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CONTENTS

The Origins of Parsi Enterprise

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The Parsis played an outstanding role in the growth of Indian industry in the nineteenth century. They pioneered the cotton textile industry in western India; and in all types of industrial and trading activity in the Bombay Presidency their participation was quite out of proportion to their numbers.

The Parsis' success has invited a number of explanations.[1] It has been attributed to their spirit of enterprise, their lack of caste, their foreign origin, etc. Some evidence can be found for these explanations; but their significance cannot be assessed unless various contributory factors are considered together. Further, such explanations do not take into account the historical character of the Parsi achievement. The Parsis played a smaller and diminishing role in the industrial development in this century, but that cannot be attributed to their becoming less enterprising, or their developing a caste system. Many of the old Parsi concerns, founded in the nineteenth century, continue to exist; but their growth in recent decades has been less rapid than the overall growth of industry in the country.

Thus, it is necessary to put Parsi enterprise in its historical context. Fortunately the documentary basis for doing so is fairly firm. With their strong sense of community, the Parsis have written a great deal about themselves.[2] Community news was reported

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1. According to Sir John Malcolm. "There is no body of natives so remarkable for their intelligence and enterprise as the Parsis." (Dosabhoy Framji Karaka, *History of the Parsis, including their manners, customs and religion and present position*. London 1884, Vol. I, p. 116). For more recent views in the same vein cf. Vera Anstey, *The Economic Development of India*. London. 4th ed. 1952, p. 232, Daniel Houston Buchanan, *The Development of Capitalist Enterprise in India*, New York 1934, p. 144.

2. D. F. Karaka, *op. cit.*, Vols. 1 and 2. Khan Bahadur Bomanji Byramji Patel, *Parsi Prakash* (in Gujarati), Bombay. Ratanji Framji Wachha, *Mumbaino Bahar*, (in Gujarati), Bombay 1874. H. D. Darukhanawala, *Parsi Lustre on Indian soil*, Bombay 1939.

extensively in their newspapers.[3] Hence the record is quite complete for most of the nineteenth century; fragmentary information is also available for the two previous centuries. The object of this paper is to review the evidence and explore the origin of the entrepreneurial activities of Parsis.

The Parsis came to Gujarat in the year 697 A.D. to escape persecution by Mohammedans in Persia. Nineteen years later they moved to Sanjan, which was then a port in the Gulf of Cambay. Nothing is heard of them in the following 700 years, and it is likely that they were substantially assimilated in the indigenous population and took up the native occupation of farming. Right up to the 19th century the Parsis showed the great influence of Hindu ways and manners. Some of them had Hindu names; and their rituals at birth, marriage, etc. also incorporated Hindu customs.

Sanjan was overrun by the Muslims in 1315, and apparently the Parsis were uprooted. They are next found settled in the Surat-Navsari area in the fifteenth century; by then they were no longer confined to agriculture, but had extended their activities to tax farming, shipbuilding, shipping and trading.[4] The growth of occupational and economic differentiation was probably behind the theological controversies that began to arise in the fifteenth century. We hear of leading Parsis getting worried about the adulteration of their religion and exchanging notes with the Parsis in Persia regarding correct procedures.

In the seventeenth and eighteenth centuries there were some famous shipbuilders among the Parsis of Surat. There is a reference in East India Company records to a Parsi shipbuilder in Surat, Cursett, who built warships around 1672.[5] When the East India Company wanted a ship in 1735, its agent was sent to Surat, and he placed an order with one Dhunjibhoy. Later, Lowjee Nusserwanji, a Parsi foreman of Dhunjibhoy, was engaged by the Company to set up a shipyard in Bombay.[6] He set up the Mazagaon Dock which

3. *Bombay Samachar* and *Jam-e-Jamshed* in Bombay and *Gujrat Mitra* in Surat were the best known ones; besides there were dozens of magazines and pamphlets.

4. D. F. Karaka, *op. cit.*, Vol. 1, pp. 1-30.

5. Rottonjee Ardeshir Wadia, *The Bombay Dockyard and the Wadia Master Builders*. Bombay 1955, p. 106.

6. *Ibid.*, pp. 124-27.

built over 350 ships in the next century and a half.[7]

Besides building ships, the Parsis were also engaged in foreign trade and in textile manufacture. They were particularly active in the trade with the Persian and Arabian Gulfs;[8] they also exported opium to China.[9]

The major export of Surat was cotton and silk textiles; and the Parsis were actively engaged in their manufacture. Some Parsi families were famous for their silks, and at least one of them—the Chhois—continued to manufacture into the twentieth century.[10] By the eighteenth century, the Parsis were almost entirely urban residents; few of them engaged in farming. But some of them were rewarded with *Jagirs* (estates) by Mogul kings. Thus, Meherji Rana, a priest who educated Akbar in Zoroastrianism, was given a heritable estate of 80 acres by him; his son was given a further 40 acres.[11] The Dordi family also received a *Jagir* from Jehangir, the son of Akbar. A Parsi named Sorabji Kavasji (1697-1760) repaired a clock belonging to the Mogul Emperor in 1744; in return he was given the title of Nek Sant Khan, as well as a number of *Jagirs* in Surat District and the right to farm the customs of Surat. His sons, who were awarded titles of Beheremandkhan and Talyarkhan, took up trade in cotton, piece-goods and silk, besides their inherited activities.[12] Beginning with Changa Asa in 1419, many Parsis became tax farmers.[13]

Thus, the Parsis in the eighteenth century engaged in manufacture, shipbuilding, exports, imports and revenue collection. They were therefore in a very favourable position for capital accumulation.

Yet the Parsis were not the only people who had opportunities for capital accumulation. The Moguls systematically farmed out taxes in their entire empire, and created hundreds of Jagirdars. The same practice was followed by other kings also. And trade and handicrafts were not the monopoly of Parsis. In the Surat region

7. *Ibid.*, pp. 329-360 where a list is given.

8. Holden Furber, *John Company at Work*, Cambridge (Mass.) 1951, pp. 21, 66.

9. D. F. Karaka, *op. cit.*, Vol. 1, p. 45.

10. H. D. Darukhanawala, *op. cit.*, pp. 382-83.

11. D. F. Karaka, *op. cit.*, Vol. 2, p. 374.

12. *Idem.*, pp. 21-23.

13. *Idem.*, pp. 4-7.

itself we know of some extremely rich Hindu and Muslim merchants.[14]

The reason for the later success of the Parsis and the failure of other rich people must be looked for in the Parsis' close contacts with the British East India Company. The East India Companies from the sixteenth to the early eighteenth century were not traders in the same sense as the "country" traders of Surat. The traders of that time were a combination of merchants and tramp shippers: the ships which they owned travelled between a number of ports, picked up the specialities of the ports and unloaded them where they were valued most. The English, French and Dutch East India Companies that traded in India at that time did a very different kind of business: they were interested primarily in taking a small range of commodities, especially textiles, to Europe, where they fetched very high prices. The range of European goods they could sell at a profit in India was limited, and they paid for the wares in bullion mainly. Thus, they were interested only to a limited extent in building up a marketing organisation in India; and since they earned a handsome profit just by transporting goods from India to Europe, they were interested only to a limited extent in manufacturing and buying goods cheaply in India. This lack of interest, as well perhaps as unfamiliarity with languages and customs, led the Companies to rely on locally based brokers, agents, and shippers, both Indian and European, in each port.[15] On the other hand, local traders came to rely on the Companies for protection on the sea. By 1750, Parsi traders were entirely dependent on the British and the Dutch for protection on the seas.[16]

The Parsis in Surat also acted as agents for the Companies.[17] The ancestors of the Frenchman family acted as agents for French and Dutch traders.[18] The Modis (literally, grocers) were originally priests, and got their new surname when one of them became the

14. H. Furber, *op. cit.*, pp. 167-68. W. H. Moreland, *From Akbar to Aurangzeb*, Cambridge 1929, pp. 153-55.

15. For an excellent account of the state of the companies' trade, see H. Furber and W. H. Moreland, *op. cit.*

16. Holden Furber, *Bombay Presidency in the Mid-Eighteenth Century*, Bombay, 1965, p. 64.

17. D. F. Karaka, *op. cit.*, Vol. 2, p. 322.

18. H. D. Darukhanawala. *op. cit.*, p. 322.

house steward and general agent of the East India Factory at Surat.[19] Another Parsi, Rustam Manak Seth (1635-1721), was broker, banker and money-changer to the East India Company, and travelled to Delhi in 1660 to make representations for the Company against the Nawab of Surat.[20]

Hence when the East India Company got political power, the Parsis came to share in it. Before Bombay was ceded to the British in 1668, it was ruled by the Portuguese. They employed Dorabi Nanabhai as their broker. The British, on coming into possession of the island, continued to use his services. They imposed a poll-tax on the population, and entrusted Dorabji with its collection. Thus he became the first Patel (tax-farmer) of Bombay. His son Rustomji (1667-1763) raised a force to defend Bombay against the Sidis of Janjira in 1692. Rustomji's son Kavasji became Patel of Thana and Bassein, Portuguese outposts near Bombay, when the British took them in 1774. He was also a shipper; and his son Hirji had an extensive China trade. His brother Dorabji supplied cotton, timber, saltpetre etc. to the Company government.[21] In Surat, in the early 19th century, two brothers who were descendants of Beheremandkhan entered the service of the Company, and one of them, Ardeshir Dhanjishaw Kotwal Bahadur, was made the warden (*Sadar Amin*) of Surat castle and town while the other, Pherozsha, became political agent of the East India Government in various districts of Gujarat.[22]

When the Maratha empire succumbed to the Company in 1802, the latter was as unprepared to administer the new territory as in Bengal and the North-west Provinces earlier; as in those areas therefore the Company farmed out the tax revenues of the empire. Two Parsi trader brothers, Pestonji and Vikaji Merji, were made farmers of the land and sea customs of Konkan and the land customs of Poona, Ahmednagar and part of Khandesh. They doubled the revenues in seven years. They also engaged in the collection of cotton for export, and later extended their cotton

19. *Idem.*, pp. 368-69.

20. D. F. Karaka, *op. cit.*, Vol. 2, pp. 9-16.

21. H. D. Darukhanawala, *op. cit.*, p. 497.

22. D. F. Karaka, *op. cit.*, pp. 49-54.

trading to the state of Hyderabad. They made advances to the Nizam in return for privileges of tax-farming and currency issue. Ultimately their firm failed in 1851 owing to the refusal of the Nizam to repay certain loans.[23]

Traders, brokers, shippers, shipbuilders, financiers, industrialists, tax farmers—which of these people went into industry? Curiously, the pioneers in the Bombay textile industry came almost entirely from among the Parsi traders, shippers and financiers of Bombay. Cowasji Nanabhoy Daver, the founder of the Bombay Spinning and Weaving Company, the first cotton textile mill in India, was the son of a rich merchant who had agencies of a number of English firms. Cowasji himself was active in cotton export and in finance. He started three banks (Commercial Bank in 1845, Mercantile Bank in 1853 and Broker's Loan, Discount and Banking Company in 1861), and installed the first hydraulic press in Bombay in 1853.[24] Jeejeebhoy Dadabhoy (1786-1849), the father of Byramjee Jeejeebhoy who started a weaving mill at Kurla, a suburb of Bombay, was a broker, director of banks, shipowner and financier of plantations in Malabar, Ceylon and Java, and traded with Europe, Egypt and the East.[25] Mancherji Nowrojee Banaji (1837-1890), founder of the City of Bombay Mill in 1885, was the descendant of a merchant who migrated to Bombay in 1690,[26] and was himself engaged in trade, shipping, real estate and company promotion.[27]

Three families were the most active in the textile industry—Wadia, Petit and Tata. The Wadias were descended from Lowjee Wadia, the master builder of Mazagaon Dock, and made fortunes in shipping, trade and finance.[28] Of them, Nowrojee Nusserwanji Wadia (1884-1899) was the owner of the Textile Mills, Bombay Dyeing, and Century Mills and built over ten other mills.[29] Manakji Petit, the founder of Oriental Spinning and Weaving Mills, was a

23. *Idem.*, Vol. 2, pp. 124-29.

24. R. F. Wachha, *op. cit.*, Vol. 1, pp. 328-29.

25. D. F. Karaka, *op. cit.*, Vol. 2, p. 133.

26. *Idem.*, Vol. 2, p. 54.

27. *Bombay Gazette*, 27 October 1890.

28. R. A. Wadia, *op. cit.*, pp. 1-72.

29. Mancherji Hoshangji Jagosh, *Seth Nowrosji Nusserwanji Wahadia* (in Gujarati), Bombay 1901, pp. 34-38.

merchant; and his son Dinsha, who was the chief shareholder of six mills and the richest Parsi of his time, traded with China.[30] Nusserwanji Rattanji Tata, founder of Central India Mills at Nagpur, was a shipper and China trader; he exported cotton and opium and imported tea, silk goods, camphor, cinnamon, copper, brass and gold. He made large profits on contracts with the Commissariat during the occupation of Bushire in 1857 and on export of cotton during the American Civil War.[31] Later his son Jamshedji started three textile mills as well as the steel plant in Jamshedpur.[32]

The direct investments in the textile industry were not the only Parsi contribution; Parsis were often secretaries, managing agents or managers of mills started by non-Parsis. Muncherji Nowrojee Banaji, mentioned above, was secretary to the Great Eastern Spinning and Weaving Mills;[33] Dorabji Framji Panday (1828-1890), who began exporting cotton during the American Civil War, later became the agent of Dharamsi and Coorla Mills.[34]. The Parsi contribution to the management of the textile industry in the nineteenth century was perhaps as great as the contribution to investment.

While the Parsi contribution to the foundation of textile mills is outstanding, they were pioneers in many other fields. Nowrojee Wadia started a paper factory around 1870, but had to close it down for lack of capital.[35] Jeejeebhoy Dadabhoy introduced steam navigation to India in 1841, and set up a highly profitable coastal shipping concern.[36] Jamsetji Dorabji Naegaumwalla (1804-1882), who began as a shipwright in the Wadia dockyard, was the first Indian railway builder, and constructed nearly 80 miles of railways around Bombay in the 1850s. Earlier, he also constructed salt-

30. D. F. Karaka, *op cit.*, Vol. 2, pp. 134-36; H. D. Darukhanawala, *op. cit.*, p. 423.

31. Bahamanji Behramji Patel, *Parsi Martyuk* (in Gujarati), Bombay 1915, part 3, pp. 142-43.

32. F. R. Harris, *Jamshedji Nusserwanji Tata. A chronicle of his life*, Bombay 1958, pp. 5-7.

33. *Bombay Gazette*, 27 October 1890.

34. *Jam-e-Jamshed*, 4 June 1890.

35. *The Times of India*, 21 December 1899.

36. D. F. Karaka, *op. cit.*, Vol. 2, pp. 132-33; H. D. Darukhanawala, *op. cit.*, pp. 340-41.

works for Dadabhoy Wadia.[37]

Among the pioneers in other fields mentioned above were a Wadia and another connected with the Wadias. Wherever we look among pioneers, they are found to come from or be related to a small circle of shippers, shipbuilders, traders and financiers in Bombay; the landlords and manufacturers of Surat-Navsari do not figure among them. This phenomenon calls for an explanation.

The roots of this phenomenon are in the eighteenth century. The British obtained Bombay in 1668. In the entire eighteenth century, Bombay was the major British possession in western India; and since it was in their possession, they were keen on developing it into a port. That was why they took Lowjee Wadia to set up a dockyard. At the same time, the Mogul empire had declined, and the Surat-Navsari area was in the intermittent control of the Mogul representatives and the Marathas. It was therefore subject to political uncertainty and occasional raids; it was a bad place for men of means. Among them, however, those whose wealth depended on trade with the interior stuck on; those whose dealings were mainly abroad moved gradually to Bombay, and Parsi shipper-traders were heavily represented among them. The latter had not only more to gain from the move, they also had a community of interests with the British who too were maritime traders.

Three radical changes occurred in the nineteenth century. First, the British achieved a technological break-through in cotton textiles. Second, they pioneered revolutionary improvements in land and sea transport. Third, as a result of technological and military supremacy the British began to force their way into Asian markets.

The Bombay Parsis were the first in India to feel and to share in these changes. They caught each British fashion as it came up: they went in for women's education, for racing, and finally for baronetcy-bound philanthropy.

When the British forced their way into Western India and China, Parsis went with them. We have described how Vikaji and Pestonji became tax farmers in Maharashtra and promoted cotton export. Parsis were involved in the export of opium to China in the eighteenth century; they greatly expanded their business, and benefited when

37. B. B. Patel, *op. cit.*, part 3, pp. 33-37.

the British opened up China with the opium war.[38] Many Parsi merchants were imprisoned by the Chinese government during the opium war, including Jamshedji Rustomji Irani, Dadabhai Rustomji Banaji, Hirjibhai Rustomji Patel, Nusserwanji Bomanji Mody, Ardeshir Furdoonji Panday, etc. — representatives of many of the Parsi families which were later involved in textiles.[39]

Thus, the Parsis, owing to their tradition of maritime trading and their close connexions with the British, shared in the profits of international trade. In the early nineteenth century these profits grew enormously because they were based on the extensive exploitation of large international markets; they rose far above the profits from intensive exploitation of courts and upper-class markets. This change in relative profits also led to the rise of Bombay Parsis in relation to Parsis and other merchants of Surat.

Further, their position earned the Bombay Parsis a lion's share in the export of cotton to Britain and the import of British textiles; and they were among the first to realize the profits that would result from manufacturing cotton textiles in Bombay. A place in western India was obviously a far more economical location than Lancashire for spinning and weaving Indian cotton for the Indian market, and once the import of textiles from Lancashire was well established, it was only a matter of time before import substitution would take place.

The lucrative opportunities of import substitution were realized early by the British also, who carried the trade in cotton and textiles between Britain and India, and they invested heavily in the Bombay cotton textile industry between 1850 and 1880. They continued to play an important technical and managerial role right up to the First World War.[40] But they could not trade and manufacture without intermediaries because of their linguistic disability; their

38. While the East India Company had a monopoly of export of opium from Eastern India, this only meant that "a favoured few of the company's servants arranged opium contracts to their best advantage" (Holden Furber, *John Company at Work*, p. 165). The export of opium from the Native States of Malwa developed independently through Bombay and Daman; the East India Company charged a duty on the exports that passed through British territory but had no monopoly of them.

39. H. D. Darukhanawala, *op. cit.*, p. 209.

40. S. D. Mehta, *The Indian cotton textile industry. An economic analysis.* Bombay, 1953, pp. 2, 57.

*Trends and Tendencies in Indian Economic History**

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> Here is Edward Bear, coming downstairs now, bump, bump, bump, on the back of his head, behind Christopher Robin. It is, as far as he knows, the only way of coming downstairs, but sometimes he feels that there really is another way, if only he could stop bumping for a moment and think of it. And then he feels that perhaps there isn't.
>
> A. A. MILNE, *Winnie-the-Pooh*

I

A scholar is pleased to have his writing taken seriously, and I confess that I am flattered by the detailed attention my essay received from such distinguished historians as Toru Matsui, Bipan Chandra and Professor Tapan Raychaudhuri.[1] Hopefully, their comments and this response will suggest new paths of research and will contribute added vitality to a field that requires broader and

*I must express my gratitude to a great many people and institutions for help in preparing this essay. More than the normal number of students and scholars reacted to my efforts to make my ideas explicit. I must particularly mention Dr. Ian Catenach, Dr. K. N. Chaudhuri, Professor Ashin Das Gupta, Dr. Biplab Dasgupta, Dr. T. J. Byres, and Professor D. A. Low and the members of his seminar at Sussex University, all of whom proved to me once again that scholarly interaction transcends national boundaries. I must also thank Professor Low of Sussex University, Professor W. H. Morris-Jones of the Institute of Commonwealth Studies, London University, and the Librarian and staff of the India Office Library for generously providing physical aid and comfort. Professor D. C. North, Professor George Taylor and Dr. Lyman Legters of the University of Washington, and the American Council of Learned Societies furnished the financial assistance which made my reading, thinking, talking and writing possible and pleasant.

