: L. PINTÉR. *The Application of Linear Programming in the Field of Transport*: A. FEKETE and I. KECSKEMÉTHY. *Prime Cows in Statistics in Czechoslovak Industry*: A. ČERVENÝ. *Some Questions of the Retail Clothing Trade*: M. ZAFIR. *Problems of Cattle Breeding in Vas County*: J. MÁTYÁS. *Average Real Earnings in Some Important Trades*: GY. BOKOLI. *Collective Tours Abroad in 1958*: B. MARKOS and I. MÁNYI-SZABO.
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- No. 6, 1959. *Some Questions of Economic Management Raised at the XXI Congress of the Communist Party of the Soviet Union*: I. HUSZÁR. *Prospects of the Fuel Balance of Our Economy: Relationship between the Volume and Unit-value of Domestic Sugar Output*: V. SÁGHY. *The Colonial System*: M. SIMAI. *A New System of Industrial Management in the German Democratic Republic*: I. GERGELY. *The Organisation and Methodology of Planning in Czechoslovakia during the Past Fifteen Years*: L. ANTOS.
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- 5, 1959. *"Theories on Surplus Value"—a Crushing Criticism of Vulgarised Economics*: Z. RNEA. *The Role of Capital Investments in Expanded Socialist Reproduction*: V. ROTARU. *Aspects of the Development of the Chemicals Industry*: I. VELEA. *Some Problems of the Rational Utilisation of Our Timber Resources*: A. HARAP. *Concerning the Increasing of Labour Productivity in Socialist Agriculture*: E. JIANU. *The Development of the Contractual System in Agricultural Procurement*: E. DAN. *Aspects of the Pauperisation of the Working Peasantry Between the two World Wars*: G. STANCIU. *Commodity Production and the Law of Value under Socialism*: RACHMUTH. *The Scientific Session of the Teaching Staff of the Lenin Institute of Economics, Iași*: M. NICOLAESCU. *Scope for Reducing Fuel Consumption in the Cement Industry*: O. IMITRIU. *Some Aspects of the Supply of Chemical Products to Our Agriculture*: D. DAVIDESCU. *The Aggravation of Crisis Symptoms in the Textile Industry of Capitalist Countries*: A. KUN. *6, 1959. Aspects of the Rate of Growth of the Rumanian Economy*: M. MĂNESCU and V. AUSSER. *The Development of the Oil Industry during the Years of People's Power*: N. IONESCU. *Precious Guide in Fighting Bourgeois and Revisionist Theories*: I. BULBOREA. *Some Problems of Teaching Economics in Higher Education*: A. POP, P. TĂNASIE and G. APOSTOL. *The 1960 World Agricultural Census*: M. LEVENTE. *The Development of Rail Transport in Rumania*: I. SCHÄCHTER. *The Apologetics of Bourgeois Econometric Theories*: I. ZAHARIA. *Some Aspects of Cost Accounting*: I. BLAGA. *The Economic Decline of Capitalist Countries in 1958*: E. APOSTOL. *Aggravating Disproportions in Industrial Development in the Main Capitalist Countries*: E. OBRESCU.
- 7, 1959. *Socialist Industrialisation and the Creation of the Technical-Material Base of Socialism in the Rumanian People's Republic*: D. PAULESCU. *Criticism of the Reactionary Theory that Rumania Is an Eminently Agricultural Country*: O. CONSTANTINESCU. *On the Increase of the Productivity of Social Labour*: D. EMILIAN. *Some Problems of Profitability in Engineering*: D. AZĂR. *For the Best Utilisation of Local Resources for Building Materials*: S. BANU. *On the Rational Utilisation of the Machine Park of Machine-tractor Stations*: I. MARIN. *The Reduction of Circulation Expenses—An Important Way to Save Social Labour*: M. STĂNESCU. *Aspects of the Accomplishments of Soviet Agriculture and of Its Prospects in the Seven-year Plan*: T. IONESCU. *The Economic Situation of the Working Class During Rumania's Participation in the First World War*: N. CONSTANTINESCU and V. BOZGA. *The Social and Economic Consequences of the Application of Automation in Capitalist Countries*: V. PASCU. *The Activity of the Vasile Roaita Metallurgical Works to Increase Labour Productivity*: N. RISTEA and L. APOSTOL.

NEW BOOKS

British

- ABRAMS (M.). *The Teenage Consumer*. LPE Papers No. 5. London: London Press Exchange (110 St. Martin's Lane, W.C.2), 1959. 9½". Pp. 25.
- This is an interesting and well-written short essay on the size of the teenage market and the social problems it poses for the manufacturer. Dr. Abrams estimates that the real earnings of teenagers have risen twice as fast as those of adults since 1938, and their "discretionary" spending after deduction, of National Insurance contributions and payments to parents for board) has risen as fast as their earnings. Teenage spending is reckoned to comprise 6.3% of all consumers' expenditure, but its incidence is extremely uneven between commodities: it accounts for a quarter or more of total spending on bicycles and motor cycles, records, cinema admissions and cosmetics, but only a negligible proportion of household durables, groceries and many others. The market is essentially a middle-class one, because of the more prolonged education of middle-class boys and girls and their comparatively low starting salaries. This adds to the difficulties experienced by middle-class entrepreneurs in catering imaginatively for the market. An interesting contrast is drawn by Dr. Abrams between the powerful American influence on teenage consumption standards and the remarkably conservative pattern of consumption that many working-class girls exhibit when they become married and fall back on the guidance of their mothers.]
- ALLEN (G. C.). *British Industries and their Organisation*. Fourth Edition. London: Longmans, 1959. 9". Pp. xi + 332. 25s.
- This very well-known book was thoroughly revised by its author in 1951, and has now again been largely rewritten. The historical approach, which has always been one of its most distinctive features, is retained, but the centre of attention is now shifted to the British economy of the late 1950s.
- No. 276.—VOL. LXIX.

1950s. Although the book is designed as an introductory one, Professor Allen contrives to include a considerable amount of detailed information on the industries he covers. A somewhat controversial element in his treatment of recent years is his attribution of many of the less satisfactory features of Britain's industrial performance to distortions caused by inflationary pressure.]

ASHTON (T. S.). *Economic Fluctuations in England, 1700-1800*. London: Oxford University Press, 1959. 9". Pp. viii + 199. 21s.

[To be reviewed.]

BENHAM (F. C.). *The National Income of Singapore 1956*. A Chatham House Memorandum. London: Oxford University Press for the Royal Institute of International Affairs, 1959. 10". Pp. 24. 2s. 6d.

[This is the first national-income estimate that has been made for Singapore alone. The method followed has been to estimate the consumption of private households and to add an estimate of net capital formation.]

BRENNAN (T.). *Reshaping a City*. Glasgow: House of Grant, 1959. 8½". Pp. 221. 25s.

[To be reviewed.]

BROWN (E. H. PHELPS). *The Growth of British Industrial Relations*. A study from the standpoint of 1906-14. London: Macmillan, 1959. 9". Pp. xxxvii + 414. 42s.

[To be reviewed.]

BUCHAN (A.). *The Spare Chancellor*. The life of Walter Bagehot. London: Chatto & Windus, 1959. 9". Pp. 287. 25s.

[To be reviewed.]

Business Forecasting. Publications of the Market Research Society No. 3. London: The Market Research Society and The Oakwood Press, 1958. 8½". Pp. 70. 15s.

[To be reviewed.]

CAMPS (M.). *The European Free Trade Association*. A preliminary appraisal. PEP Occasional Paper No. 4. London: Political and Economic Planning, 1959. 8". Pp. 38. 4s.

[The author describes briefly the background of the negotiations among "the Seven" and then proceeds to an exposition and discussion of the Stockholm plan. In her view it is clear that although the plan as such brings certain advantages to the participating countries, the main motive behind it has been to improve the chances for an accommodation with "the Six." She recognises some merit in this argument, but she is apprehensive lest the formation of the Seven may in the long run serve to discourage the creation of closer links between the United Kingdom and France and Germany.]

CONDLIFFE (J. B.). *New Zealand in the Making*. A study in economic and social development. Second edition. London: Allen & Unwin, 1959. 9". Pp. 316. 30s.

[To be reviewed.]

European Organisations. London: Political and Economic Planning (16 Queen Anne's Gate, S.W.1) and Allen & Unwin, 1959. 9". Pp. xvi + 372. 30s.

[Successive chapters describe the structure and functioning of ECE, OEEC, the Council of Europe, NATO, WEU, ECSC, EEC, and Euratom. Much of the material is descriptive, but the book aims to be a work of interpretation and not merely a source for reference, and frank appraisals are included of the successes and failures of the various organisations. Particular attention is paid to the contrast between the two opposing concepts of the nature of authority in international organisations—the "inter-governmental" and the "supra-national."]

EVERSLEY (D. E. C.). *Social Theories of Fertility and the Malthusian Debate*. London: Oxford University Press, 1959. 9". Pp. ix + 313. 35s.

[To be reviewed.]

FRIEDMANN (W. G.) (Ed.). *Legal Aspects of Foreign Investment*. London: Stevens, 1959. 9½". Pp. xiii + 812. 147s.

[This volume contains surveys by different authors of the legal conditions affecting foreign investment in forty selected countries. These country surveys are followed by a brief comparative analysis by the editors and by an essay on the protection afforded to the investor by international law.]

HARBURY (C. D.). *The Industrial Efficiency of Rural Labour*. Cardiff: University of Wales Press, 1958. 8½". Pp. ix + 228. 35s.

[To be reviewed.]

HINTON (R. W. K.). *The Eastland Trade and the Common Weal in the Seventeenth Century*. London: Cambridge University Press, 1959. 9". Pp. xi + 244. 32s. 6d.

[To be reviewed.]

IRVINE (A. G.). *The Balance of Payments of Rhodesia and Nyasaland, 1945-1954*. London: Oxford University Press, 1959. 8½". Pp. xiii + 643. 63s.

[To be reviewed.]

JOHNSON (L. G.). *The Social Evolution of Industrial Britain*. A study in the growth of our industrial society. Liverpool: Liverpool University Press, 1959. 9". Pp. viii + 178. 15s.

[To be reviewed.]

KORNAI (J.). *Overcentralization in Economic Administration*. A critical analysis based on experience in Hungarian light industry. Translated from the Hungarian by J. KSNAPP. London: Oxford University Press, 1959. 9". Pp. xviii + 236. 32s. 6d.

[This is a translation of a book which originally appeared in Hungarian in 1957. The Hungarian edition was reviewed by Mr. A. E. Jasay in the *ECONOMIC JOURNAL*, December 1957, pp. 718-21. In a preface to the English edition the author refers to the reforms that have been made in economic administration in Hungary since the book was written. These have been in line with the proposals made in the book.]