1. *The Indian Economic and Social History Review*, Vol. V, No. 1, March 1968, pp. 1-100. When referring to articles in this issue of *IESHR*, I shall use the author's initials (if necessary) and the appropriate pagination. References to my original essay will be to the *IESHR* reprint. All references to India or South Asia are intended to relate to the whole of what are now India and Pakistan.

Parsi agents and brokers filled this intermediary role.

The early dominance of the British and the Parsis disappeared with time. Cotton was an agricultural commodity liable to wide price fluctuations; hence merchants and their capital were attracted from all over western India into cotton trading. Gradually this trading capital filtered into textile manufacture, and non-Parsi Indians increased their participation in the industry: the profit rates fell, and investment became unattractive.

At the same time, the Parsis ran short of investment opportunities. In the late nineteenth century, the momentum of foreign trade shifted to eastern India, where plantations of jute, tea and indigo were developed and manganese and mica mines were opened. In western India, opium exports gradually declined, as did the exports of cotton which was increasingly absorbed by home consumption.

With the stagnation of trade, the demand for ships also fell off. Shipbuilding, which was virtually a Parsi monopoly in India in the eighteenth century, died a slow death. The size of sailing ships increased in the eighteenth and nineteenth centuries, and the shallow port of Surat went out of use for shipbuilding. The Wadias continued to build ships in Bombay; but the British closed down the ship-building activities of Mazagaon Dock in 1885, just when the steamship revolution was beginning.[41] Indian ship-building never recovered from the decline.

In a country without an independent capital goods industry, two kinds of industries are at a disadvantage relatively to their foreign competitors: capital intensive industries whose costs are raised by the high import cost of equipment, and industries producing high-value commodities which incur very low transport costs. Hence India was bound to develop industries with low capital-intensity, such as oil milling, rice milling and sugar, and industries with bulky products such as bricks and cement. These were also industries in which material and transport costs were predominant. Hence they would grow up close to centres of material supply or markets. In fact, they are dispersed all over India so as to minimise transport

41. H. D. Darukhanawala, *op. cit.*, pp. 494-95.

requirements. Cotton textiles were an exception; while their capital costs were low, their transport costs were also low. Hence while their production in India rose, it was neither dispersed all over the country nor concentrated in cotton-growing areas of the country.

The Parsis, who were concentrated in Surat, Navsari and Bombay had many advantages in the setting up of the cotton textile industry. But their very concentration prevented them from entering the other industries in which India had an advantage and which grew rapidly after 1900. Parsi capital did not enter any new industry except cement. Perhaps there was also a hiatus of scale: after going into cotton textiles, the Parsis would not have thought of going into, say, oil milling, whose capital requirements were tiny compared to textiles.

To conclude, the origins of Parsi enterprise of the nineteenth century are not to be found in their social or psychological characteristics. Being a Parsi helped in emergence as an entrepreneur; but only because a Parsi trader was neither confined to internal trade nor suffered from the depredations of authorities like the Hindu trader, and because he did not have to share the defeat of the Muslim trader by the British East India Company in the eighteenth century struggle for the mastery of the seas. Nor was it enough to be a Parsi; the rich Parsis of Surat declined together with their Hindu and Muslim neighbours. It was the Bombay Parsis that were responsible for the industrial upsurge, and their achievement cannot be divorced from their British auspices and the drastic changes in political power and trade flows in the eighteenth and nineteenth centuries.

more varied investigation than it has so far got. In order that our discussion produces the widest possible benefits, and in order to avoid a debating match in which we merely attempt to score points off one another, I ought to explain that my original paper, being intended for a somewhat special audience, had rather limited objectives. The circumstances account for some of the specific limitations that were consciously built into the article.

The essay, which was presented at an annual meeting of the Economic History Association of the United States, was addressed to historians and economists who were interested in economic growth and change. Not only was I addressing myself to an audience that had little knowledge of South Asia, but I was aware also that virtually all their research had drawn on European and American (and possibly Japanese) experience and therefore they tended to assume that economic growth is inevitable. When they found it necessary to refer to the apparent failure of other societies to develop, they merely drew on the most accessible general studies of these social systems. They neither examined the basic data nor carried out their own independent investigations. They accepted on faith certain simplistic conclusions about the characteristics of these "traditional" societies. Given their assumption of inevitable economic growth, they explained in rather mechanical ways that the apparent failure to develop was the result of imperialist policy or of general cultural and ideological characteristics.

Having worked on the economic history of Europe, the United States and East Asia as well as the Indian subcontinent, I have been impressed by the complexity of the issues at stake. My hope was to sensitize my audience to the fact that uncritically accepted notions about the careers of "traditional" socio-economic systems needed re-examination. It was in this context that I presented my *tour d'horizon* of Indian economic history. Using the Indian case, I attempted to show that without sufficient solid historical research the available evidence was susceptible to interpretations other than the ones Western scholars tended to accept. Inasmuch as my space was limited, it seemed to be easier and more symbolic of our lack of knowledge to stress the "growth" features, the movement of *per capita* income over time.

I think it is obvious that I was quite explicit about the tentativeness of my judgements. However, lack of space forced me to be less qualified in my statements than I desired, and because of this restriction I was not able to consider the issues of structural change or lack of structural change. But, since my own interests are pre-eminently structural, I felt called upon to remind my audience of the need to study the structural characteristics and changes that, although fundamental to understanding the behavior of a system, tend to be ignored by purely quantitative analysis at the *per capita* or national income level. Yet if I could show that even within the context of an "economic growth" analysis it was possible for the evidence to be interpreted in a variety of ways, I would have made the case that our knowledge of Indian economic history is not complete and that scholarly investment in new detailed research could yield a high rate of return. I hoped that by advancing my view in a rather provocative way students and scholars in the United States would be encouraged to study this fascinating field.[2]

Also, I had another objective, this one affecting contemporary policy. Economic planners and foreign aid advisers always operate, consciously or unconsciously, with certain historical assumptions. If it turns out that the historical record has been misread and the historical parameters are different than the policy makers assumed when they laid their plans, then significantly different policy judgements might be called for.[3] Certainly, I would not claim that one

2. Let my basic position be very clear at the outset. I feel that we know very little about modern Indian economic history and that the little evidence we have permits us to put forward a variety of alternative interpretations on practically every major issue. In this sense my original essay should be seen as a tentative but not unreasonable alternative to the Classical interpretation. The arguments that I offer in this essay are equally tentative.

With regard to the reasonableness of my view about the course of *per capita* income, it should be noted that Professor Mukerji, writing after me and with much more precise statistical analysis, came to virtually the same conclusion as I. See M. Mukerji, "National Income", *Economic History of India : 1857-1956*. Edited by V. B. Singh. (Bombay, 1965), particularly pp. 701-703.

3. Three examples from recent Indian experience come immediately to mind. Indian planners initially assumed a much lower rate of population growth over the long run than has occurred This followed from a rather simplistic view of demographic behavior over time. Secondly, the planners underestimated the aggressiveness of private enterprise, thus contributing to the foreign exchange difficulties of the second Five Year Plan. This flowed from certain assumptions about the character of Indian entrepreneurship after 1800. (See M. D. Morris, "Values as an Obstacle to Economic Growth in South Asia : An Historial Survey," *Journal of Economic History*, Vol. XXVII, No. 4, December 1967, pp. 588-607.) Third, much Indian labour policy has been based, I believe, on misconceived historical assumptions about the behavior of the labour force and the difficulties of recruiting an industrial

could draw alternative lines of modern economic policy directly from arguments advanced in my essay. But one major conclusion can be derived from the central theme that in the Mughal period India had neither the level of *per capita* output nor the structure of economic organization that was necessary to make rapid economic development a realistic possibility in the 19th and early 20th centuries. It follows that the creation of a structure appropriate for rapid growth in our own time is likely to be both complex and troublesome. This theme is not an argument for passive acceptance; rather it suggests that we can remain confident in the face of grave difficulties. I see my original essay as properly leading to a relatively assured and optimistic view of growth possibilities inherent in the Indian economy at the present time.[4]

Having explained the specific intent of my original essay, it should be clear that there are some things I did not do because I did not intend to do them. A recognition of this will automatically eliminate some potential points of disagreement between myself and my commentators. For example, I was not interested in discussing the historical development of ideas about the performance of the Indian economy. Therefore, it is irrelevant that long before me others said similar things.[5] It is only impor-

proletariat in India. (See M. D. Morris, *The Emergence of an Industrial Labour force in India : A Study of the Bombay Cotton Mills, 1854-1947* [Berkeley and Bombay, 1965.])

4. The Classical analysis, as it has typically been advanced, does not seem to shed any useful light on India's contemporary predicament. To the extent that it attributed the slowness of economic growth after 1800 entirely (or primarily) to British policy, it generated the unrealistic expectation that with the end of imperialism problems would become easy and economic growth would be rapid. When these anticipations were not borne out, an unwarranted mood of dispair overtook the nation's intellectuals. On the other hand, my "gestation theory," as Professor Raychaudhuri has called it, because it stresses the complexity of the development process, would lead to more realistic expectations and to less disillusionment. Thus, I would not yield to the pessimism that prevades Gunnar Myrdal's recent study, *Asian Drama ; An Enquiry into the Poverty of Nations* (New York, 1968). Similarly, I am not swayed by the overwhelming enthusiasm that has recently greeted the agricultural successes that have followed on the introduction of new seed strains. Such innovations are significant, but my analysis does not permit me to interpret one such gain as an agricultural revolution or one technical achievement as a development "breakthrough."

5. Toru Matsui and Tapan Raychaudhuri recognize this, but Bipan Chandra missed the point with his elaborate discussion of the late 19th and early 20th centuries ideological debates and his complaint that I failed to acknowledge the existence of earlier somewhat similar conclusions. To this extent, all of Part I of his essay (pp. 35-43) is entirely irrelevant.

tant that I have termed "Classical" certain current widely held views about the actual career of the Indian economy and that essentially I have not misstated them. If the dominant interpretation as I briefly outlined it (pp. 2-3) is correct, then there is no need to debate its intellectual origin and we can proceed to the fundamental issues.[6]

The difficulty is that in one way or another my commentators and I have raised or suggested virtually every general methodological and substantive issue confronting economic historians of India. I am therefore somewhat baffled to know how or where to begin or how many of these matters to deal with in an essay. Moreover, the information that is available at present is insufficient for scholars to be definitive about any substantive issues. Thus, it is only appropriate that I try to be suggestive rather than elaborately detailed. I will start with some major methodological matters that have been raised and then I will turn to the more general substantive points of disagreement.

II

The first matter relates to the applicability of economic theory to historical analysis.[7] It is widely recognized that we are not as yet close to a theory of economic development that incorporates significant relationships of economic growth (quantitative) and economic change (structural) into an integrated and interrelated system of behavior. In fact, given the variety of regions and conditions with which we have to deal, I am not sanguine about the possibility of developing a general theory that will be really useful. But lack of

6. For a slightly more extended description of the Classical interpretation see M. D. Morris, "Economic Change and Agriculture in Nineteenth Century India," *IESHR*, Vol. III, No. 2, June 1966, pp. 187 ff.

7. This is an issue about which there has been very lively debate in recent years. The most recent items that have come to my attention are R. W. Fogel, "The Specification Problem in Economic History," *Journal of Economic History*, Vol. XXVII, No. 3, September 1967, pp. 283-308 ; and M. Desai, "Some Issues in Econometric History" *Economic History Review*, 2nd Series, Vol. XXI, No. 1, April 1968, pp. 1-16. The literature of this controversy can be traced back through the references in these two articles. One other article might be specifically mentioned : D. C. North, "The State of Economic History," *American Economic Review*, Vol. LV, No. 2, May 1965, pp. 86-91.

a total formulation does not prevent us from employing the existing theoretical tools for various bits of partial analysis. Economic theory has developed a set of propositions from which a variety of results (under certain specified conditions) can be derived. In other words, economists possess a number of formal techniques with which they can examine the consequences of specific types of behavior. As I know full well, our ability to apply these techniques to the solutions of large-scale historical problems is severely limited. We economists tend to impound in *ceteris parabus* a host of factors that appropriately cannot be held constant over any length of time. Although economic analysis has become increasingly sophisticated, its application to problems of structural change remains circumscribed. But, despite these acknowledged weaknesses, economic theory as a system of analysis can be used by economic historians for two general purposes, to identify and specify problems and to test the conclusions and implications of historical arguments. I can illustrate some of the possibilities from my own work on the creation of an industrial labour force.[8]

The standard interpretation argued that Indian industrial development before 1947 was handicapped by a shortage and instability of labour. Ignoring the subtleties, this hypothesis implies that industry would have grown more rapidly if the supply of labour had been large.[9] Because the proposition is best tested by a rather narrowly defined and carefully limited industry study, I examined the issue by doing a detailed study of the Bombay cotton textile industry. In a sense, I was drawn to the problem by S. D. Mehta's statement that wages in the industry remained "remarkably stable" until 1914.[10] The proposition that there was wage stability in the face of labour shortage struck me as being inconsistent with what economic theory would anticipate initially. Given an industry that was very profitable, a fact suggested not only by the available profit data but also by the rate of expansion in the industry, and given a situation

8. What follows is discussed in detail in Morris, *Emergence of an Industrial Labour Force*.

9. This view is at odds with the general Classical interpretation that attributes the problem of limited economic growth in the industrial sector largely to imperialist policy.

10. S. D. Mehta, *The Indian Cotton Textile Industry: An Economic analysis* (Bombay, 1953), p. 29.

in which wage costs constituted a relatively small proportion of total costs, one would expect a steady upward pressure on wage rates as employers tried to draw more workers from the countryside or to tempt them away from employers.[11] In other words, the application of elementary supply-demand analysis would initially lead us to raise questions about the traditional view. One could, of course, argue that Bombay textile magnates were so bound by custom and traditional ideas about the appropriate wage that they were unresponsive to possibilities of increasing their supply of labour, their output and their profits through marginal increases in wages. No one has ever suggested that Bombay entrepreneurs were insensitive to opportunities for profit maximization ; nevertheless I would not consider their acquisitive reputation an adequate basis for refuting a major historical hypothesis. My careful examination of the data indicated that with the one exception of the plague period 1896-1898 there was no solid evidence of labour shortage. And during this epidemic period when it is clear that labour supply declined actually while demand for the product remained high, wages did rise very sharply. They subsequently fell when the menace of death dwindled and the labour supply eased. In other words, there appears to be no paradox. The fact that wages in general did not rise reflects what other historical evidence confirms, there were no limitations of raw labour supply to effect the growth of the Bombay industry.[12]

Furthermore, the development of the industry's technological and organizational structure butresses the theme of adequate labour supply. Contrary to the general view, the industry showed considerable innovative capacity. If labour shortage held back otherwise profitable expansion, there ought to be evidence of a gradual introduction of more capital-intensive equipment patterns. But in fact the mills tended to favour—as one would have predicted if labour were plentiful—labour-intensive technology.[13]

11. My point is that because wages were a small percentage of total costs, even a sharp rise in wages would have increased total costs by a very small amount.

12. Actually, wage behavior was far more complex than I suggest. The changing structure of wages among occupations and between mills also reflected in a microscopic way the working of supply and demand forces.

13. This aspect of the interpretation is supported by the fact that as labour costs in Bombay rose vis-a-vis up-country enterprises, the pattern of labour and capital utilization in Bombay also changed in ways consistent with theoretical expectations.

One can also use the same type of analysis to explain certain otherwise puzzling features of mill organization. The paradoxical persistence of the institution of the jobber makes sense when we recognize that the cost of managerial services was extremely high relative to work force costs. It was economically reasonable to minimize management expense wherever possible. The same cost-benefit calculations can be used to throw light on the otherwise inexplicable reluctance of employers to move to double-shift operations.

There is no need to go on with this illustration. I only used the example to show how the application of elementary economic principles initially produced discontent with a widely accepted explanation, then how it suggested a set of questions that could be used to test the hypothesis and how it made possible alternative explanations that appear to be highly acceptable.

I suppress the temptation to indicate ways of employing economic theory that would improve the analysis and understanding of other important historical problems. However, I must mention the useful essay by Dr. K. N. Chaudhuri that offers new views of the role and impact of foreign trade on the Indian economy. Briefly but efficiently he examined the major problems that have exercised scholars—the problem of competitive imports, the theory of "The Drain" and the relation of foreign trade to internal monetary behaviour. His article shows how far we must push theoretical clarification before we can eliminate the extensive confusion that now exists on these topics.[14]

The use of economic theory as a more efficient tool for posing questions and seeking answers does not necessarily lead one to a specific view of the career of the Indian economy. To put the matter bluntly, these techniques make possible a more orderly and more rational setting of questions and testing of hypotheses, and this does not automatically commit the user to an "imperialist" or "anti-imperialist" interpretation of what happened in the two hundred years before Independence. I think it is true that in the past neo-classical economic theory has had a strong presumption in favour of *laissez faire* solutions. It should be pointed out, however,

14. K. N. Chaudhuri, "India's International Economy in the Nineteenth Century: An Historical Survey," *Modern Asian Studies*, Vol. II, No. 1, 1968, pp. 31-50.

that both Marx and Keynes worked within the assumption of a perfectly competitive model and each emerged with conclusions that were quite at odds with neo-classical orthodoxy. Indeed, the use of simple economic theory in analysing historical problems need not bias the answers to the questions we set. For example, my study of industrial labour posed rather straightforward questions: was there a shortage, of raw labour and was industrial expansion inhibited by it? Using elements of economic theory and reexamining the available evidence, the study indicated that a negative conclusion was warranted. It does not seem to me that my application of certain very elementary tools of economic analysis biased the conclusion.[15] My essay on the 19th century which is the subject of the current discussion also set out to ask a rather simple question: What happened to *per capita* income over the period 1800-1947? My only interest was to show that on the bases of the evidence and through the use of specific kinds of analysis familiar to economists it was more than likely that *per capita* income rose.

I must clarify something about the nature of my essay and the implications that can be drawn from it. I did not start out to prove that British rule was good for India nor, for that matter, did I come to that conclusion. Whatever ultimately emerges about the actual trend of *per capita* income, the conclusion in itself will not generate a necessary and inevitable judgement about the benefits and burdens of British rule. It merely will help to define more precisely the factual framework within which that very complex issue can be tackled.

In these terms, a scholar's ideological bias, surprisingly enough, will be revealed more likely in the questions he asks rather than in answers he gives. A political viewpoint may influence the selection of questions by defining those that are considered particularly important. But once specific questions have been set, professional scholars should be able to agree upon which tests will be needed and which evidence will fulfill the conditions of required proof. I am certainly not implying that the answers to specific historical

15. I must point out that in the absence of a wide range of supporting studies, there are many propositions in my book that must still be viewed as only approximation to the truth. However, even Dr. Bipan Chandra (p. 61) seems to have been unable to find fault with my basic analysis or with my conclusions.

questions are easy. I am suggesting only that much disagreement in Indian historiography has arisen because scholars have frequently been neither precise in their questions nor systematic in their proofs. I feel that work on Indian economic history is in many cases weak because of a failure to ask specific "economic" questions and a reluctance to use economic theory in testing answers that have specific economic relations inbedded in them. I will attempt to be more explicit about this at a later point.

Viewed in the way I have suggested, my conclusion that *per capita* income probably tended to rise between 1800 and 1947 generate two different types of problems. First, there is the question of fact: and, second, there is the issue of the relevancy of the question. With regard to the factual issues, much of the disagreement hinges on my analysis of the possibilities that the very limited evidence we have in agriculture, handicrafts, rural non-agricultural activities, industry and commerce suggests. Basically, differences arise from my judgement that the level of *per capita* economic activity was relatively low during the Mughal era. Objections about the trend of *per capita* output--ignoring for the moment the difficulties of measurement raised by Toru Matsui and Tapan Raychaudhuri—can be handled within the framework of my questions. When reasonably solid research on the course of output in the individual sectors of the economy becomes available, the arguments over general tendencies and trends in the economy could be substantially reduced.