LENNARD (R.). *Rural England, 1086-1135*. A study of social and agrarian conditions. London: Oxford University Press, 1959. 9". Pp. 409. 45s.

[To be reviewed.]

MATTHEWS (R. C. O.). *The Trade Cycle*. Cambridge Economic Handbooks. Welwyn and London: Nisbet and the Cambridge University Press, 1959. 7½". Pp. xv + 300. 12s. 6d.

[To be reviewed.]

Oxford Economic Atlas of the World. Prepared by the Economist Intelligence Unit and the Cartographic Department of the Clarendon Press. Second edition. London: Oxford University Press, 1959. 10½". Pp. viii + 152. 42s.

[The *Oxford Economic Atlas* is in many respects more of a gazetteer than an atlas. More than half of its space is taken up by statistical tables and descriptive material, dealing, first, with world production and trade by commodities, and secondly, with the principal economic statistics of individual countries. Much of this material is naturally no more than a reproduction in very summary form of fuller data available without too much difficulty from other sources. The maps are all on a small scale. For these reasons the *Atlas* is likely to be less useful to many economists than the Oxford Regional Atlases, which appear in the same series. None the less, it is a convenient collection of up-to-date material.]

RICH (E. E.). *The History of the Hudson's Bay Company, 1670-1870*. Volume I: 1670-1763. Publications of the Hudson's Bay Record Society No. XXI. London: Hudson's Bay Record Society, 1958. 9½". Pp. xvi + 687.

[To be reviewed.]

SCHWARTZ (G.). *Bread and Circuses, 1945-1958*. London: The Sunday Times, 1959. 8". Pp. 220. 12s. 6d.

[This is a collection of the short articles that have appeared in Mr. Schwartz's celebrated column in the *Sunday Times*. Rereading them, one is conscious of the feelings they have always aroused: pleasure at his wit and forcefulness in driving home points with which one agrees, irritation at his seeming wrong-headedness in other matters. In the preface to the collection Mr. Schwartz states in more serious vein than usual the philosophy that underlies his approach.]

SMYTH (R. L.). *The Distribution of Fruit and Vegetables*. London: Duckworth, 1959. 9". Pp. 232. 30s.

[This book covers much the same ground as the Runciman Report (1957); the conclusions are broadly similar, but they are based on more explicit analysis. There is much that is stimulating, but the descriptive sections are the more satisfactory (the chapter on the primary wholesaler is particularly good). When Mr. Smyth turns to analysis and appraisal of the performance of the fruit trade he accepts the virtues of competition yet is uneasy about the results. He sometimes hints at, but he does not come to grips with, the question as to whether there is too much competition and whether we pay too much for day-to-day equilibrium. The analysis is not sufficiently rigorous, and there are a number of apparently conflicting statements; nevertheless this is a useful book on a very difficult subject of considerable importance and much interest.]

STURMEY (S. G.). *Income and Economic Welfare*. London: Longmans, 1959. 9". Pp. x + 208. 21s.

[To be reviewed.]

WILLAN (T. S.). *Studies in Elizabethan Foreign Trade*. Manchester: Manchester University Press, 1959. 9". Pp. x + 349. 35s.

[To be reviewed.]

Australian

DOWSETT (W. T.). *Elementary Mathematics in Economics*. Nedlands, W. Australia: University of Western Australia Press (London: Pitman), 1959. 9". Pp. xxi + 250. 30s.

[To be reviewed.]

New Zealand

WESTSTRATE (C.). *Portrait of a Modern Mixed Economy: New Zealand*. Wellington: New Zealand University Press (London: Cambridge University Press), 1959. 9". Pp. x + 310. 22s. 6d.

[To be reviewed.]

Indian

GOPAL (M. H.). *Towards a Realistic Tax Policy for India, 1959-66*. The Presidential Address to the forty-first session of the Indian Economic Conference. Madras: University of Madras, 1959. 9". Pp. 46.

[A detailed study of current Indian tax practice and of proposals that have been made to reform it (notably by Mr. Kaldor) leads the author to the conclusion that in the present circumstances of the country the objective of equity in taxation must take second place to the objective of stimulating growth, the two objectives being unhappily in conflict to a considerable extent.]

MOOKERJEE (SUBIMAL). *Factor Endowments and International Trade*. A statement and appraisal of the Heckscher-Ohlin theory. Published under the auspices of the Indian School of International Studies. Bombay: Asia Publishing House, 1958. 9". Pp. viii + 99. 20s.

[To be reviewed.]

Ceylonese

LOGANATHAN (C.). *Development Savings Banks*. An institutional device to promote economic development and assist in the provision of social justice in Asian countries within the framework of democracy. Colombo: Ceylonese National Council, International Chamber of Commerce, 1959. 9". Pp. xiii + 147.

[Under the scheme proposed here, a Bank would be set up which would receive compulsory and/or voluntary contributions from employers and employees, with various tax concessions to encourage voluntary contributions. It would also act as a channel through which part of aid granted by foreign governments to the government of Ceylon would be distributed to the economy. The funds at the disposal of the Bank would be invested in development projects on an equity basis,

in such a way as to enable small undertakings to participate to a greater extent than is usually possible from publicly raised finance while at the same time diffusing the profits over a wide class of the population.]

American

ADAMS (DORIS G.). *Iraq's People and Resources*. Berkeley, California: University of California Press (London: Cambridge University Press, 1959), 1958. 10". Pp. viii + 160.

[About half of this monograph is devoted to a discussion of population tendencies in Iraq and to a general cultural description of its people. The author then considers the level of income and consumption, and the potentialities of and obstacles to economic development. In many respects the purely economic problems of Iraq are less acute than in most under-developed countries. Oil resources form an ample source of finance, and this is made effective by legislation under which the greater part of oil revenues is awarded to the Development Board, a semi-autonomous body; at the same time the population is not excessive in relation to the amount of cultivable land, so that the application of most of the development funds to irrigation and other forms of agricultural improvements is a feasible policy. Cultural and administrative obstacles to development are none the less formidable. The chief value of Mrs. Adams's monograph is in her treatment of these problems, and in her description of the underlying social conditions, which is excellently done.]

AITKEN (H. G. J.) and others. *The American Economic Impact on Canada*. Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1959. 9½". Pp. xviii + 176.

[This contains the following papers: "The Changing Structure of the Canadian Economy, with Particular Reference to the Influence of the United States," by H. G. J. Aitken; "Recent American Influence in Canada, with Particular Reference to Economic Factors and Canadian Reaction," by J. J. Deutsch; "Canadian Economic Policy from 1945 to 1957—Origins and Influences," by W. A. Mackintosh; "The Impact of United States Farm Policy on Canadian Agriculture," by C. L. Barber; "The American Economic Impact on Quebec," by M. Lamontagne; "The Flow of United States Investment Funds into Canada since World War II," by I. Brecher.]

ALLEN (W. R.) and ALLEN (C. L.) (Eds.). *Foreign Trade and Finance*. Essays in international economic equilibrium and adjustment. New York and London: The Macmillan Company, 1959. 8½". Pp. xii + 500. 42s.

[This is a collection of twenty-three reprinted articles, mostly very well-known ones. Most of them are reproduced in part only. Brief commentaries by the editors follow the five main sections into which the book is divided.]

BABSON (T. E.) and BABSON (D. L.). *Investing for a Successful Future*. How to surmount rising prices and high taxes. New York and London: The Macmillan Company, 1959. 8½". Pp. viii + 312. 34s. 6d.

[The purpose of this book is to give advice to those who wish to plan their savings and the uses to which they are put on a long-term basis, with particular reference to provision for old age. In order to give the background to this, the authors survey some of the present characteristics of the American economy and explain why in their view conclusions drawn from the experience of earlier times, especially the crash of 1929, are likely to be misleading. They emphasise the need to form a clear plan and stick to it, acquiring securities with the desired characteristics and holding on to them without making any attempt to out-guess the market about short-term price movements.]

BACKMAN (J.). *Wage Determination*. An analysis of wage criteria. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 9". Pp. xv + 316. 51s.

[To be reviewed.]

BAIN (J. S.). *Industrial Organization*. New York: John Wiley (London: Chapman & Hall), 1959. 9½". Pp. xviii + 643.

[To be reviewed.]

BAUMOL (W. J.). *Business Behavior, Value and Growth*. New York and London: The Macmillan Company, 1959. 8½". Pp. xiv + 164. 33s.

[To be reviewed.]

BOCK (B.). *Concentration Patterns in Manufacturing*. Some findings from an inquiry into the relevance of data being used to measure market shares in specified industries. National Industrial Conference Board, Studies in Business Economics No. 65. New York: National Industrial Conference Board, 1959. 9". Pp. 128.

[This is a factual survey, based on statistics published in a report from the Senate Subcommittee on Anti-Trust and Monopoly in 1957; these statistics were in turn drawn from data in the 1954 Census of Manufactures. The meaning of the statistics is examined, and a study is made of comparative degrees of concentration in different product classes and industries in 1947 and of trends in concentration between 1947 and 1954. The chief criteria used are the proportions of output due to the four largest companies and to the eight largest companies.]

BRADLEY (P. D.) (Ed.). *The Public Stake in Union Power*. Charlottesville, Virginia: University of Virginia Press, 1959. 9½". Pp. x + 382.

[To be reviewed.]

BREMS (H.). *Output, Employment, Capital and Growth*. A quantitative analysis. New York: Harper & Brothers, 1959. 8½". Pp. xiii + 349.

[To be reviewed.]

COLE (A. H.). *Business Enterprise in its Social Setting*. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½". Pp. xiii + 286. 45s.

[To be reviewed.]

COMMONS (J. R.). *Institutional Economics*. Its place in political economy. In two volumes. Madison, Wisconsin: University of Wisconsin Press, 1959. 8". Pp. xiii + 921.

[This is a reprint of one of Commons' principal works, which first appeared in 1934.]

Economic Lessons of Postwar Recessions. Report of the Committee on Economic Policy. Washington, D.C.: Chamber of Commerce of the United States, 1959. 8½". Pp. 31. 50c.

[This contains a brisk survey of the course of events in America's three post-war recessions, with special reference to that of 1957-58, and of the opinions that have been expressed about them by economists and other commentators. The conclusions drawn are that complete stabilisation of the level of activity is bound to be extremely difficult and is perhaps not necessary so long as major depressions are avoided. In view of the difficulties of forecasting, substantial reliance on built-in stabilisers is recommended.]

FOX (H.) and WORTHY (N. B.). *Severance Pay Patterns in Manufacturing*. National Industrial Conference Board, Studies in Personnel Policy, No. 174. New York: National Industrial Conference Board (460 Park Avenue, New York 22), 1959. 11". Pp. 48.