But the sharpest disagreement with me stems from the feeling that the question with which I was mainly concerned—what were the quantitative rates of growth ?—was not the proper question to ask. All three essayists, in one way or another, suggest that the appropriate issue is why India developed so slowly during British rule, or, really a different question, why there was no industrial revolution after 1800. I, of course, did ultimately touch on these issues (pp. 11-13) but with ideas that certainly did not find great favour.

At this point, I fear, we enter a thicket of hidden or only partially explicit assumptions that need detailed sorting out before specific operational propositions can be postulated. To formulate the question as my commentators presented it to me seems to beg the question I raised. Moreover, their query automatically assumes that on balance British policy did retard Indian economic development, and as typically stated, it implies that under any and all alternative sets of circumstances economic development would have proceeded more rapidly quantitatively and/or structurally. But before such an idea can be accepted it is necessary to specify the initial conditions so the "counter-factual proposition" can be properly tested. I can think of a number of alternative (and essentially contradictory) propositions that we can examine.[16] Let me explore the implications of two different possibilities of Indian economic development, one assuming no British rule and the other assuming the British departed during the second half of the 19th century.[17]

The first possibility assumes that neither the British nor any other European power ever gained political control on the sub-continent. (This case is suggested by Professor Raychaudhuri, p. 87). Accepting this, we would have to establish an additional set of subsidiary political assumptions within the context of a decaying Mughal authority. (Raychaudhuri implies a congeries of independent chieftainships.) I will deal with the character of the Mughal period later. Here I would only say that if one accepts Irfan Habib's description of the Mughal system in the 17th and first half of the 18th centuries, it would be difficult to assume that significant economic growth after 1707 was possible in any region of the land. He concludes that not only was the Mughal Empire "its own grave

16. Leaving aside for the time being the matter of defining an industrial revolution, a nation could have a rather rapid long-run rate of quantitative growth without changing its economic structure very dramatically. In other words, there need not necessarily be a profound structural shift in the direction of a factory system and heavy industrial-type output in order to achieve substantial rises in *per capita* well-being over time. On this theoretical point Professor Raychaudhuri might well agree while still arguing, as he does, that it would not be appropriate to expect this in the Indian instance.

Dr. Bipan Chandra, however, confuses the issue in the way he states the case. He tells us (p. 38) that the main concern of the Classical tradition is "not that of *per capita* income or destruction of handicrafts but of economic development." By economic development he seems to mean some undefined notion of "Industrialization," structural changes without regard to the effect, negative or positive, on *per capita* income. (pp. 38-40). If this is his view, he has not addressed himself to the problem with which I was primarily concerned.

17. I do not prented that I will fully explore these propositions, but it does seem worthwhile to examine some of the possibilities in order to illustrate my point that much of the discussion of Indian economic history is handicapped by a lack of clarity in the initial propositions and the failure to specify explicitly which assumptions are being accepted. Let me state in advance that I do not consider that what follows is a useful way of proceeding to work on Indian economic history at this time. I engage in the exercise only to illustrate the unsatisfactory nature of the assumptions that colour a great deal of existing work.

digger" but "No new order was, or could be, created by the forces ranged against it."[18]

However, let me assume away this problem. Let me accept the possibility that India in 1750 could somehow have imposed a protective tariff against all imports.[19] Would this have set the continent or any significant part of it on its way to rapid economic growth and structural change? Whatever else might be said, the fact is that textile exports, "the overwhelming bulk of India's exports" (TR, p. 85), would have declined after 1750 even if no European nation had established its political power in India. The growth of more effective competition from British and Western European cotton textile industries after 1750 would have sharply curtailed the export of Indian fabrics regardless of India's political status.[20]

After 1813, the economic pressures on handloom weavers would have been reduced because our assumed tariff protection would have prevented any import of Manchester cloth. At the same time, hand spinners would have been protected against any imports of machine yarn. As a result of the absence of both foreign cloth competition

18. Irfan Habib, *The Agrarian System of Mughal India* (Bombay, 1963), p. 351. (Henceforth cited as *Habib.*) This view that the social forces operating in Indian society were creatively weak is also suggested by A. I. Levkovsky, *Capitalism in India: Basic Trends in its Development* (Bombay, 1966), pp. 2-4. Our two types of analysis are quite different, but Levkovsky seems to agree that India's level of structure and performance in the 17th-18th centuries resembled Western Europe four or five centuries earlier.

19. If we accept the likelihood of India's regional fragmentation following the collapse of Mughal power, we must then investigate the possibilities of regional tariffs and their consequences. In this context Professor Ashin Das Gupta has shown me how trade even between two principal Mughal *subhas* like Gujarat and Bengal had become difficult by the 1740's, not to speak of the greater difficulties of trade between Maratha, Mughal and English ports. I am simplifying the argument by assuming only a prohibitive all-India tariff barrier and no internal inhibitions except those imposed by transport costs.

20. There is no evidence that during the Mughal period the Indian handicraft system threw up any significant cost-reducing technological innovations. Nor is there reason to expect technological improvements in the decades after 1750. (For support on this point, see S. Bhattacharyya, "Cultural and Social Constraints on Technological Innovation and Economic Development: Some Case Studies," *IESHR*, Vol. III, No. 3, September, 1966, pp. 240-267.) In fact, for reasons that can be traced out, cost-reducing technological or organizational innovations are unlikely in an environment in which total demand was falling. Thus, in a situation where Indian costs were not declining, not only would the major Western European markets have vanished but neutral markets would also have been lost to the technologically innovating and cost-reducing European entrepreneurs. In other words, what happened to Indian foreign trade in textiles (and thus to most of its traditional foreign trade) between 1750 and 1813 would have happened under any circumstances.

and cheap Manchester yarn imports, internal cloth prices would necessarily have been higher. We must assume that these higher domestic prices would have resulted, *ceteris parabus*, in a much smaller internal demand for textile products than actually was the case in the 19th century. Although it is not easy to express the intensity of the consequences, it is probable that a combination of vanishing markets abroad and higher textile prices at home would have produced in the first half of the 19th century a stagnant or, more likely, a somewhat smaller handloom sector than existed in the previous century.

Because cotton textile exports, "the overwhelming bulk" of all exports, fell sharply, foreign trade could have played only an insignificant role in 19th century economic development unless and until other exports developed. What could there have been? Once this question is asked, it becomes reasonable to assume that new exports entering the international trade network would probably be the same type as actually were exported during the 19th century.

I need only remind the reader that a nation's exports depend on the general structure of international demand and on the comparative resource advantages possessed by the supplying region. Excluding the textile trade, India in the Mughal period apparently had no comparative advantage in manufactures, and, given the technological developments which were then occurring in the West, we must assume initially that there was no special reason for a sudden transformation in the post-Mughal period.[21] India had some comparative advantage in agricultural commodities and it is these that would have been sold in world markets. Under my conditions, however, Indian agricultural exports probably would have been smaller, partly because there would have been no export pressures in the form of home charges and other imperially-generated remittances and partly because, lacking an imperial connection with India, the

21. The coal industry was developed to meet British-generated demands. The lack before 1800 of both coal and an iron industry based on coal and water power would certainly have persisted well into the 19th century. Indian charcoal iron, as Mr. Heath discovered, tended to be high cost and probably could not have become a substantial export commodity either as a raw material or as the input for other manufactures. For support of this judgement see H. Sanyal, "The Indigenous Iron Industry of Birbhum," *IESHR*, Vol. V, No. 1, March 1968, pp. 101-108. Moreover, Bhattacharyya, *op. cit.*, points to other difficulties which impeded the introduction of improved technology.

British might have generated alternative sources of supply. Furthermore, the Indian export trade probably would have been adversely affected by a British balance-of-payments problem. Barred from selling textile products and other manufactures of South Asia, Britain would have been forced to import less.[22]

Therefore, if Indian agricultural exports expanded less rapidly, probably Indian industry also would have developed less rapidly than it did. After all, the Bombay cotton textile industry, based on Indian capital which was accumulated mainly in the British-sponsored import and export trade, developed as a form of import substitution. The exports of raw cotton to Britain and China and the import and sale of British textile products in the first half of the 19th century improved the knowledge of internal markets and created a network of upcountry commercial relations. This effectively reduced the risks of the Bombay textile mill ventures which were to follow. Had this traffic been smaller, the opportunities to develop a native cotton factory industry on a scale comparable to that which did occur would have been much reduced.[23]

Two additional consequences ought to be mentioned. In the absence of a direct British interest in their construction, it is likely that the development of a railway network would have been delayed. The reduced role of foreign trade would not only have lessened the need, but interest charges probably would have been higher. If decisions to construct rail lines were based on economic calculations these factors would exert a negative influence.[24] And the

22. Given the way in which the world trade network developed in the late 18th and 19th centuries, it is not likely that my assumed reduction of British imports from India would have been offset in any significant way by increased Indian sales to other parts of the world. It is possible that the entire world economy would have grown somewhat less rapidly in the 19th century had India's foreign trade been sharply reduced, but on balance the retardative effects would probably have been marginal. This last point might seem to be challenged by S. B. Saul. *Studies in British Overseas Trade, 1870-1914* (Liverpool. 1960), Ch. III. But India's critical role in the British balance-of-payment and trade structure after 1870 had developed only because the imperial connection existed after 1757.

23. It tends to be ignored that the rapid expansion of India's foreign trade after 1800 was partly attributable to the security directly provided by British naval power.

24. I am assuming that as in the case of most North Atlantic nations, railway construction would have depended on foreign capital to a large extent. If we look at the possibilities of entirely indigenous financing of such enterprises, we must consider whether incentives to private enterprise would have been sufficient to mobilize the required capital. If we think of State financing as the alternative, we introduce complex and unlikely political assumptions.

same factors without providing offsetting incentives for indigenous capital probably would have slowed the rate of British investment in jute mills.

One more question needs to be asked. Was there an innovative potential in India that would offer promise of independent technological developments? With either independent innovation or rapid borrowing of the new Western technology Indian production functions could have been transformed in such a way as to make the bulk of my above argument irrelevant. But there is no sign of this before 1800 in either the areas under British control or the areas independent of it, and by the beginning of the 19th century there would have been the experience of the much lower level of economic activity in the foreign trade sector that I have already described. This together with consequential adverse effects on domestic demand would have created an economic environment that was unfavourable to technological innovation either via borrowing or from indigenous sources. In fact, it has been suggested that for social-structural reasons, there were powerful impediments to the adoption or development of new technology in India.[25]

Professor Raychaudhuri has quite rightly reminded me that we must conduct our investigation not on an all-India level but in regional terms.[26] He hypothesizes (pp. 79-80) that a regional

25. See Bhattacharya, *op. cit*. Although I am not entirely satisfied by Dr. Bhattacharyya's explanation of the forces at work, there is no question that new techniques seem to have spread with great difficulty. In addition to the cases which he examined, there is one other striking illustration. Despite efforts of the East India Company to introduce the fly-shuttle in the late 18th century, the device did not take hold until the early 20th century.

Discussions about Indian technological creativity and receptivity usually neglect the absence of any experimental tradition tending toward modern scientific forms. Western scholars noting the phenomenon have glibly attributed it to ideological and caste barriers imposed by Hinduism. However, it is possible that the relatively slow absorption of Western technology may be partly attributed to the fact that modern innovations were designed for the Western environment and factor proportions. Applications to Indian conditions required considerable adaptation and this was inhibited by lack of a strong experimental tradition and absence of an appropriate educational structure. Clearly there is need for studies that will concentrate on the structure and content of traditional education as it bears on these points. Economic historians who are concerned with estimating the prospects of Indian economic development in the 19th century cannot ignore the implications of this issue.

26. I agree entirely with Raychaudhuri on the need for a detailed regional analysis. Many years ago in an essay prepared jointly with Professor Burton Stein, I made this point very strongly. See M. D. Morris and B. Stein, "The Economic History of India: A Bibliographic Essay," *Journal of Economic History*, Vol. XXI, No. 2, June 1961. pp. 195-196.

analysis would yield different results because

> the highly commercialized coastal regions and their hinterlands,—Gujarat and the West coast, Malabar, Coromandel, the Bengal-Bihar region,—had no less potentialities for growth before 1800 than Japan in the pre-Meiji era.

Yet it is possible that to the extent that these regions were much more dependent on foreign trade than the all-India average, the impact of my assumed protective tariff could have been even more disruptive in these regions than in India as a whole.[27]

However, Raychaudhuri is trying out a different possibility. He suggests (p. 87) that

> a different set of historical accidents,—*e.g.*, a failure of the East India Company to occupy these territories and a decision of the local powers to modernize the army in order to resist western aggression, might have led to a different pattern of economic change . . . From Indian armies commanded by European generals to Indian armament factories run by western experts might not have been a very long step.

This is a fascinating alternative line to explore. But here again we need more precise specification of the underlying assumptions. For example, what political conditions are assumed? Given the circumstances of the entire 18th century, a decision to modernize local armies could have produced more conflict within the system and more disruption than was produced without modernization.

What are the underlying technical and economic assumptions? I suppose that Professor Raychaudhuri is assuming that the growth of several states with modern armies would have produced a demand for products that would have led inevitably to more industrialization. The relationship of war to economic development by way of generating large-scale demand for specific kinds of products has a certain plausibility. Nevertheless, it is not entirely clear that wars have had the creative results that he implies.[28]

27. I ought to mention that all of us have been assuming that foreign trade played a major role in shaping the Indian economy. But, if as I will argue later, foreign trade involved only a very small proportion of India's resources, we would then face a different range of problems. There is a need for studies that will estimate the role of foreign trade in total economic activity.

It could be argued that the benefits arise not directly from the fact of war but from the distributive advantages of victory, from the ability to modify the parameters of economic action in ways favourable to the long-run flow of economic goods. However, to capture such advantages from the jaws of military triumph requires the existence of an appropriate administrative and economic structure. I am not overly impressed by such possibilities when I consider the general assumptions that have to be made. For example, Professor Cipolla has pointed out that Indian artillery technology, to the extent that it was taken over from the West, was for very sound socio-structural reasons borrowed mainly from Turkey. The long-run social, technological and economic implications of this were actually adverse to the process Professor Raychaudhuri anticipates.[29]

I am suggesting that the idea of "historical accidents" is not satisfactory if the term is meant to imply that random chance would produce any result one pleases to choose. Raychaudhuri, however, was not playing fast and loose with hypothetical alternatives. To the contrary, when one specifies his suggestion we are able to enrich our understanding of the socio-economic situation. We become aware that the Indian system opted for Turkish military technology and strategic concepts for reasons that were not accidental and with consequences that seem to have been adverse to the possibilities of further economic development.[30]

28. See, *e.g.*, J. U. Nef, *War and Human Progress* (Cambridge, Mass., 1950.)

29. In this sense, not all military hardware has the same consequences. See Carlo M. Cipolla, *Guns and Sails in the Early Phase of European Expansion 1400-1700* (London, 1965), Ch. II.

30. Those who claim that the British were the main barriers to rapid economic development in the 19th century never consider that in their absence the consequences might have been more like those in the Turkish empire in the 19th-20th centuries, hardly a case of rapid economic change. Yet this possibility strikes me as thoroughly consistent with the historical alternatives we have been considering. The "Turkish" alternative is suggested by the failure of the Indian princely states to modernize their administrations after 1800. Most notably, it was impossible to separate the privy purse from the public treasury. (On this problem, See L. I. and

This hypothetical case could be explored further. But I have said enough to indicate that it is uncertain whether India's economic development, based on foreign trade, would have been any better than it in fact was had she not been burdened by British rule after 1750. It is quite possible that the economy would have developed more slowly that it actually did.[31] I now turn to the second and alternative possibility that the British did establish their power but were driven out in 1857 or in the 1880's. Actually, 1857 is probably not a good date to pick. The uprising of 1857-1858 drew its support essentially from backward-looking groups. It is difficult to believe that the economic and social policies favoured by such groups would have been helpful to economic development. On the other hand, by the 1880's some modernizing groups had emerged around the new industrial, commercial and professional activities. I take it that this is the meaning of Bipan Chandra's statement (p. 40) that

> the major anti-imperialist writers . . . all without exception, accept that the English introduced some structural changes and nearly all of them welcome these changes as the entry of the progressive wind from the West.[32]

S. H. Rudolph, "Rajputana under British Paramountcy: The Failure of Indirect Rule," *Journal of Modern History*, Vol. XXXVIII, No. 2, June 1966, pp. 138-160.)

An alternative possibility, probably even more realistic, is revealed by Chinese experience. (Indian scholars have almost entirely neglected Chinese history although it probably is the most clearly relevant to Indian circumstances.) European nations were absolutely unable to establish an effective foothold in China until the Opium Wars of the 1840's. And during the century that followed they were not able to penetrate in any meaningful way beyond the Treaty Ports. (Japanese development of Manchuria is a special incident that does not really qualify the point.) Although indigenous economic activity and organization was able to survive largely unscathed, China did not exhibit any independent capacity for internal development. For better or worse, most modernization occurred in the Treaty Ports. (I am indebted to Professor Rhoads Murphey who has let me read an early draft of his manuscript on this subject.)

31. Once again, let me stress that this argument is not intended to open up a field of counterfactual analysis. At the present state of our knowledge this would be disastrous. I used the technique only to suggest that the Classical assumptions, hidden and explicit, do not necessarily lead to Classical conclusions. Nothing further is intended, and I strongly warn against any attempt to do more.

32. Bipan Chandra (pp. 40 n 8 and 55) agrees with the Marxian view that European intervention was necessary for the shattering of the static traditional order. This puts him at odds with Professor Raychaudhuri who has a much more sophisticated view of the Mughal system.

What then could we expect of subsequent economic developments had India suddenly found herself free to shape her own independent destiny after 1885?

There are great analytic difficulties in this question. I can conceive of a variety of alternative possibilities the choice of which would depend on one's assumptions regarding the distribution of political power in the system.[33] This is not the place to explore the alternatives.

Nevertheless, it is worth noting that all those who view the presence of the British after 1880 as the sole factor responsible for India's economic backwardness neglected this problem. Rather, I propose to examine the implications of a fundamental assumption that is implicit in Bipan Chandra's statement (p. 40) that Indian industrialization was "delayed, frustrated, and obstructed" and in his reference (p. 41) to the "arrested nature of India's industrialization . . ." I do this because his essay gives explicit voice to the rather widespread assumption that India would have followed a natural, inevitable path of economic development leading to industrialization had British policy not distorted and frustrated it.[34] This assumption is important. Its pervasive influence has led scholars to neglect virtually all aspects of Indian economic history other than the impact of British policy. It is also the reason many scholars such as Bipan Chandra are willing to accept as documented and proven a whole range of propositions that by any sober test of evidential adequacy would have to be treated as unsubstantiated. Can we automatically assume that had British rule ended in 1885 (or had it not existed at all) a rapid rise in the rates of economic performance and a massive transformation in the structure of the economy—an industrial revolution, to use a summary term for this combination of developments—would have occurred?

Is it sufficient to assume, as Bipan Chandra does, that, because the Nationalist writers attacked British rule as the fundamental

33. Certainly, what one assumes was the distribution of power among landlords, peasants, merchants, industrialists, moneylenders and princes would affect the consequences one would anticipate.

34. In fact, one gets a very clear impression that elimination of British rule would have led to an industrialization rivaling that of Britain. This seems to be implied by Bipan Chandra's references (pp. 41 and 69) to the widening economic distance between Britain and India during the period 1800-1947.

cause of India's economic backwardness, they were necessarily right about either the cause or even the factual characteristics of the situation? It must be recognized that much of the material of modern Indian history was generated in the course of sharp policy debates. This means that not only is a great deal of the evidence contradictory, but frequently the contemporary interpretations of the evidence are also contradictory.[35] The fact that policy objectives inspired scholarship does not make it wrong. But it does add to the difficulty of accepting any one interpretation of modern Indian economic history as necessarily the correct one. How then does one proceed to examine the actual pattern of development?

For writers such as Bipan Chandra the issue is quite simple. India's great poverty and its failure to match the economic progress of Britain and the "U.S.A., France, Germany, Canada, Italy, Russia, and even Japan" before 1947 proves his position. (p. 41). In other words, he accepts as a basic axiom the inevitability of rapid economic development. Once this teleological assumption is accepted, the only factor that could have prevented the inevitable progression was the exogenous effect of British rule and policy.[36]

I cannot accept this teleological notion for a number of reasons. The occurrence in the 19th century of an industrial revolution in Britain and a limited number of other countries does not make it automatically inevitable that a similar development should have occurred also in India. Nor does the fact of acceleration of economic growth and change in "follower countries" in the 20th century, specifically Russia and Japan, prove that an industrial

35. The problem is partly a consequence of different social perceptions and long-run objectives and occurs whether the data are generated in the debates among British policy makers or between them and the Nationalists.

36. In order to keep the record straight, I ought to point out that Bipan Chandra's facts are at least partly wrong. Italy before World War II was certainly not a case of rapid economic progress however the process is measured. (See, *e.g.*, R. S. Eckaus, "The North-South Differential in Italian Economic Development and *Journal of Economic History*, Vol. XXI, No. 3, September 1961, pp. 285-317; Angus Maddison, *Economic Growth in the West* (New York, 1967), Ch. I.) This latter source also shows that French performance, measured by annual rate of growth of total output between 1870-1950, was only marginally better than Italian. Moreover, Russia before 1930 could not boast of any substantial economic advances. In 1890 and in 1913 her *per capita* income was the lowest in Europe and in 1930 it was probably farther behind relatively than it had been three decades before. Not until after 1930 did Russia begin to exhibit the structural features of a modernizing economy. See M. E. Falkus, "Russia's National Income 1913: A Reevaluation," *Economica*, N.S., Vol. XXXV No. 137, February 1968, pp. 52-73, particularly p. 62.

revolution was inevitable in South Asia at the same time. To the contrary, the very fact that each of these countries which we now call developed entered the "development stream" at a different point in time, proceeded at different rates of development and emerged in the present day with different structural consequences and different levels of performance, indicates that the process was not inevitable. After all, a large number of independent countries with apparently favourable population/resource ratios—*e.g.*, in Latin America—have not done very well. In other words, there is no historical evidence of a teleological force which moves nations along a logically necessary and inevitable path towards a specific economic structure or a specific growth rate.[37]

There is, as I indicated in my orginal essay, a cluster of basic preconditions that a system must realize before economic development can proceed at a high rate.[38] It is insufficient to establish

37. I must note the lack of precision in Bipan Chandra's essay. He is not clear about what he thinks India would have achieved by 1947 if British rule had ended in the mid-19th century. He refers (p. 41) to the widening "economic distance" between India and Britain 1818-1947. His term "economic distance" is peculiarly ambiguous. At various places in his essay he implies three different measures: the long-run growth rate should have been the same, or the economic structure in 1947 or the level of economic performance in 1947 should have been the same. He compares India with countries whose economic experiences have been as diverse as the United States, Italy and Japan. Bipan Chandra seems to be not only unaware that developed countries had very different long-run growth rates and ended up with distinctive economic structures and quite different levels of economic performance, but he also does not seem to understand that there is no necessary correlation among these various measures of economic achievement. Let me give a rather striking example to illustrate this point. Australia with a growth of *per capita* product of only 8 percent per decade during the last century ranks fifth among the nations of the world in gross national product per person. On the other hand, Japan, with a growth rate more than three times as rapid as Australia's ranked fiftieth among the nations in terms of GNP per person. (Australian GNP *per capita* in the mid-1950's was slightly more than five times that achieved by Japan!) Moreover, the economic structures of the two countries are significantly different. The diverse behavior of these three indexes suggests that Bipan Chandra needs to be explicit about precisely what he means. They also intimate the extreme importance of something else that Bipan Chandra neglected entirely, the decisiveness of the economic performance level and socio-political organization from which a region begins its economic growth. (The figures cited came from S. Kuznets, *Modern Economic Growth: Rate, Structure and Spread* (New Haven and London, 1966), pp 10-11 The volume by Kuznets, particularly Chapter 9, is directly relevant to my analysis).

38. These preconditions can be roughly identified as the need for some minimum degree of political stability and flexibility; facilities for the appropriate accumulation and allocation of capital; rapidly widening markets; increasing agricultural productivity; and the creation of a mobile labour force. Stated in this fashion, they are intended to be a taxonomy and not an elaborated theory. Thus the preconditions are not subject to Gerschenkron's criticism of Marxian preconditions. See A. Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1962), Chs. I and II.

merely the formal presence of these preconditions and then use this as proof that in the absence of some "distorting force" rapid economic growth and change would have occurred. A society must create appropriate socio-political linkages before the formal economic characteristics will yield sufficient as well as necessary conditions of growth. For example, Dobb suggests that Western Europe, and particularly Britain, was able to transcend the structural inhibitions of medieval economic life only as a consequence of a specific conjuncture of social, political and economic circumstances after 1300. Dobb's discussion of the "recrudescence of serfdom" reveals how with a different concatenation of relationships, Eastern Europe and Russia followed an entirely different path after 1300.[39]

For a later period, Eric Hobsbawm showed how Britain's move to an industrial revolution before it occurred elsewhere depended on the specific outcomes of the 17th century revolutionary upheavals. John Nef enabled us to see how initial structural differences between France and Britain 1540-1640 were crucial to the ultimate results.[40] Professor Gerschenkron provided us with an explanation of the process by which various nations entered the industrializing stream in the 19th century. Although his analysis differs from the Marxian formulation in many ways, it confirms the view that there was neither a "natural and inevitable" tendency towards industrialization nor a single path that a development process need follow once it got started.[41]

Even if one argued that in the Marxian model there was an inevitable tendency to move from feudalism to industrial capitalism, it is dubious whether the stage analysis that Marx advanced as relevant to Europe would be appropriate for India. Marx, after all, had an alternative social analysis for Asia that was based on his concept of "oriental despotism" and this he applied to India. Even

39. M. Dobb, *Studies in the Development of Capitalism* (London, 1946) Ch. II. For a specific examination of the distinctive East European process, see J. Blum, "The Rise of Serfdom in Eastern Europe," *American Historical Review*, Vol. LXII, No. 4, July 1957, pp. 807-836.

40. E. J. Hobsbawm, "The Crisis of the Seventeenth Century," in *Crisis in Europe, 1560-1660*. Edited by Trevor Aston. (London, 1965), pp. 5-58; and J. U. Nef, *Industry and Government in France and England, 1540-1640* (Ithaca, N. Y. 1957.)

41. Gerschenkron, *op. cit.* See also Kuznets, *op. cit.*, Ch. 9.

if one prefers to think of the pre-British era as "feudal" rather than "oriental," it is obvious that "feudalism" in South Asia was quite a different socio-structural phenomenon than feudalism in the West. But whatever the choice, there is no logical reason to expect that the consequences of shattering the Indian socio-economic structure that was so different from any in the West should have paralleled in rate or timing those that occurred there.

Once the assumption of a natural and inevitable tendency towards industrialization is purged from our minds and our work, both the range of questions that we can ask and the answers that we can expect become enormously widened. More attention would be paid to the nature of the pre-British period and its links with the period after 1800. We would recognize that the specific institutional and economic characteristics of the traditional Indian economy may have had as much to do with what happened after 1800 as British policy did.[42] In fact, we would be in a position to examine the actual impact of British rule with greater care than has so far been exhibited. Moreover, once we shake that fundamental preconception, our methods of research and ideas of proof will be forced to change.

Professor Raychaudhuri recently expressed his disenchantment with much historical scholarship when he referred to

> a common frailty of our historians, namely, the tendency to quote contemporary opinion on a crucial question as indicative of what happened rather than to examine the factual evidence bearing on the point.[43]

The difficulty of this type of scholarship-by-quotation in economic history is that there is no way of determining the accuracy of either the conclusions or the facts themselves.[44] Let me offer an example.

42. Here I have in mind the significant observation made in one way or another by both Toru Matsui and Tapan Raychaudhuri that extensive parts of the subcontinent were primarily engaged in subsistence activities both before and long after 1800.

43. In a book review which appeared in *IESHR*, Vol. III, No. 3, September 1966, p. 319.

44. It should be clear that I am referring to the studies of the actual economic process. To a scholar concerned with the history of debates over policy—how, for example, a land settlement policy was worked out or what factors influenced tariff or railway policy—the conflict of opinion and interest is the very substance of his story.

It is widely believed that the proportion of landless labour in India increased sharply during the course of the last century. This assumed fact has led to a rather elaborate interpretation of its causes. The legitimacy of the interpretation in the first instance, however, rests upon the accuracy of the fact, and in this case a recent study of South India, the region supposedly most affected, indicates that there was not a significant increase in the proportion of landless labour.[45]

But accuracy of specific facts is not enough. How are the relevant facts selected? How do we penetrate to meaning, to the relationship among facts? In order to study the functioning of an economy, it is necessary to utilize the array of tools, statistical and formal, that economic theory has developed specifically for analysis of economic behavior and relationships. The use of economic theory will not predetermine the specific conclusions about past economic performance. As I have said before, it helps us to be more specific about our questions, more explicit about our assumptions and more cautious about evidence we accept as being adequate for testing our hypotheses. It also will help us to determine the accuracy and limitations of our answers. In these terms, neither my argument that there was no inevitable tendency towards industrialization nor my belief in the usefulness of economic theory as applied to economic history leads to any predetermined judgement about the career of the Indian economy after 1800. Only detailed research can ultimately show whether and in what ways the economy expanded, stagnated and declined. At this stage it is important that we do not assume away the most significant and interesting issues.[46]

This is the realm of political and intellectual history. I should also note that in policy debates it is somewhat irrelevant whether a participant has his facts right. It is important only that his perception of reality, whether correct or incorrect, helps shape his policy position. But if we want to determine the actual consequences of a policy (and not what people thought the consequences were), then opinion is not a satisfactory substitute for a specific evidence and formal tests.

45. Dharma Kumar, *Land and Caste in South India: Agricultural Labour in the Madras Presidency during the Nineteenth Century* (Cambridge, 1965.) See, also, Morris, *IESHR*, June 1966.

46. I have frequently encountered historians who seem to feel that they cannot justify the Nationalist struggle for independence unless they can show that economic conditions deteriorated under British rule. This is surely a *non sequitur*. (If this were a legitimate position, an American would find it hard to justify his own fore-

III

I turn now to another methodological issue, the application of quantitative methods to the study of Indian economic history. Both Toru Matsui and Tapan Raychaudhuri entertain serious doubts about the possibility of quantifying Indian economic behavior prior to 1800. They offer strong reasons why we could not develop useful measures of overall economic activity for the period before the late 19th century. There is no question but that I was excessively bold when I tried at this stage of our knowledge to suggest the possible trend of total and *per capita* income. However, my difficulty arises not from the Matsui-Raychaudhuri view that any such attempt is doomed to failure but from the fact that studies which could generate the basic data have not yet been undertaken.

Matsui (pp. 19-20) points correctly to the conceptual and practical difficulties of estimation for an only partially monetized economy. He properly notes that aggregative indexes such as those that measure *per capita* income can conceal as much as they reveal. Raychaudhuri evokes a similar substantive difficulty when he refers to the lack of an all-India economy until well after 1800 and argues (p. 79) that the attempt to construct an all-India index out of widely differing regional performances is therefore "a fruitless quest."

Matsui and Raychaudhuri raise formidable methodological issues the importance of which I would be the last to minimize. In fact, I would argue that these difficulties have not even been satisfactorily overcome in either the post-1947 Pakistan or Indian national income studies. But it should be recognized that these aggregate indexes are not arrived at directly. They are constructed from

fathers' revolution because it is likely that the economic conditions of the American colonies improved under British rule!) But in fact, independence has its own moral justification and the demand for it did not depend on a specific level of economic performance. Revolutions are made not only by facts but also by how people perceive facts. Whatever was happening in the economy, if the Nationalist leaders felt that the conditions were getting worse, or were not improving rapidly enough, or were changing in inappropriate ways, it was their discontent and their ability to generalize it that was important. This view of the situation makes it possible for political and intellectual historians to develop an analysis that is far more fruitful and sophisticated than the so frequently employed and rather simplistic link between poverty and unrest. Moreover, such an approach enables scholars to ask a new range of questions about the nature of social perception and the links between ideology and group action.

detailed economic studies of individual sectors and regions. Thus, if we apply ourselves to careful studies of trends in various parts of the economy, the results ultimately can be combined into reasonable quantitative estimates of an aggregative sort. One intent of my essay was to identify some specific sectors that require detailed scholarly study.

Though we must recognize the difficulties of interpretation and the limited usefulness of any single aggregate index of performance, we should not allow this to lead us to an implicit or explicit denial of the fundamental need for quantitative work of all sorts. Whatever the tasks are, economic history is essentially a quantifying form of scholarship. Regardless of whether we are able to find adequate statistical data for a problem we choose to investigate, we always are making quantitative judgements. They are made not only when we deal with matters such as total or *per capita* income, but they also permeate a vast bulk of the qestions that interest economic historians. Indeed, my critics when they go beyond strictly conceptual issues disagree with me on essentially a quantitative basis. A statisfactory resolution of these issues depends on quantification.[47] Let me illustrate with some examples.

Professor Raychaudhuri disagrees (pp. 85-87) with my view that in the Mughal period "commercial activity, international and domestic, must have constituted an infinitesimal portion of total economic activity on the sub-continent." Both of us recognize the difficulty of getting satisfactory measures of this. Obviously there was commerce, but how much? Was it at all comparable to the growing proportion of GNP that commerce seems to have represented in the 19th century? The problem arises out of the vivid but imprecise writing of scholars who work on the Mughal period and give the impression of an economic system in which commercial activity was extremely important. It appears in Raychaudhuri's description of the scope of foreign trade and from his reference to the "ant-tike activity along the internal trade-routes criss-crossing

47. I recognize that precise quantitative data are frequently lacking. But problems can be made less ambiguous and answers can be made more explicit by using the data that are available to achieve ordinal if not cardinal accuracy. Moreover, it is possible to infer a great deal when imaginative statistical techniques are combined with the use of economic theory. See *e.g.*, A. H. Conrad and J. R. Meyer, *Studies in Econometric History* (London, 1965), particularly pp. 3-40.

the subcontinent..." My reading of the evidence suggests that such descriptions vastly overstate not only the proportion that commerce was of total economic activity but also its significance for potential economic growth.[48]

Let me first examine the scale of domestic commerce. Raychaudhuri points out that there was only a one-way tributary traffic between country and towns. The fact that urban commodities found no markets in the country-side clearly reduces the significance of commerce in the economy. At the same time, there was very little regional and local specialization which means that commercial activity within the rural sector was of an extremely small order.[49] This combination of characteristics leads me to infer that internal trade could have constituted only a very small part of total domestic economic activity. If commerce was indeed a large share of GNP, the burden would have to fall very heavily on international trade.

Professor Raychaudhuri (p. 85) feels that foreign trade, overwhelmingly dependent on textiles, satisfies this requirement because India was the major supplier of both fine and cheap fabrics to the whole of South-east Asia, the mid-East and East Africa during the 16th century. The trade was later expanded by the Europeans to Europe, West Africa, the New World and Japan. Is it possible to get a sense of the quantities of resources involved?

The only study I know that offers any sense of the scale of early 16th century trade refers to Southeast Asia which was one of the major markets for Indian commodities. At that time about a hundred large ships arrived annually in Malacca, the major entrepot. Of these, fewer than twenty came from all South Asia. Four to six came from Coromandel, six to eight came from Bengal and another five came from the Gujarat. At most this would have constituted

48. Even if we are able to establish the proportion of total GNP that trade constituted, it would not automatically settle the question of whether the sum was a "small" or "large" proportion of the total. "Small" and "large" typically carry with them implications of significance and there should be recognition of this when the terms are used.

49. T. Raychaudhuri, "The Agrarian System of Mughal India," *Enquiry*, N.S., Vol. II, No. 1, Spring 1965, pp. 118-120. (Henceforth cited as *Equiry*.) See also *Habib*, p. 77.

no more than 6,000 tons of shipping.[50] Although the traffic was of high relative value, the physical resources involved were clearly of a small order and were divided among the three major regions on the subcontinent.

We do not yet have satisfactory data on the physical quantities of cloth that were involved in this early 17th century foreign trade, hence we are not in a position to estimate its employment effects. Buchanan collected some data in the early 19th century which suggest that on the average a weaver could produce 2.6 yards of cloth per day. Allowing for the fact that it was mainly better grade cloth that entered into foreign trade, I estimate that a weaver could produce two yards of cloth per day.[51] If we define a 300-day work-year as full time employment, the production of a million yards of cloth for the foreign trade sector would require fewer than 1,700 full time weaver equivalents. Complementary full time labour probably amounted to five times the number of weavers required, therefore a million yards of cloth produced for export would give employment to fewer than 11,000 workers in the textile sector. It is still impossible to offer a reasonable estimate of the amount of cloth exported in the early sixteenth century, but my calculations of production possibilities indicate that our estimates about the employment effects of the foreign trade in textiles probably need to be scaled down. Our views about the total effects of foreign trade on the economy would have to be adjusted accordingly.[52]

50. M.A.P. Meilink-Roelofsz, *Asian Trade and European Influence in the Indonesian Archipelago between 1500 and about 1630* (The Hague, 1962), pp. 43 and 87-88. The author estimates vessel size ranged from 100 to 400 tons. I have assumed twenty vessels averaging 300 tons to arrive at the 6,000 ton figure. To the extent that other commodities were carried or average ship size was smaller, the textile carrying capacity would have to be reduced. These figures do not include the Arabian Sea trade, nevertheless they are useful to indicate the probable orders of magnitude with which we are dealing. The small tonnage suggests that Raychaudhuri may have overestimated the amount of "everyday wear for the masses" that was carried, a judgement supported by the fact of high transport costs.

51. Francis Buchanan, *An Account of the Districts of Bihar and Patna in 1811-1812* (Patna, N. D.), Vol. II, Appendixes 42 and 43, pp. 774-775. My calculation assumed a standard yard of 36 inch width. This estimate and the ones which follow are extremely crude. I hope to publish much more careful calculations in the near future. I am offering these early and very tentative estimates only as an indication of the quantitative possibilities that do exist.

52. Professor Das Gupta also suggested to me that I underestimated the international trade in common cloth. If he and Professor Raychaudhuri are correct, the employment effect of any given volume of cloth export needs to be scaled down even more than I have suggested because output of coarse cloth per weaver was much greater than output of fine cloth.

Our ideas about the subsequent European impact on textile exports needs similar readjustment. For example, at the peak of the Dacca trade, in 1747, European merchants (in both private and official trade) were responsible for apparently no more than one-third of all textile exports. And less than 20 per cent of Dacca exports went into the European trade.[53]

All of this is extremely tentative and requires detailed research, but it does suggest methods whereby estimates of the quantitative significance of foreign trade before 1800 can be got. As they stand, these inferences, which are not inconsistent with the evidence we now have, suggest that South Asian international trade, however impressive its structure and sophisticated its administration, had relatively small effect on resource employment.

More general empirical evidence supports this view. Kuznets has shown for modern times that there is a strong negative correlation between the size of a country and the proportion of GNP [that is foreign trade.[54] Even after 1800, when foreign trade grew rapidly as a share of total Indian economic activity, it never seems to have amounted to more than 7 or 8 percent of total GNP. Thus the sheer size of the Indian economy even in the Mughal period makes it unlikely that foreign trade could have played a major role in the economic life of the region.[55]

With this reasoning in mind I had suggested in my original essay

53. Moreover, at least 25 percent of the Dacca export trade actually went for consumption elsewhere in North India. See Abdul Karim, *Dacca the Mughal Capital* (Dacca, 1964), pp. 87-88. Professor Raychaudhuri seems to agree that even after the European companies intervened the impact of foreign trade in textiles was "rather limited." (Raychaudhuri, *Enquiry*, p. 119).