[This is a factual study of the extent and form of arrangements in existence in American manufacturing industry under which some compensation is paid to employees who lose their jobs because of factors outside their control, such as the shutdown of a plant or operation. The study is based on an inquiry conducted among some 700 firms. It emerges that rather more than half of the larger companies have some arrangement of this sort, and a lower, though still sizeable, proportion of smaller companies. The amount of the payment usually varies with the length of service; under the more generous schemes it may be of the order of a week's pay for each year of service, while under others there is a maximum of one month's pay.]

HARBISON (F.) and MYERS (C. A.). *Management in the Industrial World*. An international analysis. New York and London: McGraw-Hill, 1959. 9". Pp. xiv + 413. 54s. 6d.

[The first part of this book is an essay of some 130 pages on the concept of management. This essay deals with management in turn as an economic resource, as a system of authority, and as a nature of management. Emphasis is placed on the mutual interaction of economic growth and changes in the nature of management. The second part of the book contains a series of papers by the two main authors and by a number of other contributors on management in the following countries: India, Egypt, Chile, Israel, France, Italy, Japan, Germany, Sweden, Great Britain, the Soviet Union and the United States. A final chapter deals with the management of foreign firms in underdeveloped countries.]

HAYNES (W.). *Brimstone: The Stone that Burns*. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 8½". Pp. xx + 308. 45s.

[The first edition of this readable history and description of the sulphur industry appeared in 1942. It is not primarily an economic history, but the author has plenty to say about economic issues as well as about technical problems and about the personalities of the industry. Extensive statistics are provided in an appendix, and there are numerous photographs.]

HAZLITT (H.). *The Failure of the "New Economics"*. An analysis of the Keynesian fallacies. Princeton, New Jersey and London: Van Nostrand (358 Kensington High Street, London W.14), 1959. 8½". Pp. xii + 458. 56s. 6d.

[The author has read assiduously Keynes' *General Theory* and the commentaries on it, and he works through the book pointing out all the fallacies he believes it to contain. Almost everything Keynes said is attacked with zest, not to say rancour. In such a cannonade it is inevitable that a certain number of targets should be hit; but the book cannot really be regarded as a serious contribution to knowledge. It is not, for example, at all on a par with that equally vigorous but much more discerning polemic, Terborgh's *Bogey of Economic Maturity*. The points that Mr. Hazlitt regards as irretrievable fallacies include many that have by now been settled to the satisfaction of almost all economists, such as the equality of savings and investment. Mr. Hazlitt's particular *bête noire* is Keynes' "anti-business bias," of which he finds traces on almost every page of the *General Theory*.]

HERSUM (A. C.). *Operating Results of Variety Chains*. Bureau of Business Research Bulletin No. 153. Boston, Mass.; Harvard University Graduate School of Business Administration (London: Bailey & Swinfen), 1958. 11". Pp. 37. 16s.

[This report is the 27th annual report in its series. Particulars are given of volume of sales, earnings, expenses and margins.]

HOROWITZ (M. A.). *Commuting Patterns in Seven Standard Metropolitan Areas in Eastern Massachusetts, 1958*. Boston, Mass.: Division of Employment Security of the Commonwealth of Massachusetts (881 Commonwealth Avenue, Boston), 1959. 11". Pp. xi + 39.

[The findings are presented without much comment or attempt to draw conclusions of more than local interest. The main general conclusion appears to be that in a densely populated region like that studied, the labour market is a wide one and people are willing to travel long distances to work.]

KLEIN (B. H.). *Germany's Economic Preparation for War*. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½". Pp. xi + 272. 0s.

[To be reviewed.]

LEISEN (M. W.). *Wages and Economic Control in Norway, 1945-57*. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 9½". Pp. iv + 174. 36s.

[To be reviewed.]

LINDEN (F.). *The Business of Department Stores*. National Industrial Conference Board Technical Paper No. 7. New York: National Industrial Conference Board (460 Park Avenue, New York 22), 1959. 11". Pp. 48.

[This paper consists of two parts. The first reviews trends in department-store sales during the last ten years and considers to what extent the decline in the competitive position of department stores, which the statistics at first sight suggest, has in fact taken place. The second part deals with the effect of the weather on department-store business.]

MARCH (J. G.) and SIMON (H. A.). *Organizations*. New York: John Wiley for the Graduate School of Industrial Administration, Carnegie Institute of Technology (London: Chapman & Hall), 1959. 9½". Pp. xii + 262. 48s.

[To be reviewed.]

MARKHAM (J. W.). *The Fertilizer Industry*. Study of an imperfect market. Nashville, Tennessee: Vanderbilt University Press, 1958. 9½". Pp. 249. \$6.00.

[To be reviewed.]

MASSON (R. L.). *New Shares for Old. The Boston and Maine stock modification.* Boston, Mass.: Harvard University Graduate School of Business Administration, 1958 (London: Bailey & Swinfen, 1959). 8½". Pp. xvii + 398. 36s.

[This is a detailed study of the process by which the Boston and Maine Railroad made use of a 1948 amendment to the Interstate Commerce Act to simplify a complex and unwieldy hierarchy of stock classes and reduce the total structure to fit earnings prospects. The successive stages of the plan evolved by the management are described. The case-study is then used to cast light on the general issues involved in devising such a scheme and working out canons of fairness.]