On the other hand, there is evidence that total foreign trade, Indian and European, did expand during the later 17th century and early 18th centuries. The number of native vessels sailing from Indian ports was apparently much larger than a century before. Here the role of European naval power in expanding total international commerce—a naval authority which no South Asian authority ever pretended to emulate—was crucial.

54. Kuznets, *op. cit.*, pp. 300 ff. Bipan Chandra (p. 73, note 73) obviously misread Kuznets on this point.

55. Kuznets, *op. cit.*, pp. 30?-306, shows that in the 19th century Indian foreign trade was growing at a faster rate than the rate at which total world trade was growing. Also, foreign trade growth rates were more rapid than GNP growth rates in the industrializing countries. Thus, unless we assume that Indian GNP was also growing at an equivalently rapid rate, foreign trade as a proportion of GNP necessarily had to rise during the period. None of this would be useful proof of my point if it could be shown that Indian foreign trade fell sharply after 1750 (as distinguished from being transferred into European hands), but it is not clear that this occurred.

that the impact of India's foreign trade was felt primarily in the European economies. On this point specifically, Kuznets estimates that in the 18th century India's share of total world trade was exceeded not only by the United Kingdom, but also by France, Germany and Spain in Western Europe. Given the small absolute size of these economies when compared with the Indian, the quantity and value of trade that was insignificant to the latter would have been of considerable significance to the former.[56]

Nevertheless, this point should not tempt one to assume that Brooks Adams was correct in his exaggerated stress on the importance of the India trade to British economic development in the 18th century. North American/Caribbean trade, for example, was of much greater importance to Britain in 1700 and its significance became increasingly more marked as the century moved on.[57]

Professor Raychaudhuri may agree with my argument as applied to all-India, but he (pp. 79-88) prefers to stress the impact of pre-1800 trade on specific regions—Bengal, the Coromandel and Malabar coasts and Gujarat. It is arithmetically obvious that if all (or most) Mughal foreign trade were allocated to these regions, its economic significance would have been proportionately greater. Still the population for each of these regions was certainly much larger than the population of most Western European nations at the same time.[58] In terms of the argument already offered, the impact of foreign trade on the regional economies of India is likely to have been smaller than the same amount of trade on the various countries of Western Europe even without considering the institutional differences between parts of Western Europe and the commercial regions of India. Structural differences probably made the

56. The main condition under which this proposition would be invalid is one which posited that in the Mughal period Indian GNP was absolutely smaller than in these Western European countries. See Kuznets' general discussion, *ibid.*, pp. 302-304.

57. In 1700 the value of British commodity trade (imports, exports and re-exports) with India was 7.3 and with the Americas was 16.0 percent of all British foreign trade. In 1773, British trade with India had risen to 9.5 percent and with the Americas to 34.9 percent of all trade. In 1700 British trade with the Americas was 2.2 times as great as with the East; by 1773 it was 3.7 times as great. (These figures have been calculated from data in Ralph Davis, "English Foreign Trade 1770-1774," *Economic History Review*, 2nd S., Vol. XV, No. 2, 1962, pp. 300-303.)

58. W. H. Moreland, *India at the Death of Akbar* (London. 1920), pp. 9-22; and K. Davis, *The Population of India and Pakistan* (Princeton, 1951), pp. 24-25.

impact of foreign trade on the former even greater than would be suggested by purely quantitative measures.[59]

It should be apparent by now that there are inherent quantitative implications in most questions relating to the economic history of the pre-British period. Clearly, it would be desirable to approach the material with a greater awareness of this. There is really no need for the same sort of justification of quantitative studies of the British era. However, economic historians of modern India, although presented with quantitative data in profusion, have used the materials in a very limited and typically unsatisfactory way. Let me illustrate with one or two examples.[60]

A major proposition of the conventional wisdom is that agricultural rents rose steadily through the 19th and 20th centuries and became an increasing burden on the rural sector. (BC, pp. 48-52; and TR, pp. 92-23.) Despite virtually universal acceptance of this idea, there is not one solid piece of research that actually examines the movement of rents in detail. Ironically, the only quantitative statement on long term rent movements in a single district that has come to my attention suggests precisely the reverse of the Classical conclusion.

Walter Neale's data for Muzaffarnagar District in Uttar Pradesh show that between 1840 and 1924 the number of people per cultivated acre rose by 47 percent, the size of an average holding fell by 22 percent, rent per cultivated acre rose by 315 percent and land revenue per cultivated acre rose by 63 percent.[61] These figures, showing a rise in population, a decrease in average size of holding and an increase in rent and land revenue per acre, are not only consistent with the Classical interpretation but would seem to offer

59. The problem in discussion about the regional impact of foreign trade is partly associated with determining what the region is. In some places Raychaudhuri seems to be concerned with rather large regions (*e.g.*, Bihar-Bengal) and in others he seems to be thinking of not very extensive areas. It is possible that he is referring to the actual trading centers and the relatively circumscribed hinterlands from which they regularly drew their supplies. If so, the differential effects of foreign trade in India and Western Europe would have to be attributed to institutional differences, differences in the nature of the State and the character of towns and merchant groups.

60. There are some recent honorable exceptions to this stricture, particularly the work of George Blyn, K. Mukerji and Dharm Narain. But detailed quantitative analysis has been almost entirely concerned with the 20th century.

61. Walter C. Neale, *Economic Change in Rural India* (New Haven and London, 1962), p. 164.

a precise confirmation of it. However, none of these characteristics, whether taken separately or together, prove anything about the economic burden on cultivators until we know what happened to productivity. And in Neale's example the value of output per acre (productivity) rose by 900 percent in the 84 year period[62] Thus, rent as a proportion of the value of output per acre fell by 59 percent and land revenue as a proportion of the value of output per acre fell by 85 percent during the period. In other words, in the Muzaffarnagar case the real burden of rent and land revenue per acre fell sharply. Apart from other implications, the example challenges those who glibly assume that all gains were transferred into the hands of landlords and the State.[63]

I am certainly not suggesting that this single piece of evidence is sufficient, but at least two obvious conclusions can be drawn from it. The first is that statistical investigation can certainly shake long-cherished but essentially undocumented impressions. This should stimulate us to examine the career of the Indian economy in a less doctrinaire fashion than has typically been done. The second lesson we can learn relates to the dangers inherent in unsophisticated reliance on individual indexes of performance. If we depend solely on data showing declining acreage *per capita* or absolute rent increases per acre without considering productivity changes we can be seduced into totally incorrect conclusions.[64]

The revisionist potential of quantitative work in the field of

62. This phenomenon raises serious questions about the reliability of Bipan Chandra's statement (pp. 48-49) that production functions in Indian agriculture did not change. The assumption of constant productivity, which certainly has not been demonstrated by any research is responsible for the unsatisfactory state of much of the discussion of Indian agricultural problems after 1800.

63. This positive real income effect could only be rendered meaningless if the population that was supported per acre overwhelmed the productivity effect. In Neale's data this is not the case. Value productivity per person (*i.e.*, value of output per acre divided by population per acre) rose by 580 percent.

B. M. Bhatia, "Growth and Composition of Middle Class in South India in Nineteenth Century," *IESHR*, Vol. II, No. 4, October 1965, p. 344. reports that the index of assessment per acre in Madras fell from a base of 100 in 1851-55 to 72.5 in 1886-91, and at the same time the index of grain prices rose from 100 to 147. Although he claims that this failed to bring prosperity to the peasantry, he provides no specific evidence to support such a conclusion.

64. These kinds of difficulties have existed in the economic histories of other societies. For the analysis of a similar problem in Japanese economic history, see M. D. Morris, "The Problem of the Peasant Agriculturist in Meiji Japan, 1873-1885," *Far Eastern Quarterly*, Vol. XV, No. 3, May 1956, pp. 357-370.

agricultural history does not rest solely on this one example from Muzaffarnagar District. Dr. Dharma Kumar's work has been particularly unsettling to established notions. It is well known by now that her study seriously challenges the canonical views that landless labourers were a creation of the post-1850 period and that the growth rate of this class was very rapid. For at least one period, 1881-1901, her data suggest that the proportion of landowners to work force in agriculture actually increased substantially. These conclusions, consistent with Neale's evidence from an entirely different part of the subcontinent, seem to indicate the need for a detailed re-examination of our notions about what was going on in the agricultural sector and hence in the economy as a whole.[65]

I have said enough to suggest that detailed quantitative work is necessary where it is possible and in cases where quantitative data are not as yet available it is still desirable to maintain a sense of magnitudes and proportions. Although fundamental questions cannot be resolved always by purely quantitative tests, a great many important issues depend on explicit (or implicit) estimates of economic trends, rates of change, proportions and averages. All of these are matters susceptible to and dependent on quantitative research. Moreover, the statistical dimensions established by quantitative investigations also help define the parameters within which theoretical analysis can proceed. Concern with ideas of quantity and relative magnitudes enables us to be less ambiguous in our formulations and more precise in our judgements. Furthermore, a commitment to quantitatively oriented research leads inevitably to an examination of hitherto generally neglected but extremely important subjects.[66]

65. Kumar, *op. cit.*, and Morris, *IESHR*, June 1966, pp. 185-209. At this point I am moved to ask Bipan Chandra to retract his unequivocal statement (p. 75) that "no evidence has been offered so far to justify even a hint" of the need to modify the Classical interpretation. Dr. Kumar's work is known to him. His failure to be influenced by it indicates again that an excessively strong and emotional commitment to a single intellectual model can endanger sound scholarship.

66. One would expect, for example, specific studies of the economic effects of railway development, studies of price behavior and the business cycle, analyses of 19th century international and domestic trade, etc.

IV

A fundamental aspect of my original essay is the proposition that traditional India even at the apparent peak of its pre-1800 performance was structurally and functionally more like late medieval or early modern Eastern Europe than it was like 18th century Western Europe. Only Professor Raychaudhuri comes directly to grips with this theme, and he, of course, disagrees sharply with my interpretation.[67]

Bipan Chandra seems to accept a view at odds with Raychaudhuri's.[68] This is worth noting because it points to a specific characteristic of work on Indian history, the deep cleavage between those who work on the traditional society and those who are preoccupied with the modern period. This gives the impression that what went on before 1800—in terms of structure, function and level of performance—is essentially irrelevant to 19th century developments. For example, recent work on the Mughal period suggests, among other features, that land could be rented or sold, that there was production for the market, that the moneylender was an important figure in rural economic life and that the system generated large groups of landless labour.[69] Students of the modern period have made no serious attempt to establish the bearing that these propositions have on our views of post-1800 developments. Yet I see two essentially contradictory sets of implications. We argue either that scholars of the Mughal period have grossly misinterpreted the meaning of their studies, or we accept the probability that the consequences of India's exposure to 19th century international commercial forces were much less revolutionary than most modern historians claim. To the extent that production for

67. I should make it clear once again that I was not implying a unilinear "stage theory" of economic progress through which all societies inevitably have to move. I was stressing only that traditional India had not developed the characteristics that made for economic expansion and change in Western Europe. Thus, in my original essay it was not necessary to state a judgement on the controversy between those who think of traditional India as "feudal" and those, such as Marx, who viewed the society as a form of "Oriental Despotism."

68. Bipan Chandra explicitly accepts the traditional Marxian interpretation. (See p. 55 and p. 40, particularly footnote 8).

69. See Raychaudhuri, *Enquiry*, for an excellent summary of the current state of knowledge on the subject.

the market, considerable class differentiation in the rural sector and large amounts of rural indebtedness developed initially within the traditional system, they could not be the specific consequences of the imposition of foreign rule. Such a possibility immediately widens the range of problems we must examine if we are to understand the behavior of the Indian economy after 1800. I have in mind this continuity when I suggest that although studies of the British impact on India are important, they neither exhaust the relevant issues nor do they yield in themselves the substance of a balanced view of the economy's career. We need many studies on the persistence of the traditional economic features and on how these indigenous relationships affected economic performance over time.[70]

There are other reasons why we cannot dismiss traditional organization and function as being irrelevant to 19th century experience. Raychaudhuri and I agree that British rule on the subcontinent was not established in one simultaneous act. We agree that it was a process which took nearly a century to achieve. The introduction of British administration and its sensitivity to existing institutions varied over time and by region. Thus, traditional economic institutions and arrangements survived more or less intact until conquest occurred, and even then they persisted in many areas.[71]

Moreover, the act of conquest in itself was not sufficient to establish British authority. Whatever were the objectives of the British *raj*, its administrative ability to achieve the ends it set for

70. As a matter of convenience I frequently refer to the 18th century as part of the Mughal era. This is certainly questionable usage The period is one of considerable change, even in economic terms. On the foreign trade side there is an increasing dependence on the Europeans, a shift in the weight of the country trade from West Asia to the Far East and a greater emphasis on the export of raw cotton as compared with cotton textiles. Internal economic activity was certainly affected by both foreign trade shifts and domestic political changes. However, for the purposes of this essay no harm is done by my failure to deal with the issue.

There is one other matter that should not be taken for granted although I am forced to ignore it in this essay. While the Mughals dominated Indian affairs after the mid-16th century, it still may be inappropriate to assume that all of the subcontinent, particularly Southern India, was affected by Mughal economic policy and organization.

71. Professor Raychaudhuri (pp 89-90) and I would agree on this point. We would both accept the likelihood that the British impact on the Indian economy would be less dramatic before 1820-1830 than most writers have typically assumed. But I suspect that we would draw different implications from the fact. I believe that an excessive preoccupation with Bengal and the tendency to generalize its experience to the rest of the country has led to an exaggerated view of the British impact.

itself—its power—was never as great as has been assumed. Even within the sectors the British were prepared to transform their effectiveness was limited and slow to come.[72] If the impact of British rule was less rapidly and less throughly felt than has typically been argued, the importance of indigenous economic institutions and behavior (as distinguished from "the impact of the West") has to be recognized.[73]

Viewed in these terms, understanding 19th century South Asian development depends on knowing how the pre-existing traditional structure worked. Professor Raychaudhuri, quite properly, has stressed (p. 79) the enormous regional diversity and the absence until well into the 19th century of any significant economic integration on a national or even multi-regional scale.[74] The implications of this are of great importance. It means that functional interdependence among regions was so limited that changes affecting one area had little if any effect on others. There was no State apparatus that was sufficiently powerful to impose on the subcontinent or on any multi-regional part of it the kinds of policies and types of economic demands from which a common economic response or interdependence would develop. In other words, the impact of Mughal imperial power as an integrating factor in Indian economic life certainly has been exaggerated by many writers.[75]

In this context I disagree with Raychaudhuri's comparison (p. 79) of Mughal India with the European economy in the 18th century. From the medieval era onward a large part of European economic development and political conflict was intimately associated with

72. The point has been illuminated in considerable detail by R. E. Frykenberg, *Guntur District 1788-1848* (Oxford, 1965).

73. I did not stress this enough in my original essay. It does not affect my point about the economic significance of political stability in 19th century and after, but it might help to explain the relatively slow rate of growth and limited transformation of economic relationships after 1800. The important fact was not the change that the British made but rather their failure to disrupt Indian life more.

74. Raychaudhuri recognizes the regional fragmentation, but he occasionally falls afoul of the fallacy that this was "entirely irrelevant to post-1800 developments" (pp. 78 and 87-88). The growing regional interdependence of the subcontinent would be an extremely fruitful theme around which to organize research into post-1800 economic history.

75. One must distinguish between the Mughal's ability to impose common institutional arrangements (*e.g.*, land revenue styles) and their inability to establish meaningful functional interrelationships among regional economies. In economic terms it was the latter that is crucial to the point I am making.

growing economic integration, inter-regional specialization and division of labour. These developments linked Eastern, Western and Mediterranean Europe in continually more meaningful ways. As well as a trade in luxury products, this required a traffic in basic raw materials and manufactures.[76]

What then were the factors that kept India economically fragmented? Why was it not possible to achieve the regional specialization and division of labour on which all-India economic unity had ultimately to rest? Stating the answer baldly, the relative cost of transport and communication was so high that economic activity and relationships necessarily had to be confined within quite narrow limits. Most interregional traffic consisted of high value commodities, and these moved in response to the demands of a very small class of wealthy consumers.[77] We must recognize that even evidence of absolutely large amounts of cargo being moved in Mughal times must be related to both the vast population and the distances the cargoes moved. Not only were the *per capita* effects of this traffic small, but the ton-mileage that it generated (the meaningful measure) had to be very low.[78] The transportation-communication cost constraint had its obvious effects on the structure of interregional economic relationships as well as on the character, distribution and application of political power. From this, too, flowed certain important economic consequences.

The imperial center could increase its revenues and thus finance the extension of its authority in two ways, by expanding the tax base in the areas it already held or by bringing new areas under its

76. This is not the place to discuss the enormous differences between the European and South Asian trade patterns, important though they are as pointers to basic differences in patterns of development. For the European system, the reader can refer to *Cambridge Economic History of Europe*, Vol. II, edited by M. Postan and E. E. Rich (Cambridge, 1952); and to Hobsbawm, *op. cit.*

77. *Habib*, pp. 61-75, particularly pp. 64-65. Even the traffic in wheat which because of sharp declines in transport costs became a widely consumed commodity during the past century was in Mughal times probably a luxury commerce. See Habib's description of the trade of Malwa and Agra-Delhi. (*Ibid.*, p. 91).

78. Some idea of this latter point can be obtained by calculating the carrying capacity of a bullock or a boat and the number of trips that could be made annually. Cf. A. P. Usher, *A History of Mechanical Inventions* (Cambridge, Massachusetts, 1954), pp. 7-8, and W. Bowden, M. Karpovich and A. P. Usher, *An Economic History of Europe since 1750* (New York, 1937), pp. 4-5. My commentators have paid almost no attention to the implications of the exceptional burdens imposed by Indian climate and geography. This was an important theme of my original essay.

control. To increase the revenue potential of its core area required investments in technological innovations that would raise productivity and this was not forthcoming for reasons I will shortly identify. The only other alternative was the extensive solution, bringing more revenue-yielding territories under its control. However, given high transport-communication costs, the burden of getting and holding territories beyond the core quickly exceeded the revenues derived. Viewed in this way, warfare and continual political instability were inherent in the system.[79]

The Mughal empire, whatever its ambitious grandeur, was unable to establish an effective central bureaucratic structure that did not have adverse effects on the levels of economic performance.[80] Even in the heartland of the empire the overwhelming bulk of the territories had to be administered via the *mansabdar-jagirdar* device. Habib suggests (pp. 271-272) that except for a very brief period during Akbar's reign, at least 80 percent of the Empire's territories was always assigned.[81] Assignment cannot be interpreted merely as a convenient administrative form. The subcontracting of power via the *mansabdar-jagirdar* institution was not the equivalent of administrative decentralization in the modern state. Rather it expressed the essential technical and economic weaknesses of the system, and from it there flowed certain economically significant consequences.[82]

The Mughals employed the *mansabdar-jagirdar* device because there was no other available to it. However, this institution unless

79. There are other systematic features which tended to encourage political instability and warfare in South Asia, but it is not necessary to consider them here. Suffice it to say that my view is not inconsistent with Habib's *judgement, (pp. 319 ff)* although he does not stress my particular theme.

80. In one important sense, it is possible that Raychaudhuri and I agree on the broad theme that the very success of the Mughal empire as a system of political integration worked against its success as a system of economic development (See his concluding paragraph, *Enquiry*. p. 121. It also seems to be Habib's view, pp 317 ff.) I suspect that our basic point of disagreement derives from my judgement that the economic and institutional consequences of the Mughal period, not easy to expunge from the system, played a very important role in the 19th and 20th centuries.

81. This figure seems to exclude the many areas in which the Mughals were able to establish only more or less casual tributary arrangements with largely self-governing satraps. In these quite large parts of the sub-continent Mughal rule apart from the occasional drain of tribute had insignificant economic effects. (See *Habib*, pp. 182 ff.) I am forced to ignore the implications of this fact.

82. Raychaudhuri, *Enquiry*, p, 102 ; and *Habib* Chapter VII.

properly controlled quickly built up centrifugal tendencies as the permanently sited *jagirdars* invariably established local power bases, and this of course threatened central political control. The shrewd administrative solution was a system of temporary assignments. Even in the central regions *jagirs* were rarely held for more than two or three years. "Without any long term interest in the territory assigned to him for a short period, the *jagirdar* unhesitatingly fleeced the peasant." The Mughals recognized this extreme threat to the economy, but the administrative need to maintain centralized political control made it impossible for them to protect the vitality of the economy.[83]

Having identified certain basic contradictions in the political system which countered high level economic performance, I will now examine the situation in a slightly different way.

Whatever else we can say about the Indian economy, it is agreed that the overwhelming bulk of its economic activity was agricultural. This is seen in the Imperial budget's dependence on land revenues. Also, the lack of economic specialization in the countryside implied a very limited amount of rural non-agricultural activity beyond the traditional self-contained village type.[84] Habib stresses and Raychaudhuri agrees that land revenue and other fiscal devices stripped the peasantry of all but the barest subsistence.[85] Assuming that this description is accurate, it follows that the actual cultivators, the vast mass of the rural population, were too impoverished to save and hence were unable to supply from their own resources any direct investment that would increase productivity.[86] Moreover, such a system must certainly have undermined the cultivator's incentives to invest even if resources could have been mobilized by them.[87]

83. *Habib*, pp. 260 and 317 ff. ; and Raychaudhuri, *Enquiry*, pp. 102-103. Great towns, specifically their markets and the ports, were frequently assigned as *jagirs*. (Habib, p. 259). Urban economic activity and potential economic expansion must thus also have been adversely affected.

84. Raychaudhuri, *Enquiry*, pp. 118-120.

85. *Habib*, pp. 190 ff. and 319 ff. ; and Raychaudhuri, *Enquiry*, pp. 119-121.

86. Too much has been made of the Mughal tax system's responsiveness to the needs of the peasantry during famines. *Habib* (pp. 249-251) has serious doubts about the meaningfulness of tax relief.

87. Raychaudhuri (*Enquiry*, pp. 117-118) recognizes that there had to be structural incentives to invest and innovate. Most writers tend to neglect this important point when they attempt to describe the dynamics of the system.

Economic growth in the sense of *per capita* output increases required investment. Capital, not being provided by the cultivators, could come only from the State or from non-cultivator groups.[88] Did the State itself invest to raise agricultural productivity? The only major direct investment outlet available to it was in irrigation facilities, and *Habib* (pp. 255-256) expressly denies that the State provided these.

Did the state then indirectly stimulate investment via revenue concessions and *Taqavi* loans to private entrepreneurs? *Habib* claims (pp. 252-253) that "Revenue concessions were the chief instrument devised by the Mughal administration to encourage development...." Inasmuch as this device required that capital be already in the hands of the risk-taker, groups other than direct cultivators had to finance the efforts. *Taqavi* loans came from the State as well as from local rulers. Here again, apart from the question of how extensive was the use of *taqavi* loans, they could not have served as great incentives to actual cultivators because they inevitably resulted in higher tax burdens. Raychaudhuri thinks more of its general significance than I do, but still he does not make the case that it served as a direct incentive to the actual cultivator. The benefits seem to have flowed to the zamindars and local moneylenders. In other words, these State-supported techniques would have generated incentives only for various intermediary groups rather than for the peasantry.[89]

88. There is in the literature a profound ambiguity associated with the concept of the peasant, a confusion which permeates not only the discussions of the Mughal era, but of the British period as well. I have not the time to explore this problem. I use the term "peasant" to refer to the direct cultivator and use other terms to refer to superior groups.

89. *Habib*, pp. 253-255; Raychaudhuri, *Enquiry*, pp. 116-117. Raychaudhuri attempts to qualify the view that all gains went to the middleman, arguing (*Enquiry*, pp. 118-120) that in some circumstances the cultivator also benefitted and thus had incentives to innovate. But the conditions under which this could occur necessitated that the cultivator have both direct access to a growing urban market and the ability to escape from the monopsonistic and monopolistic controls of the merchant-moneylenders who typically were in the position to capture all gains via the price paid for the crop and the price charged to advance working capital. When clearly set down, it is obvious that these conditions of freedom could have served as an incentive to only an infinitesimal portion of the cultivators. Professor Raychaudhuri's reference to the expansion of tobacco cultivation as proof of large-scale peasant responsiveness to a price incentive is unsatisfactory. Apart from the fact that we do not know how large an acreage was actually involved—I suspect it was quite small—we also do not know whether the stimulus for its cultivation came from incentives working directly on the peasant or via groups higher up in the social structure.

These intermediary groups, standing between the direct cultivator and the State and disposing of that proportion of the agricultural surplus not taken by the State itself, raise a host of difficult and as yet unsolved problems. Despite uncertainties, we can suggest a few of the important features that bear on the question of incentives to invest. Did any of these groups have strong, systematic incentives that would stimulate investment which in turn would make the system more productive.[90]

We are agreed that *jagirdars* had no interest in either local well-being or investment for economic expansion. Under their "despotic irresponsible authority the condition of the peasantry was probably the hardest and the incentives to production correspondingly the lowest...." But what is more important is that the *jagirdars* were the decisive intermediary element because they disposed of "the bulk of the agricultural surplus" in the economy.[91] In other words, this group with the greatest control over resources except for the State itself had absolutely no incentive to employ these resources in agricutural investment.

The other major group with access to investable resources was the "autonomus chieftains," the zamindars. It has been suggested that because of their hereditary links with an area they had some stake in the long-term prosperity of their localities. Not only has this view been challenged by Nurul Hasan and Irfan Habib, but the evidence indicates that the group was far less important in the disposition of the agricultural surplus than the *jagirdars* and the State.[92]

The only other group worth considering is the local merchant-moneylender. Whatever other roles this group played in the economy, there is little to suggest that merchants contributed meaningful amounts of capital for long-term investment in land development. Their gains came mainly out of arbitrage operations, from the differences between the prices at which crops were obtained in

90. This incentive problem must be distinguished clearly from the obvious concern each group had in obtaining more income. I am trying to determine whether there was any systematic structure of incentives that tended to encourage these intermediary groups to accumulate capital and invest in agriculture.

91. Raychaudhuri, *Enquiry*, p. 111.

92. Raychaudhuri, *Enquiry*, pp. 106-107, 111; and *Habib*, pp. 167 ff.

the countryside and the prices at which they were sold elsewhere.[93]

We can say by way of summary that the Mughal agrarian structure was such that the actual cultivator had neither capital nor incentive to invest and innovate. The State, whatever its interests in long-run agricultural improvement, was unable to furnish the capital from which expansion could come. The intermediary social groups who did possess the resources were locked into a system which destroyed all or virtually all incentives for investment in agricultural improvement.

I am not implying that the State and the intermediary social groups had no interest in expanding their revenue, but since the system did not provide this through capital investment and increasing productivity, the only alternative was territorial expansion. This meant forceable expansion that engendered constant conflict both on the Imperial frontiers and among local chieftains. If military power defined the size of the revenues that a chieftain had at his disposal, then the resources he squeezed out of the rural sector were plowed back into increasing the supply of armed retainers, the most profitable of all aristocratic investments.

Not only did the social structure and the way it was articulated inhibit investment in agriculture, but the system of warfare and political instability had also directly adverse effects on the average level of agricultural performance. Professor Raychaudhuri (p. 82) explicitly denies this, claiming that there was no significance to the waxing and waning of cultivated area and the forced migration of population. I think he misses the importance of fixed capital in agriculture. The productivity of a peasant's land depends not only on his immediate labour and investments in cattle and equipment but also on the immovable improvements that he has embodied in his plot—the clearing he has done, the wells he has dug, the water channels he has cut, the house and animal shelters he has built. If he is forced to move, all this embodied labour (his investment) is lost. (This ignores the probability that warfare and migration may also cost him his animals and his equip-

93. Urban merchants appear to have been the richest merchant groups on the subcontinent. They seem to have had neither interest nor ability to penetrate and establish control over the rural sector. This is a dicisive, functionally critical difference between Indian and Western merchant groups. (I am indebted to Dr. Ashin Das Gupta for making this point clear to me.)

ment.) Before the land to which he flees can be made equally fertile, the same investment has to be embodied in it. Thus warfare and political instability had in the form of capital loss a directly adverse effect on agricultural investment. There would not only be serious capital wastage but endemic political instability could also produce serious disincentive effects. The cultivator is likely to be reluctant to commit himself to these kinds of investments. His productive performance therefore would be at a much lower level than it would under conditions where peace and political order was certain. Evidence from the Mughal era is consistent with this argument, emphasizing as it does the peasant's very limited investment in either fixed or working capital.[94]

In the course of arguing for relatively high and growing agricultural productivity in the Mughal period, Professor Raychaudhuri suggests (p. 83) that it would be plausible to assume "a very slow but steady upward trend in population." This statement opens up a most important issue about which, as he says, our ignorance is almost complete since the only datum is the Moreland and Kingsley Davis all-India estimate of 100-125 millions for 1600. Most scholars who work on the 19th century accept that there was a population of roughly the same magnitude in 1800. However the question of what happened to population between 1600 and 1800 still remains. If we accept Raychaudhuri's surmise of an upward trend in the Mughal era, we would probably have to argue for a fairly substantial population fall in the 18th century, and this would raise doubts about his suggestion (p. 81) that the 18th century was prosperous. One, of course, could argue that the population growth of the Mughal era was wiped out by the Bengal famine of 1770-71. However, given that Bengal population constituted a relatively small proportion of the all-India total, that catastrophe could not have wiped out Mughal gains unless population growth had in fact been very slight.

The alternative possibility is that population did not grow but rather that in the Malthusian fashion it fluctuated within a more or less constant range. But a case for a relatively stable population leads to an interesting question. If it is true that at existing levels

94. *Habib*, pp. 116-117.

of technology virgin cultivable land was easily available in the Mughal era (TR, p. 82), why did population not grow swiftly during the period? The explanation would have to be found in the institutional structure that inhibited investment in any significant extension of cultivation. Such an analysis would be consistent with my emphasis on the influence of both socio-political structure and endemic political instability.[95]

Obviously, with the evidence still lacking it is impossible to make a firm case one way or the other. However, Professor Raychaudhuri has done a useful service by pointing to the possibility that analysis of demographic behavior in the Mughal era would yield important insights into the economic history of the period. Clearly, scholars should begin to examine this problem.

Having already briefly elaborated my reasons for stressing the relatively limited importance of internal commerce in the Mughal period, I will now point out a few of the difficulties that seem to be inherent in Raychaudhuri's views on the subject. For example, there is the fundamental ambiguity implicit in the concept of a monetized Mughal economy. This leads him (pp. 84-87) and many others to imply that the entire system generated an effective exchange of values that was based on regional specialization and division of labour with all the institutional characteristics thereby required. Yet at the same time Professor Raychaudhuri recognizes (p. 86) that the "major limitation of the internal trade was its character of one-way flow from rural to urban centers." In other words, except within the urban sector itself this traffic possessed the character of tribute rather than commerce. In fact, Raychaudhuri elsewhere points out (*Enquiry*, p. 119) that

> The relationship of mutual exchange between towns and country which developed in Europe in the later Middle Ages and its consequences in terms of specialization and division of labour are absent from the Indian scene.

95. Raychaudhuri is probably correct when he points (pp. 82-83) to new lands that were being continually colonized during the 17th and 18th centuries, but he ignores the tendency for other land to go out of cultivation. *Habib* (p. 326) doubts that there was any significant net expansion of cultivated land during the Mughal era.

This uncertainty about the role of money and monetization in the Mughal economy also leads to misinterpretations about the nature of the urban sector. It seems to me that the difficulty arises out of a tendency to confuse what the geneticist calls phenotypical similarities with genotypical similarities. Finding trade, urban centres, money and monetary transactions, he concludes—erroneously, I feel—that these activities and institutions played the same functional role in the South Asian system as they did in the early stages of capitalist development in Western Europe.[96]

For example, Raychaudhuri leaves us with a wrong impression when he writes (p. 87) of "the influence of the Surat merchants over Mughal commercial policy" and stresses "their ability for successful combined action against the imperial bureaucracy." Even in the coastal regions where foreign trade was important, the urban elements had not the organization nor were they embedded in a system that yielded an independent political power which was capable of challenging the authority either of the Mughal State or the regional landed aristocracy.[97]

The location and survival of towns over most of the subcontinent was determined primarily by the presence either of a pilgrimage center or a court and its bureaucratic apparatus. Few towns developed an independent vitality that was based on intimate commercial interdependence with a large rural hinterland.[98] Probably, the Gujarat in Mughal times was the one region whose somewhat urbanized economy was not entirely dependent on the continued

96. Professor Raychaudhuri is aware (*Enquiry*, p. 118) of the danger inherent in the unsophisticated use of terms like "monetization" and "money economy." But in the absence of adequate available research, he has tended to make more of the limited evidence than is warranted.

97. For an interesting example of the weakness of urban merchant groups, see A. Das Gupta. "The Crisis at Surat 1730-32," *Bengal Past and Present*, Diamond Jubilee Number, Vol. LXXXVI, Part II, Serial No. 162, November, 1967, pp. 148-162.

98. Abdual Karim's study of Dacca (*op. cit.*) is not inconsistent with this interpretation. The rise of Dacca is associated with the shift of the capital from Rajmahal. Both Rajmahal and Sonargaon decline as a result. Dacca's economic significance begins to decline when the court and bulk of its administration moved to Murshidabad after 1700. My only question about Karim's analysis is his underestimation of the post-1700 population decline that probably occurred as a result. I suggest that the sharp fall in population did not occur after 1765, as Karim assumes, but rather that it began, probably, in the early 18th century.

presence of courts and bureaucracy.[99] Yet even here there is no urban bourgeoisie capable of establishing and sustaining an independent power base. The great merchants of Surat, being unable to provide themselves with an independent military power, were very vulnerable to bureaucratic pressures. Thus, they could not defend themselves as a collectivity either against imperial demands or the pressures of regional rural-based chieftains.[100] Their inability to establish direct control over their rural hinterland explains at one and the same time both their political weakness and their ultimate economic insignificance.[101]

I have discussed summarily the basic weaknesses of agriculture, the absence of a system of mutual interchange between rural and urban sectors, the lack of regional specialization and division of labour and the weak role of the urban merchant class in the general structure of political arrangements. These features of the Mughal system were structurally and systematically interrelated with one another, and in combination they were responsible for a set of institutional arrangements that were not only incapable of serving as a preparation for subsequent economic development of a modern sort but probably were actively hostile to such a possibility.[102] These features carried over into the 19th century as India's inheritance, an inheritance that was not easily transformed nor easily destroyed. India, after all, was not a tiny barbaric society that could be quickly

99. It is a revealing commentary on the underdeveloped state of Indian economic history that there is no systematic study of the economic career of this major region. But soon this defect will be remedied partially. Professor Ashin Das Gupta is now preparing a major study of Gujarat commerce during the 17th and early 18th centuries. I am indebted to him for discussing his tentative findings with me.

100. Nor were they able, of course, to protect themselves against European aggressiveness. Ashin Das Gupta discusses aspects of these matters in his essay, "Trade and Politics in 18th Century India," in a volume entitled *Islam and the Trade of Asia*, edited by S. Stern and D. Richards (Oxford, forthcoming.)

101. Detailed studies of urban centers are needed desperately. Some years ago Professor Raychaudhuri presented a brief introduction to the problem. Tapan Raychaudhuri, "Some Patterns of Economic Organization and Activity in Seventeenth Century India," *Second International Conference of Economic History, Aix-en-Provence, 1962*, Vol. II. (Paris, 1965), pp. 751-760. I only hope he will explore more deeply the imaginative insights he presented there.

Lacking detailed work on India, there are useful notions to be derived from Rhoads Murphey, "The City as a Center of Change: Western Europe and China," *Annals, Association of American Geographers*, Vol. 44, 1954, pp 349-362.

102. For example, the comparatively low level of technological development, a phenomenon Raychaudhuri also recognizes (TR, p. 84), should be noted both as a separate fact and as a consequence of these other characteristics.

overwhelmed and changed from the outside. It was a great civilization and its weighty and impressive past has to be reckoned with. It cannot be dismissed as irrelevant to the possibilities and difficulties of economic development.[103]

With this in mind, I have emphasized the importance of having a realistic understanding of the pre-1800 era, and I have pointed to the unfortunate breach that exists between the work done by scholars on this period and the work done on the period after 1800. I believe that if we are to obtain a truer view of India's career in the 19th and 20th centuries we must not concentrate solely or even mainly on the British impact. Indian society had its own internal dynamics. It was not a static system against whose passive institutions the sledge hammers of European enterprise and power smashed more or less at will.

V

I have already responded to many criticisms of my post-1800 analysis. Let me now deal with some major issues that I have not examined yet. Given the present lack of precise knowledge, nothing will be gained by my pointing to one piece of inadequate evidence as an argument against another piece of eqully frail evidence. Instead I will continue to explore, as I have done so far, both the logical possibilities suggested by the data and the comments of my critics. In this way it will be possible to identify a variety of more or less specific research problems for which we need answers.

103. My argument may exaggerate the low-level equilibrium characteristics of the Mughal economy. *If so, the exaggeration derives legitimately from the emphasis* in Dr. Habib's book and from Professor Raychaudhuri's discussion in *Enquiry*. On the other hand, Raychaudhuri seems to be dissatisfied with the ultimate rigidity of Habib's interpretation and offers (*ibid.*, pp. 118-120) a tentative qualification to Habib's description. I pointed out in Footnote 89 above that within the general framework which Raychaudhuri accepts, there is little scope for such exceptions. Nevertheless, I share with him a sense of uneasiness about Habib's description of the Mughal economy. I suspect that Habib may overdo the view that the entire rural surplus was transferred from the peasantry and that there was no rural demand for urban products and very little commercial activity with the rural sector. Professor Das Gupta's forthcoming study of the Gujarat economy will show that urban foreign trade centers were commercially linked with the villages that produced export commodities. At the same time, villages were not entirely self-sufficient. There was a certain amount of specialization, particularly in the production of salt, iron and textiles. It remains to be seen whether detailed local and regional economic studies will yield a different interpretation than the one Habib has given us.

Population. My comments on the size and rate of population growth were not intended to imply a specific demographic theory. I treated population as an independent variable that was related to my conjectures about the growth of total output. As long as I was concerned to estimate only changing levels of *per capita* income, my procedure was legitimate. But Toru Matsui has correctly pointed out (pp. 22-23) that demographic behavior is functionally related to economic performance. Unfortunately, the precise nature of that functional relationship has never been established. Furthermore there is no satisfactory ready-made theoretical formulation that we can apply.[104]

Perhaps there will never be a precise detailed description of the relationships between population behavior and economic process in India. The unsatisfactory data on birth, death and age even after 1872 probably precludes the likelihood. On the other hand, with the evidence we do have we can do a great deal of demographic work that would throw light on post-1800 economic history. For example, we have no responsible ideas about the path of population behavior before 1872. A careful study of local materials should enable us to develop some reasonable estimates of population movements 1800-1872.[105]

We also need much more work on the period covered by the decennial censuses. The volume by Kingsley Davis barely introduces the task. For example, the shift from the slow rate of population growth 1872-1921 to the increasing rate after 1921, demands detailed explanation. I agree with Mr. Matsui that probably the pre-1921 phenomenon is associated with the slow development of the economy as a whole, but I suspect that the connection is a rather complicated one.[106] I earlier suggested that slow population growth may well reflect the possibility that British rule really had a much more limited overall impact on the Indian economy than has been previously assumed. The problem is complicated because a notable feature of the whole post-1872 period is the fact that despite extreme fluctuations in the growth rate between one census and the next there is not one decade in which an absolute decline of population occurred. This differs not only from the typical Malthusian pattern associated with traditional societies, but it also seems to be different from the situation in India before 1750-1800. The apparent transformation of population behavior seems in itself to suggest that expansionist economic forces were taking hold.[107]

It is possible that we are observing the phenomenon of a complex demographic changeover. Masked by the sheer absolute size of the population, the response could have expressed itself statistically in two phases, the first being in a suppression of absolute population declines and the second, after 1921, showing up in a more rapid rate of growth. The various possibilities can be tested only by detailed regional and district studies. Differential rates of population growth could then be linked to variations in local economic performance.[108]

Having said all this, I now raise an issue that may force us to reconsider the demographic problem in quite a different light. Kingsley Davis' adjusted figure of 255 million for the population of India in 1872 can be taken as reliable. However, it raises serious doubts about the validity of the commonly accepted estimate of 100-125 million people for 1800. The 125 million figure requires an average annual growth rate of 1.0 percent and the 100 million

104. A great deal of work has been done on the relationship between population and economic activity in Britain in the 18th and 19th centuries. The current uncertain state of that debate is proof of my point.