PEN (J.). *The Wage Rate under Collective Bargaining.* Translated from the Dutch by T. S. PRESTON. Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 9½". Pp. xiv + 216. 48s.

[To be reviewed.]

RAPKIN (C.). *The Real Estate Market in an Urban Renewal Area.* New York: New York City Planning Commission, 1959. 9". Pp. xii + 139.

[This is a study of the problems of a declining urban area, namely a group of twenty blocks in Manhattan's West Side. It is a heterogeneous district, including both expensive multi-storey apartment blocks facing Central Park and densely occupied brownstone rooming houses, let to an increasing extent to Puerto Ricans. The report contains voluminous statistics on types of owner and occupancy, prices and assessed values, mortgage availability and profits of owners. A paradoxical implication of the findings is that the social deterioration of a district does not necessarily involve a decline in property values; indeed, the profits to be had from letting out a large old house on a one-room-per-family basis seem to be considerable. This means that the prospects of substantial demolition and redevelopment being carried out by unaided private enterprise are small, notwithstanding the attractive potentialities of the district, and public aid on a large scale is therefore likely to be needed to bring this about. The reader is left with slight misgivings about the economic rationale of a policy of large-scale demolition and replacement of existing property by dwellings designed for middle income groups, unless it is to be accompanied by an equally determined effort to provide attractive accommodation for the Puerto Ricans and others to whom these squalid and by no means cheap quarters are none the less at present the best available.]

RIPPY (J. F.). *British Investments in Latin America 1822-1949. A case study in the operations of private enterprise in retarded regions.* Minneapolis, Minnesota: University of Minnesota Press (London: Oxford University Press), 1959. 9½". Pp. xii + 249. 40s.

[To be reviewed.]

SHUBIK (M.). *Strategy and Market Structure. Competition, oligopoly and the theory of games.* New York: John Wiley (London: Chapman & Hall), 1959. 9". Pp. xviii + 387. 64s.

[To be reviewed.]

SILCOCK (T. H.). *The Commonwealth Economy in Southeast Asia.* Durham, North Carolina: Duke University Press for the Duke University Commonwealth-Studies Center (London: Cambridge University Press), 1959. 8½". Pp. xvii + 259. 30s.

[To be reviewed.]

SMITH (E. L.). *Common Stocks and Business Cycles. A practical analysis of the basic causes and patterns of cyclical behavior in economic series.* New York: The William-Frederick Press, 1959. 9". Pp. 226. \$10.00.

[Mr. Smith has been concerned with stock-market problems for over fifty years, and he is the author of *Common Stocks as Long-term Investments*, which was published in 1924 and attracted a considerable amount of attention. In his new book he develops his ideas further in the light of subsequent experience, especially that of 1929-32. His theory of cyclical fluctuations is based on weather cycles, and a prominent part in it is played by the notion of 89½-year periodicity.]

Thirty Years of Research in Human Fertility: Retrospect and Prospect. Papers presented at the 1958 Annual Conference of the Milbank Memorial Fund. New York: Milbank Memorial Fund, 1959. 9". Pp. 157. \$1.00.

[The papers included in this symposium are divided into three groups, dealing respectively with Trends and Measures of Fertility, Fecundity and Contraception, and Social and Psychological Factors affecting Fertility. Most of the empirical material considered relates to the United States.]

TIMOSHENKO (V. P.) and SWERLING (B. C.). *The World's Sugar. Progress and Policy.* Stanford, California: Stanford University Press for the Stanford University Food Research Institute (London: Oxford University Press), 1957. 9½". Pp. xii + 364. 52s.

[This appears in a Stanford series dealing with Food, Agriculture and the Second World War. However, it views the problems of sugar in a broader context than this might suggest, and though the effects of the war are fully treated, much attention is also paid to events since 1945. In the authors' opinion the International Sugar Agreement has not made any real contribution to the solution of the industry's main problems. These problems are largely political in origin, and stem from the disparity in the position of Cuba and other countries that sell the bulk of their output in the competitive world market, on the one hand, and colonial producers, such as Puerto Rico and the British West Indies, on the other, who have access to protected markets at above the world price.]

VICKERS (D.). *Studies in the Theory of Money, 1690-1776.* Philadelphia, Pennsylvania: Chilton Company, 1959. 8½". Pp. viii + 313. \$6.50.

[To be reviewed.]

WALD (H. P.). *Taxation of Agricultural Land in Underdeveloped Economies. A survey and guide to policy.* Cambridge, Mass.: Harvard University Press (London: Oxford University Press), 1959. 8½". Pp. xiii + 231. 36s.

[To be reviewed.]

WARNTZ (W.). *Toward a Geography of Price. A study in geo-econometrics.* Philadelphia, Pennsylvania: Pennsylvania University Press (London: Oxford University Press), 1959. 8½". Pp. 117. 40s.

[To be reviewed.]

WATILLON (L.). *The Knights of Labor in Belgium.* Translated by F. MEYERS. Los Angeles, California: Institute of Industrial Relations, University of California, 1959. 9". Pp. 35. \$1.50.

[This is a translation by an American labour historian of a pamphlet written in the 1920s. The author is now Director General of Social Security in the Belgian Ministry of Labour and Social Security. The pamphlet describes how organised labour in certain Belgian industries, especially glass-making and coal-mining, became affiliated to the American Knights of Labour. Whereas a similar affiliation in some British industries proved short-lived, the Belgian Knights of Labour remained in existence until the 1930s.]

ZANK (E. E.) and BAKKEN (H. H.). *Light and Power. Rates and costs of service in Wisconsin R.E.A. Co-operatives.* Madison, Wisconsin: University of Wisconsin Press, 1959. 9½". Pp. xii + 180. \$4.00.

[Rural Electrification Administrative co-operatives were first set up under federal auspices as a relief measure in 1935. They were designed to bring electricity to regions considered sub-marginal by privately operated undertakings. They have continued to grow, though at a diminishing rate, as population and industry have spread into rural suburban areas. This study is intended primarily for those actually engaged in a managerial capacity in the co-operatives. It analyses the forms taken by operating costs, depreciation and other costs, and the relation of these to rate-fixing. The authors favour the adoption of a simple two-part rate in place of the more complex systems now in use.]

Dutch

ARROW (K. J.) and HOFFENBERG (M.). *A Time Series Analysis of Interindustry Demands. Contributions to Economic Analysis, XVII.* Amsterdam: North-Holland Publishing Company, 1959. 9". Pp. 292. 55s.

[To be reviewed.]

BJERVE (P. J.). *Planning in Norway, 1947-56. Contributions to Economic Analysis, No. XVI.* Amsterdam: North-Holland Publishing Company, 1959. 9". Pp. xx + 383. 70s.

[The author is Director of the Central Bureau of Statistics of Norway. His book is a critical appraisal of the national budgeting policy that has been pursued in Norway. This policy involves the annual publication of projections of the chief economic variables of the system, based on a comparatively unsophisticated theoretical model but involving a fair amount of detail. It has not always been quite clear whether these projections have the status of forecasts or of goals, but to some extent at least the original intention that they should be goals has been retained. The author's

general verdict on the system is favourable. He argues that the not infrequent deviations between projected and actual figures do not necessarily point to a fault in the system, and he argues that the need to produce consistent sectoral projections imparts realism to government policy, even if the principles of calculation are crude. Some faults have admittedly been present, particularly the insufficient attention given to prices in the budgeting system, and inflation has not been avoided. A curious feature has been that investment has persistently tended to absorb a larger proportion of gross national product than it was planned to do. Dr. Bjerve has written a clear and able study of an interesting experiment.]

HARTOG (F.). *European Trade Cycle Policy*. Leyden: Sythoff, 1959. 9½". Pp. 55. D.fl. 2.90.

[This pamphlet was written in 1956, but the author believes that subsequent events have served to strengthen, not weaken, his conclusions. He argues that the high-level stability enjoyed since the War is not something that can be permanently taken for granted. Policy to ensure it is needed, not least in the inflationary stage of the cycle, and this policy to be fully effective calls for a large degree of international co-operation.]

Swedish

Prospects of Development in Western Europe, 1955-1975. Industrial Institute for Economic and Social Research, Stencil 1. Stockholm: Industrial Institute for Economic and Social Research, 1959. 11½". Pp. 65.

[The author of this short study is Professor I. Svennilson. It contains statistical projections of output and employment by sectors for Europe as a whole and for groups of countries within Europe. The projections are in very broad outline, but the author gives a good lucid statement of the main considerations to be borne in mind in putting numerical values on such key variables as the saving-income ratio, the rate of growth of productivity and the rates of growth of the labour force in different sectors.]

Danish

ULDALL-HANSEN (H.). *Tid og Rente*. (Time and Interest.) Copenhagen: Dansk Videnskabs Forlag, 1959. 10". Pp. 248. Danish kroner 35.

[As described in the brief English summary, this is a theoretical and in parts mathematical analysis of the relationship between the length of life of a bond and its yield. An important part in the argument is played by the assumption that the future can be usefully divided into two parts, the "predictable" period, during which an investor may reasonably predict a trend in the interest rate, and the subsequent period.]

Finnish

HONKO (J.). *Yrityksen Vuositulos*. (The Annual Income of an Enterprise and its Determination.) Helsinki: Liiketaloustieteellinen Tutkimuslaitos, 1959. 10". Pp. 246.

[This is a theoretical work, which builds on the ideas of Fisher, Lindahl and others, in an attempt to distinguish the various meanings of the concept of income and their application. Emphasis is placed on the appropriateness of different concepts of income for different purposes. The author argues that a concept of income based on expectation would be superior to one based on realisation in a world of perfect foresight where income statements could be made as often as desired instead of at set annual intervals, but that in practice the concept based on realisation has to be preferred.]

LARNA (K.). *The Money Supply, Money Flows and Domestic Product in Finland, 1910-1956*. Helsinki: The Finnish Economic Association, 1959. 10". Pp. 227. Finnish Markkas 1000.

[The author considers first the concepts of money supply, money flows and marketed domestic product and their measurement for the Finnish economy. After a discussion of the decomposition analysis of their mutual inter-relations. This is done first by working out the movement over time of the velocity of circulation, calculated in various ways, and then by a more elaborate econometric analysis. The purpose of the latter is to "explain" movements in marketed domestic product in terms of money supply and money-flow variables. The results are encouraging, and it is suggested that this approach may provide a useful method of computing an approximation to national income more rapidly than hitherto and for shorter periods than a year.]

Japanese

TAKAHASHI (C.). *Dynamic Changes of Income and its Distribution in Japan*. Institute of Economic Research, Hitotsubashi University, Economic Research Series No. 3. Tokyo: Kinokuniya Bookstore, 1959. 8½". Pp. 182.

[To be reviewed.]