105. I must mention the important work being done by D. and B. Bhattacharya on pre-1872 census materials. *Census of India. 1961. Report on the Population Estimates of India (1820-1830)*. Edited by D. and B Bhattacharya. (Delhi: Government of India, Ministry of Home Affairs, Office of the Registrar General, India, 1963.) I urge that Indian scholars give all possible support to this enterprise so that it can be rapidly completed.

106. After all, if slow population growth before 1921 was caused by slow economic development, how do we explain the increasing rate of population expansion after 1921? Medical innovations do not seem to be the relevant cause before 1947.

107. The study of pre-1872 data would enable us to determine when the traditional Malthusian pattern of growth and absolute declines actually came to an end.

108. I still find it impossible to understand the mechanism implicit in Bipan Chandra's presumption (p. 44) that population could be steadily growing not only after 1872 but probably before that if the standard of life was steadily declining as he seems to suggest. It may be possible to find the combination of increasing population and declining *per capita* income persisting for a decade or two, but it strikes me as inconceivable in the South Asian situation that such a phenomenon could persist for a century or more. Possibly, I have misinterpreted his use of the phrase "economic stagnation and increasing misery." It may be that he does not intend the phrase to mean an actual decline in the average standard of life. In that case, our disagreement could be quite modest. But then I would urge upon him the desirability of using more neutral and precise language to describe what he does mean.

figure needs a growth rate of 1.3 percent for the period 1800-1872 in order to produce 255 million people at the end of that time.[109] Population growth rates of 1.0 to 1.3 percent per annum seem excessively high for the 19th century. After regular censuses began such growth rates were not achieved until the 1921-1941 decades. It is possible that population grew very rapidly 1800-1872, slowed down in a startling way 1872-1921 and then rose again in the subsequent decades. This strikes me as an unlikely pattern unless one assumed that the 1800-1872 political and economic changes were far more dramatic and benevolent than any of us have been willing to consider.[110]

The alternative is that the population in 1800 has been grossly understimated. If we project the annual average growth rate for 1872-1921 (0.4 percent) back to 1800, it would yield a population of 192 million for that date, almost double the figure derived from Moreland and more than half again as large as Kingsley Davis' estimate. And if we assume that even the 0.4 percent growth rate is too high for the pre-1872 period, the 1800 population figure would have to be even larger.

The accepted estimate for 1800 based on Moreland has always been a recognizably tentative figure. Davis' revision involved only the most cursory tampering with the Moreland figure. My estimate, for all its weakness, seems to be derived in a much more realistic manner. Without exploring all the assumptions, it would be safe to start examining the possibility that India's population in 1800 was of the order of 200 million rather than in the 100-125 million range.[111]

Clearly, a population revision of this magnitude would force a rethinking of a good many accepted ideas about the 19th century. Moreover, if my suggestion is reasonable, we must return to Professor Raychaudhuri's proposition (p. 83) that population may have grown somewhat during the period 1600-1800, but I do not think

109. All the figures I am using have been crudely calculated and provide only approximate orders of magnitude.

110. I am not suggesting that the possibility of such swift population expansion is beyond consideration. Despite famines and epidemics, a great many 19th century descriptions gave the impression of very rapid growth.

111. So frail is the state of our field that this important demographic question has remained almost entirely unexplored.

that we can start with Moreland's figure of 100 million for 1600. I agree with Kingsley Davis that Morland's method produced an excessively low estimate; however, I am not satisfied that Davis raised it enough. Without going to details, it would certainly be reasonable to raise our estimate of population at the beginning of the 17th century above 125 million. But even this implies population growth between 1600 and 1800.[112] Such a judgement immediately runs afoul of Habib's cautiously established conclusion that there was no significant net increase in land under cultivation in the 17th century. It is possible that later research will show that Habib is wrong. Assuming that he is right, does this then mean that there was population growth in the 17th century but that it was made possible by innovations that increased the productivity of the relatively stable cultivated area? Or does it suggest that popugrew very rapidly in the 18 th century? Or does it means that population in 1600 was 200 million rather than 100-125 million? This is not the place to examine these matters. Obviously, with nothing clear or certain, it will take much careful research to shed light on these issues.[113]

Agriculture. The distinctive feature of most work on the rural sector has been a preoccupation with land tenure and the formal aspects of the settlement process. Whatever significance such studies may have for other kinds of history, they tell us nothing about changing patterns of agricultural organization, output or productivity. Yet if economic historians are to be able to say anything at all about the functioning of the Indian economy these certainly are the most important processes that have to be investigated. Lacking such studies, my commentators and I have had nothing solid on which to base our discussion.[114]

112. Assuming various population sizes in 1600-100, 125 or 150 millions—and a population of 200 million in 1800, average annual population growth would have been 0.35, 0.24 or 0.14 percent over the two centuries.

113. Of course, if we can show that population did grow very rapidly between 1800-1872, this particular Mughal demographic problem vanishes.

114. Some studies of land tenure and revenue policy seem to assume that they are saying something about the effectiveness of specific tenurial and revenue arrangements on agricultural performance. In fact, they are only identifying certain distributive features of the system Until studies specify with some degree of

traditional irrigation system fell into disrepair, a fact that reduced the net gains from new investment. Actually, my brief statement (p. 8) was intended (perhaps too cavalierly) to summarize the net consequence of deterioration and improvement. I suggest also that the growth of private investment in wells and small irrigation schemes may possibly have offset the decline that can be identified.[119] Nevertheless, I recognize the potential significance of Raychaudhuri's point. The disagreement reflected in our different emphases can be resolved only when scholars have produced detailed studies that deal with the issue.

This leads me to a point where I think that possibly my commentators have gone astray. Both Bipan Chandra (pp. 45-50) and Professor Rachaudhuri (pp. 91-92) have claimed that in 19th and 20th centuries there was no technological change in Indian agriculture. Not recognizing that changes in the organization of agricultural operations (which I would also call innovations) can produce rather significant productivity gains, they seem to have identified technical change solely with visible mechanical and scientific innovations.[120] In my reading of local 19th century records I have been struck by evidence which implies that such organizational transformations were going on in many districts of the subcontinent. Possibly, the very growth of population and markets contributed to changes in the way that land was technically administered and thus without requiring any visible alteration in equipment to a development of a more intensive and productive agriculture.[121] Unfortunately, I depend here on impressions gained from rather unmethodical reading. Whereas there are studies concerned with the most detailed aspects of land settlement policy debates, there is not even an introduction to the post-1800 changes in the organization and conduct of village agricultural activity. Nothing has been written

119. See, for example, Ravinder Kumar, "The Rise of the Rich Peasants in Western India," in *Soundings in Modern South Asian History*. Edited by D. A. Low (Canberra, 1968) pp. 54-55.

120. One example of a major change that did not require any visible machanical or scientific innovations is the shift from two-field to three-field agriculture in parts of medieval Europe.

121. See the possibilities suggested by Esther Boserup, *The Conditions of Economic Growth* (London, 1965.) As one example of what I have in mind, there seems to have been a systematic and fairly widespread shift from various forms of slash-and-burn agriculture to much more intensive types of cultivation.

on how actual production decisions were made in villages, on how changes in crop patterns occurred, or on how these induced or required changes in the way agriculture was carried on.[122]

Another issue on which our ignorance is virtually complete but to which enormous causal significance is attached is the problem of "commercialization" of agriculture. It usually is argued that 19th century commercialization and its consequence in the form of regional specialization were forced upon the cultivator by the requirement that land revenues be paid in cash. However, the argument is seriously weakened by the fact that similar demands in the Mughal era seem not to have produced equivalent effects. As Professor Raychaudhuri noted, the references to "commercialization", "monetization", and "specialization" have not been precise, nor have they contributed to an understanding of post-1800 developments. Even assuming that we know the meaning of these concepts, we still lack knowledge of how the processes of monetization, commercialization and regional specialization occurred.[123]

Furthermore, it is rather startling to me in this context that no one has examined the relation of the price mechanism to historical changes in agriculture. The relevance of price to economic behavior is usually ignored. Yet it seems probable that changing price relationships rather than a necessity to pay land revenue in cash was primarily responsible for the changing pattern of agricultural performance after 1800. For example, there is some evidence that the rise in cotton prices following the outbreak of the American Civil War in 1861 stimulated some regions to shift more resources into cotton cultivation. This in turn increased the demand for (and the price of) food grains and hence encourged other districts to specialize in food grain production. But again we lack solid published

122. A recent study by V. H. Joshi, *Economic Development and Social Change in a South Gujarat Village* (Baroda, 1966) has just come into my hands. It is virtually the first detailed attempt to describe this process historically. Dr. Joshi's study also throws considerable light on the process of commercialization and specialization. I am indebted to Mrs. D. P. Pandit for bringing the book to my attention.

123. Raychaudhuri (p. 91) makes an interesting reference to Gadgil's observation that famines were responsible for setbacks to commercialization and specialization. Toru Matsui (p. 19) also refers to the blurred and fluctuating borderland between the commercial and subsistence sectors. This again points to the fact that we are attempting to describe not a simple unilinear process but an extremely involute one.

In Part III I alluded to the unsatisfactory handling of data in many cases. Let me now identify one or two other issues that have suffered from misinterpretation. For example, many scholars by referring to arrears in land revenue, the "unchecked and continuous growth of indebtedness and the general and growing ubiquity of the moneylenders..." seem to believe that they have dismissed either the fact or the significance of increases in agricultural output. (BC, p. 48). Apart from the absence of careful studies on any of these matters, problems arise from the way the available evidence has beeen handled.[115] Most evidence on land revenue arrears, land loss and indebtedness, has been taken from easily accessible reports that were prepared during or immediately following periods of extreme agricultural distress. There is a tendency to imply that the data show a cumulative effect between one period of distress and the next. To take figures on indebtedness or land loss from one famine period, then add them to figures resulting from another famine some years later and then to use the total as proof that the situation became that much worse is not valid unless it also can be demonstrated that during the intervening years the trend was not reversed.[116] It is very probable that during the intervening period of recovery much land was repurchased and much debt was repaid. The fluctuations between periods of prosperity and depression are precisely the features that are fundamental to our understanding of both the rural sector and the Indian economy as a whole. In other words, our concern must be with fluctuations over time and variations among regions as well as with long run trends.[117]

Even if we are able to identify a steady upward trend in the absolute amount of rural indebtedness, we still would not have said anything particularly useful about the condition of the agricultural sector. One would expect that increasing commercialization would be accompanied by rising amounts and proportions of rural indebtedness. These would reflect operational changes in the economy. Nor would a rising rural indebtedness be automatic proof of a growing burden on the cultivators. As I pointed out in Part III, productivity also has to be taken into account. Neale's Muzaffarnagar data indicates that with rising productivivity an increasing debt may be consistent with a declining burden on income received.[118]

I am not saying that this in fact occurred either throughout the period after 1800 or in all regions of India. Such a case cannot be made (or disproven) because as yet scholars have not examined the problem in this way. What I am suggesting is that both for methodological reasons and because some evidence tends in this direction, we cannot dismiss the possibility without careful investigation.

Professor Raychaudhuri (pp. 91-93) offered a thoughtful critique of my views on 19th century agricultural performance. We agree that there was an upward trend in both total and *per capita* agricultural output, but we do differ about how substantial the upward trend might have been. His disagreement is certainly reasonable. He believes that I tended to undervalue the impact of negative features in the system. For example, I emphasized the expansion of great irrigation projects and neglected to note that much of the

precision the relationship between tenure types and actual levels of economic performance, we are not very far along. For example, despite the enormous attention given to ryotwari and zamindari arrangements, no one has attempted to determine whether there is any relationship between them and a particular pattern of performance. The recent important historical studies by George Blyn, Dharma Kumar and Dharm Narain suggest that the intellectual drought may now be coming to an end.

115. The comments which follow have been made somewhat more elaborately in Morris, *IESHR*, March 1966.

116. It is even possible that many historians have been impressed by the wrong data. For example, Neale, *op. cit.*, p. 161 reports that in the United Provinces the actual sales of property for arrears of revenue as distinguished from orders of distraint of property were insignificant not only in normal periods but even in the great famine year 1877-78. In that year there were only seven outright sales of property for arrears in the Northwestern Provinces and two in Oudh. As Neale concludes, such figures for a region larger than Britain in size and population do not seem excessive.

117. Forthermore, in this context there is need to study the specific demographic consequences of famine and epidemic. We know nothing about socio-economic responses to these catastrophies. What were the effects on migration, on socio-economic structure, and on the actual performance of the economy ? What happened to rural economic activity, to prices, to wages ? How were commercial and industrial behavior affected by such dramatic changes in agricultural performance ? These are a few of the issues that clearly need examination. D. R. Gadgil, *The Industrial Evolution of India in Recent Times* (4th edition, Calcutta, 1942), noted the importance of agricultural fluctuations on the performance of the entire economy. But he did not trace out the implications of the agricultural production cycles in detail and no scholar has attempted to follow up his leads. B. M. Bhatia, *Famines in India* (Bombay, 1963), does not deal with these issues.

118. If my argument has any validity at all, it could not automatically follow that because of rising indebtedness there was "no continuous or general increase in rural savings or investment . . ." (BC, p. 48).

research into these themes of agricultural price history.[124]

Professor Raychaudhuri (pp. 92-93) has emphasized the depressing effect of rural property relations on agricultural performance. I would not disagree with his generalized explanation of why output growth was slower than it might otherwise have been, but I disagree somewhat with the causal implications that I detect. I would lay greater emphasis on the persistence of traditional organization. If he is suggesting that because they were not harsh enough the British did not entirely disrupt the traditional structure of rural economic activity, I would agree. Such an interpretation would be consistent with my emphasis on the tenacity of traditional institutions, and it would fit with my genetic approach. It emphasizes that the task of agrarian reorganization that was necessary to raise the average level of responsiveness to new productive possibilities was extremely complex.[125]

Our differences are probably modest, but in the interest of encouraging new research I would prefer that certain specific problems not go unmentioned. For example, Raychaudhuri notes the role of tenurial system in affecting agricultural performance. I have already pointed out that there are no studies which attempt to relate different systems of tenure with variations in levels and rates of output. It is quite possible that there were no significant correlations, and if this is true, scholarly preoccupation with land policy would yield no information on the actual functioning of the agricultural system. After all, there was expansion of total output throughout the 19th and 20th centuries; regional specialization and shifts to higher value crops did occur. Agricultural history desparately needs detailed local studies of the specific relationships between systems of tenure and these changing types and rates output[126]

124. I am indebted to Dr. Peter Harnetty for allowing me to look at some material he has been working on. However, he should not be held responsible for my interpretation. Joshi's *op. cit.*, is suggestive on this theme. And Dharm Narain, *Impact of Price Movements on Areas under Selected Crops* (Cambridge, 1965) deals directly with one aspect of the issue.

125. The British were continually preoccupied with maintaining both political stability and their power. Because of this their expanding concern after 1859 with legislation that would protect various classes of tenants ran counter to the possibilities that were inherent in a ruthlessly functioning market economy.

126. Here, as for all other types of economic studies, let me stress the absolute importance of moving beyond the limits imposed by official materials. Modern economic historians unfortunately have neglected the vast troves of private materials, particularly those in the local languages. Agricultural historians have failed to make use of rent rolls, farm budgets and account books, estate manuals, village records, local price data, merchant accounts, diaries and the like. The persistent failure to seek out and use these kinds of materials is particularly distressing because many of them are now being destroyed. It is urgent that Indian scholars mount a systematic effort to preserve and use these records. I would also suggest that greater imagination be exercised in determining the possibilities. For example, a great many students and scholars come from families who have preserved records of considerable economic significance. There is no reason why they should not be encouraged to select research problems that are based on their own family materials. From these records they could produce important case studies. One rare example is V. Krishnan, *Indigenous Banking in South India* (Bombay, 1959.)

Professor Raychaudhuri refers also to the imperfections of the capital market and to transport limitations that placed the producer at the mercy of the village *bania*. Clearly, imperfections existed but there has been a tendency—not necessarily Raychaudhuri's—to discuss the *bania* in entirely perjorative terms as an evil vulture battening parasitically on the economic system. Yet in the absence of a radically different system of rural credit, the local merchant-moneylender must also be seen as a necessary element in the functioning of the rural system. The combination of the *bania* and the institution of the *hundi*, two traditional carryovers, were not only necessary for the functioning of the agricultural sector but were also essential to the creation and expansion of rural-urban economic links.

This is not intended to be a defence of the *bania*. However, I must point out the typical emphasis on his monopolistic and monopsonistic role needs to be qualified. I already have intimated that for the post-1800 period the *bania* (together with the landlord) was not able to skim off and preempt all gains that might otherwise have gone to the producers. The incentives of relative price shifts and the influence of periods of high prices did filter down to the individual cultivator. This is generally supported by the changing crop pattern in the countryside and is specifically illustrated (to take one extreme example) by rural price behavior and peasant incomes in western India during the American Civil War. It is also supported by the apparent secular downward trend of rural interest rates in the 19th century.[127]

127. Morris, *IESHR*, p. 189 and footnote 13. A comparison with urban interest rates would probably indicate that rural rates were not excessively high. The real problems were not so much the monopsonistic position of the *bania* as the effective shortage of capital and the economy's inability to create efficient vehicles for mobilizing the resources that were available.

I am not arguing that given the distribution of income and power in the countryside and the imperfections of the market system the peasant received either the full impact of favorable incentives or the facilities (resources) to respond to them. I am suggesting that the role of the *bania* needs to be examined with greater care and with considerable caution. We need a much more sophisticated analysis of rural credit and commercial institutions than we have got so far. For example, even if we can demonstrate that the combined power of *bania* and landlord made it possible for them to capture the great proportion of rural surplus, it would still not automatically follow, as Raychaudhuri (pp. 92-93) and most others suggest, that allocation of their gains was utterly uproductive. This proposition and its corollaries must be tested by research.[128]

These comments on the need for more careful exploration of the entire structure of rural marketing, finance and investment lead me to identify two other major subjects for research: the relation between urban and rural sectors, and the nature of the rural non-agricultural sector. As far as I know, there are no detailed historical studies of the impact of urban growth on the countryside. Nor are there any historical investigations of the impact of the countryside on the demand for urban products. To take an obvious case, on one has studied the effect of the rise of Bombay city on the demand for foodstuffs, raw materials, labour and capital in the economy.[129] Similarly, no attempt has been made to link the fluctuating performance of Bombay's expanding commerce and industry changing to the conditions of rural markets on which

128. My doubts about the Classical interpretation were generated by certain aspects that I perceived to be paradoxical. For example, there is a widespread belief that increasing peasant indebtedness led to land loss and the transfer of land ownership into the hands of a small class of moneylenders. If true, a century of the process would lead us to expect an extremely high degree of land ownership concentration and a very high proportion of tenancy. But available evidence at the beginning of Indian independence does not support our expectations. (Morris, *IESHR*, June 1966, pp. 190-192). Moreover, the view of parasitic *banias* and landlords ignores the widespread expansion of the sophisticated rural marketing system after 1800 and fails to ask who provided capital to make it function.