TAKENAKA (T.). *Business and Government*. Tokyo: Science Council of Japan, 1958. 8½". Pp. 52.

[The author is interested in the currents of opinion which have shaped relationships between persons and government in the sphere of public utilities. He discusses this with special reference to the experience of the states of Wisconsin and California.]

Chilean

Outlook for Chile's Foreign Trade and Economic Growth, 1959-1965. Santiago: Institute of Economic Research, University of Chile, 1959. 10½". Pp. 90.

[The greater part of this report is devoted to projection of Chile's capacity to import. It is estimated that the volume of imports should be able to rise at something near to the average rate recorded in recent years, namely somewhat less than 6% per year. The projections of gross national product are based on those of the capacity to import, and are likewise moderately optimistic. The analysis is confessedly incomplete, since no direct attention is paid to the problem of inflation, which has in the past beset the Chilean economy so severely. From 1953 to 1958 the real national income fell, while population increased rapidly. This was partly due to a decline in the capacity to import in 1954-55, but this factor was not in operation in 1956-57, and the stagnation of those two years may be attributed instead to the government's severe anti-inflationary programme. The authors' method of analysis implicitly assumes that growth in 1959-65 will not be subject to any obstacles of this sort.]

Official

BRITISH

MINISTRY OF AGRICULTURE, FISHERIES AND FOOD. *Domestic Food Consumption and Expenditure, 1957*. Annual Report of the National Food Survey Committee. London: H.M. Stationery Office, 1959. 9½". Pp. vi + 137. 8s. 6d.

[This year's report follows the now familiar lines, with a number of additions. A chapter has been included on dietary differences associated with occupational status and skill. A special section compares the diets of families containing older and younger children.]

INTERNATIONAL

GENERAL AGREEMENT ON TARIFFS AND TRADE. *International Trade 1957-58*. Geneva: GATT, 1959. 9½". Pp. 318. \$2.00.

[Hitherto this publication has appeared annually; the present issue for the first time deals with developments over a period of two years. Much useful statistical information and comment is provided on trends in world trade in total, by commodity and by country. Particulars are also given of developments in the sphere of commercial policy.]

INTERNATIONAL BANK FOR RECONSTRUCTION AND DEVELOPMENT. *Fourteenth Annual Report, 1958-1959*. Washington, D.C.: International Bank for Reconstruction and Development, 1959. 11". Pp. 56.

[This year's activities are described, with details of the loans granted.]

INTERNATIONAL LABOUR OFFICE. *Labour Costs in European Industry*. Geneva: International Labour Office, 1959. 9½". Pp. viii + 170. 7s. 6d.

[This study provides data on labour costs in selected industries in different European countries. Comparison is then made both between labour costs in different industries in the same country and between labour costs in the same industry in different countries. Particular attention is paid to the relative magnitude of basic wages, wage premiums (holiday pay, overtime premiums, etc.) and other labour costs (bonuses, social security contributions, etc.). The importance of this last item in total labour costs varies from 12% in the United Kingdom to 42% in Italy and 46% in Yugoslavia.]

INTERNATIONAL MONETARY FUND. Annual Report of the Executive Directors for the Fiscal Year ended April 30, 1959. Washington, D.C.: International Monetary Fund, 1959. 9". Pp. 237.

[This report is devoted mainly to a survey of developments during the year in the spheres of international payments and domestic financial policies. Brief particulars of the Fund's own activities are also included.]

ORGANISATION FOR EUROPEAN ECONOMIC CO-OPERATION. The Work of the Organisation for European Economic Co-operation. A report by the Secretary-General. Paris: O.E.E.C., 1959. 9½". Pp. 114. 7s.

[This is a general survey of the functions of O.E.E.C. It is designed to help create a better informed public opinion on the Organisation, and to place in their context the particular activities which from time to time attract unusual publicity.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. FAO Mediterranean Development Project. The integrated development of Mediterranean agriculture and forestry in relation to economic growth. A study and proposals for action. Rome: F.A.O., 1959. 11". Pp. 213. \$2.00.

[Although much of this report is occupied by fairly technical discussion of the agriculture and forestry of the region, it has a more general interest as well, and care is taken throughout to view agriculture in the broader context of economic development as a whole. The first two chapters describe the general characteristics of the region and consider the objectives of policy and the roles to be played by different sectors of the economy and by the state. Chapters 3 and 4 then treat in more detail the programmes needed for increasing production and improving land use. This is followed by studies of individual countries, namely Iraq, Syria, Lebanon, Tunisia, Morocco, Spain, Yugoslavia, Greece, Israel and Turkey. The F.A.O.'s chief consultant for the project was Dr. Thomas Balogh.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. The State of Food and Agriculture 1959. Rome: F.A.O., 1959. 11". Pp. 197. 10s.

[This contains a survey of recent events and outlook and also two more general chapters. The first of these is devoted to the relationship between agricultural incomes and the level of economic development; the second considers certain problems of agricultural development in backward countries with special reference to marketing, land tenure and investment.]

UNITED NATIONS FOOD AND AGRICULTURE ORGANISATION. Yearbook of Fishery Statistics. Vol. VIII, 1957. Rome: F.A.O., 1959. 12". Pp. xvi + 334. 20s.

[The last issue of this yearbook to deal with international trade in fishery products appeared in 1955. The present issue is differently arranged. It contains no separate country tables, but is divided up instead according to seven groups of fishery commodities. Each section shows imports and exports by country for the commodity group in question.]