129. Joshi, *op. cit.*, describes the changing output of his village and the transformed system of labour utilization as a response to demand from Bombay. But this is the only detailed study I know of.

Bombay depended so extensively.[130]

Economic historians have failed also to attach significance to the rural non-agricultural sector. Rural activity has been equated almost exclusively with agriculture. There are impressionistic, monochromatic and largely unsubstantiated statements about the economic impact of railways or the decline of traditional handicrafts. There is, however, not a single detailed investigation that attempts to trace the impact of cheapened transport and vastly expanded traffic on industrial and commercial developments in the countryside.[131] Nor are there studies that examine the rural impact of coal mining, raw material processing (e. g., cotton ginning, sugar refining flour milling) metal working, construction, or the extension of merchant activities.[132]

Handicrafts. Despite the importance that scholars attribute to the decline of the traditional handicraft sector and its influence on the entire economy, this is another subject where careful investigation has not supported the conclusions. In my original essay (pp. 8-10) I suggested that probably the Classical view had misinterpreted the fate of handicrafts as illustrated by textiles, its most important branch. Let me explain the origins of my reasoning.

My early reading in Indian economic history exposed me to statements about the "destruction" of the Indian handloom industry. I ecountered references to "the ruin," "the rapid collapse," the "almost total extinction," of the industry in the 19th century. "The ruin of artisans is an established theme . . ."[133] Such statements convinced me that two things happened in the 19th century: the absolute number of handloom weavers had been enormously reduced

130. S. D. Mehta, *The Indian Cotton Textile Industry* (Bombay, 1953) identified the connection but treated the problem in an impressionistic manner.

131. All railway studies I know of are concerned with railway policy, railway finance and rate structures.

132. Where, for example, are studies of rural entrepreneurs? No one has examined the extension of Marwari trade throughout North India in the 19th century, activities which culminate in their enormous industrial commitments in the 20th century. Who is studying the career of weavers who went on to merchant-manufacturing and then to great trading enterprises, or the cases of peasants who accumulated capital and then went on to nonagricultural activities?

133. These statements are all drawn from Bipan Chandra, *The Rise and Growth of Economic Nationalism in India* (New Delhi, 1966) pp. 56-57; and from his comments on my essay (pp. 41, 53, 57.)

from what they had been 1750-1800, and the absolute output of handwoven fabrics had fallen even more disastrously. From the catastrophic descriptions to which I was exposed, I expected to find only a few isolated pockets of miserable survivors.

However, in the late 1950's I encountered official statistics which estimated that in 1950-51 the number of active handlooms in India and Pakistan combined totalled 3,125,000 and the minimum direct employment on these looms amounted to 4,688,000 people.[134] These figures were startling. Given the descriptions on which I had been weaned, I might have expected estimates of 40,000 handloom weavers, possibly even 400,000. But 4.7 million handloom weavers struck me as something more than the miserable remnant of a once glorious industry. Assuming that there were the same number of weavers in 1800, when total population according to the Moreland-Davis estimates amounted to 100-125 millions and the labour force (calculated at 50 percent of that) could have been no more than 50-62 millions, I was confronted with magnitudes that seemed unreasonable. Without allowance for the great amount of complementary labour needed to keep them employed, weavers alone would have constituted between 7.6 and 9.4 percent of the total labour force.[135] If account is taken of all other non-agricultural employments in the economy and the likely level of agricultural productivity, it becomes unlikely that there could have been anything like 4.7 million weavers in the 18th century. In other words, it seemed probable that over the century-and-a-half after 1800 the absolute number of handloom weavers increased substantially. The argument in my original essay briefly sketched the mechanism that made possible the survival of the industry and expansion of employment

134. The figures are calculated from S. D. Mehta, "The Handloom Industry—A Restatement," *The Textile Digest*, Vol. XXII, No. 4, October-December, 1952, pp. 3-4; and the Report of the Fact Finding Committee on Handlooms (Karachi, 1956), pp. 13-14. Until I have completed my analysis, the specific figures cited must be considered very tentative. All that is important here is that they are of the proper orders of magnitude. This same caution applies to other figures used.

135. If we accept Kuznets' judgement (*op. cit.*, p. 74) that in traditional economies the labour force could have been as low as 30 percent of total population handloom weavers alone would have constituted between 12.4 and 15.7 percent of the total labour force.

without apparently reducing the net *per capita* income of weavers.[136]

Not only are early 19th century data extremely difficult to work with, but even the late 19th and 20th centuries statistical materials are filled with ambiguities and assumptions that make them almost incoherent. Inasmuch as this is not the place to describe either the nature of that information or the problems of using it, it will suffice to point to some of the types of data that are consistent with my basic view.[137]

First it seems possible that all-India *per capita* consumption of cotton cloth, allowing for cyclical fluctuation, rose over the period 1800-1947. The facts seem quite clear for the 20th century.[138] They are less so for the 19th century. However, some casual 19th century evidence seems to support the assumption, and there is also Habib's judgement (pp. 94-96), based on the Mughal evidence, that an increase in *per capita* consumption had occurred since that time.

Second, a large proportion of India's total available yarn—including most imported yarn and a great percentage of Indian mill yarn—was going to handlooms. In fact, as late as World War I handlooms consumed much more than half of all yarn available in the country.[139]

Finally, there is evidence at least for the late 19th and early 20th centuries that available yarn for handloom consumption was rising. Evidence for the interwar period is less certain. At worst, yarn used

136. If my earlier suggestion is valid that India's population in 1800 might have been 200 million, the impact of my argument based on weavers as a proportion of the total labour force is of course somewhat reduced. However, the reader must recognize that the paradox was only the starting point for my investigations. My case ultimately does not depend upon it. Independently, P. K. Nambkar, Superintendent of the 1961 Madras Census came to a similar conclusion that the absolute number of handloom weavers had increased since 1800. (See *Census of India*, 1961, Vol. IX, "Madras," Part XI-A; "Handlooms in Madras State," p. 3.) Mehta, *Indian Cotton Textile Industry*, p. 95, seems generally to agree with this view, at least for the period 1800-1914.

137. For readers who are interested in some of the fundamental statistical problems, Mehta, *Indian Cotton Textile Industry*, pp. 86 ff., succinctly discusses the assumptions that are used to build up yarn and cloth consumption statistics. Apart from the issues Mehta considers, there is also the vexing problem of defining a weaver.

138. Mehta, *op. cit.*, p. 167 estimates that between the end of the 19th century and the early 1950's, *per capita* consumption of cotton cloth rose by some 30 percent. Neither Mehta's figures nor any others include the apparent increase in the consumption of cloth made from other fibres.

139. Mehta, "Handlooms" p. 2.

by handlooms declined slightly, or, conversely, it may have increased significantly.[140] In the absence of significant technological innovations, increases in yarn consumption suggest either expansion in the number of weavers or increase in fulltime employment of weavers, or both.[141]

I do not pretend that my argument or my work to date has solved all the critical issues, but on the basis of both logic and evidence I think it is reasonable to claim that there is a case that is worthy of consideration.[142] A number of matters still need clarification and I want to say a few words about some of them. First, there is the problem of hand-spinners. A surprisingly large number of them survived well into the 20th century.[143] I, too, would have expected that the demand for their labour would have declined as competition with factory yarn intensified, but it is uncertain that in fact their absolute numbers or *per capita* income did fall. However, even if hand spinning did decline absolutely, it may well have been offset by the rise of alternative opportunities.[144]

140. *Report, Indian Industrial Commission, 1916-1918* (Calcutta, 1918), Statement II, pp. 396-397. This is supported by a different set of figures in Mehta, "Handlooms," p. 2. See also R. K. Mukerjee, "Organization of Cottage Industries and Handicrafts," in *Economic Problems of Modern India*, Vol. II. Edited by R. K. Mukerjee and H. L. Dey (London, 1941), p. 5.

141. The spread of the fly-shuttle after 1900 was not rapid enough to modify this point. At the same time, we have to allow for a rather large and apparently growing export trade in handloomed cloth. *Report, Indian Tariff Board (Cotton Textile Industry Enquiry) 1927* (Bombay, 1927), p 163; D. Narayana Rao, *Report on the Survey of Cottage Industries in the Madras Presidency* (Madras, 1929)

142. The argument in my original essay was sufficient to lead Toru Matsui (pp. 20-22) to uncertainty and Raychaudhuri (p. 93, note 23) to a general acceptance of the possibility, albeit with qualifications. Bipan Chandra as usual remains unsusceptible and doctrinaire, but surely there is enough evidence to justify at least a hint of revisionism even from him.

Part of Bipan Chandra's trouble arises out of his misunderstanding of the statistical evidence that he cites. Let me give one example. In his essay (p. 56) he uses data from Ellison which shows that between 1819-21 and 1880-82 the price of cotton cloth exported from Britain was falling more rapidly than the price of yarn exported. He intends this as proof that the competitive position of Indian handloom weavers "was weakening throughout most of the nineteenth century." However, these data refer to British exports to all markets and not merely to India. (T. Ellison, *The Cotton Trade of Great Britain* (London, 1886), p. 60.) Given the variety of products exported and the specific demands of the Indian market, it is likely that the particular price relationships between yarn and cloth exported to India were quite different (and thus the conclusions about the handloom sector would be quite different) than those which Bipan Chandra purports to find.

143. Mukerjee, *op. cit.*, p. 4.

144. The notion that spinners had no alternatives is based on the fallacious assumption that all or a large proportion were upper caste women who could only work at home. This of course was far from the case.

Second, it is not accurate that handloom weavers were forced into the production of coarse cloth. Much of British yarn exports to India throughout the period were high count, and at the time of Independence about 20 percent of handlooms were producing both high count cloth and the more complicated fabrics.[145] Moreover, the fact that the bulk of handloom output was in coarse cloth does not automatically lead to the conclusion (TM, pp. 21-21) that weavers' incomes fell or that they had to work harder to earn the same income.[146]

In addition, some of the data I have so far examined vaguely suggest that during the 19th century there was a tendency towards professionalization. In other words, it is possible that a larger percentage of handloomed fabrics was produced by people who worked fulltime at the occupation.[147]

This leads me to refer to some common and persistent misinterpretations of census caste data. It is not possible to use these census materials as proof of decline in a handicraft activity. One cannot show that the caste population in the "traditional occupation" is less than 100 percent and then conclude that the difference between the actual proportion and 100 percent represents the numbers forced out of the industry. Ignoring the difficulties that afflict the census occupational classifications, it was never true (so far as we know) that 100 percent of a caste was ever found in its "traditional occupation." Thus, deviation from 100 percent employment in the industry is not proof of absolute or even relative decline. Moreover, this form of analysis overlooks the persistent and large-scale movements of "non-traditional" groups into weaving after 1800, a fact ignored by almost all writers on the subject. Finally—and I must stress this because the reverse is frequently taken for granted—we cannot assume that movement out of weaving (or out of any "traditional occupation") necessarily implies a worsening of economic condi-

145. Mehta. "Handlooms," p. 5.

146. A weaver can turn out in a day much more coarse cloth than he can fine cloth. Moreover a shift from handspun to millmade yarn reduced breakage and hence increased a weaver's output. To the extent that the weaver depended on yarn produced by others, the availability of factory yarn reduced the time he had to spend searching the countryside for suitable yarn.

147. Gadgil, *op. cit.*, p. 183. This may explain why historians gained the impression that weavers were working harder.

tions.[148] Undoubtedly some groups did decline economically, but there were those who took advantage of profitable trading opportunities, and still other groups who became prosperous cultivators. The ultimate judgement can be made only after we have detailed examination of the evidence.

Scholars frequently and erroneously point to the decline of certain traditional weaving centers as proof of absolute decline. They overlook the fact that weavers and weaving communities for a host of reasons shifted their centers of work. There is evidence of this phenomenon in the traditional era; and it certainly was true in the modern period. In fact,

> the growth in the use of mill spun yarn . . . has freed handlooms from the necessity of being regionally dispersed in a pattern, more or less, coincident with the spinning industry.[149]

In other words, whereas some centers of weaving activity would decline, there will be others growing up.

Even Dacca, on close investigation, is not as obvious a case of decline as it might seem on first inspection. I have not the time to explore all the evidence or discuss all the issues. Let me merely say that this traditional handloom centre, which has been so frequently used to illustrate the grim fate of all handicrafts, was still such a great handloom fabric producer in the 20th century that on at least two different occasions it was proudly referred to as "the Manchester of India."[150]

The purported decline and then the apparent resurrection of Dacca as a major handloom centre brings up the most difficult issue of all. Although it now seems clear to me that the number of handloom weavers did increase between 1800 and 1947, I still have no idea about the shape of the trend line. Was expansion more or

148. The interpretive sections of the census volumes frequently have been guilty of these same errors of analysis.

149. Mehta, "Handlooms," p 2.

150. Mukerjee, *op. cit.*, p. 6, where he stated that there were 60,000 weavers working 20,000 looms in Duburhat in the Dacca District. The Fact Finding Committee on Handlooms, *op. cit.*, p. 13, actually referred to Madhobi near Dacca as the "Manchester of Bengal" a somewhat more modest claim.

less steady throughout the period or was there a secular decline for some period and then a recovery? Or were there episodes of decline and more than one period of expansion? Answers to these questions will have to wait on the completion of my work.[151] If I am able to get relatively precise results, particularly on a regional basis, my conclusions should lead to a better understanding of the rural sector particularly and of the economy as a whole.[152]

Industry. Although there are no fundamental disagreements on this subject, Raychaudhuri (p. 94) is correct in pointing out that my statement (p. 10) on the growth of industry tended to stress the relative rate of growth rather than the limited significance of that expansion. In this context, I wish once again to emphasize the need for detailed studies of cotton textile industry expansion.[153] Not only is the industry important in its own right, but its history is specially useful for comparative analysis. The development of cotton mills invariably is treated as independent of the economic environment within which it occurred. Yet its very rapid growth in the face of the most blatant Lancashire pressure illustrates the necessity of a specific environment within which expansion could take place. Why was it possible for the cotton industry to grow so swiftly in the teeth of most ferocious opposition while other industries did not thrive? Tariff protection was clearly neither a necessary nor a sufficient condition for its development. Rather it seems

151. Gadgil, *op. cit.*, pp. 180-183, suggests the possibility that there was rapid decline until the late 19th century and then some stable equilibrium was reached. However, Gadgil's analysis is essentially static and ignores the effect of the general increase in the demand for textiles. My own tentative judgement at the moment would be that, allowing for periods of crisis and decline, there was a long-run rising trend until the first World War after which real stagnation and some absolute reduction in numbers may have occurred.

152. There is one historiographic note to add in conclusion: I am sure that all my readers are familiar with Marx's famous passage: "The Governor General reported 1834-35: 'The misery hardly finds a parallel in the history of commerce. The bones of the cotton-weavers are bleaching the plains of India.'" (*Capital*, Vol. I, Chapter XV, Section 5). Bentinck must have been the Governor-General referred to. To the best of my knowledge, the two quoted sentences do not appear in Bentinck's report or in any of his papers. Though the matter is not terribly important, it is interesting as one of the few examples where Marx erred in a reference. (I am indebted to the distinguished Bentinck scholar, Dr. John Rosselli for his advice on this point.)

153. S. D. Mehta's two major studies, *The Indian Cotton Textile Industry* and *The Cotton Mills of India, 1854-1954* (Bombay, 1954), are important contributions but they barely skim the surface of the subject.

that on both the supply and the demand side there were appropriate indigenous technical and structural features that favoured the industry's growth. These elements must be kept in mind by students working on other industries or on India's growth in general.

My reference to the importance of the cotton textile industry brings forward another matter. I frequently have referred to a need for comparative regional studies that would enable us to understand the behavior of the Indian economy. (In this I am, of course, at one with Professor Raychaudhuri.) Yet I am struck by the absence of satisfactory investigations into the process of regional economic growth. For example, a study of Gujarat's economic development in the 19th and 20th centuries would certainly throw a different light on our all-India analysis as well as on our perceptions of developments in other regions.[154]

There was substantial commercial development in Gujarat (including Bombay city) after about 1830, and this was followed in the second half of the century by the growth of one of the world's greatest cotton textile industries. It seems to have been accompanied by substantial agricultural expansion that developed to meet the food and raw material needs of both the expanding urban centres and foreign markets.[155] Transport and marketing networks developed along with the industrial, commercial and agricultural expansion. All this indicates a fairly rapid expansion of economic activity plus some degree of those structural changes that are identified with an "industrial revolution." Detailed examination might possibly show that Gujarat's rate of economic development 1860-1947 rivalled the most rapid found anywhere in the world, including Japan's. If the impression is not unreasonable, it is also worth noting that this economic growth was not an enclave phenomenon. It occurred in a region as large and as populous as many of the countries that did go through an industrial revolution. Clearly

154. I think it safe to say that our views on the all-India story have been affected by a scholarly tendency to generalize from the Classical view of the situation in Bengal. On the other hand, I am certain that my perception of Indian economic history has been coloured by the fact that my initial introduction to the economic history of the subcontinent was concerned with the experience of western India.

155. H. V. Joshi's study, *op. cit.*, describes a development that occurred only after 1900. However, it is my guess that the intensive agriculture that preceded mango cultivation also was a response to earlier urban developments.

Gujarat's development did not suffice to pull up the whole of India. But the results in Gujarat (and their implications) have been ignored because they have been buried within the all-India averages and because most scholarly attention has been given to the patently less satisfactory structural developments in Bengal.[156]

Foreign Trade. My original essay made no mention of that most controversial issue, "The Drain," nor did it dwell particularly on other foreign trade issues. In this essay I already have suggested that I am not entirely convinced that foreign trade was the decisive instrument of change in the Indian economy. Possibly, its role has been greatly exaggerated. India whose external trade constituted a very small proportion of its gross national product was certainly in a quite different position than a country like Britain for whom foreign trade was proportionately very much more significant. Once this is recognized, the issue of "The Drain" (the Home Charges) is reduced to a matter of relatively minor economic significance compared with other factors. This is why I did not discuss it.

I realize that in making such a statement I am treading on peculiarly sensitive ground. Yet as I have said earlier, the mere fact that an issue possesses political vitality does not prove anything about the accuracy of economic conclusions drawn from it. "The Drain" is an issue of much greater political significance than it is of economic substance. Its political force derived, I suspect, from its capacity to serve symbolically as the focus for a host of substantial dissatisfactions. Its continued survival as a fundamental theme for many economic historians is explained by their failure to distinguish between an issue that could have been important for British economic performance but because of major structural differences was not for India's. There is no need for me to examine this matter in

156. This enthusiastic view of 19th-20th centuries developments in Gujarat, if correct, poses problems about some of my earlier comments. It may force me to reexamine my view of the structural characteristics of Mughal Gujarat. Or it may imply quite favourable changes in the 18th century, a point suggested by Raychaudhuri. Or it may confirm his view that post-1800 developments did not have to depend on these pre-existing characteristics. On the other hand, I made my earlier judgements having taken into account the post-1800 possibilities. Again, the issues will not be resolved until we get studies of Gujarat along lines suggested in this and in my original essay.

detail. Dr. K. N. Chaudhuri has explored the theoretical issues with considerable skill and has suggested what seems to me to be the appropriate lines of analysis.[157]

The Role of the State. Here we enter the area where so many of our differing interpretations are focused. Toru Matsui (pp. 23 ff.) made the important point that much of the disagreement is not necessarily basic but arises because different questions are asked and different features are stressed. A great many of the apparent disagreements on specific matters can be dissipated. They will yield to the test of detailed research, specifically to analysis with a quantitative orientation. I should note that had my original essay been longer, my description of the role of the State after 1800 would have been much more sophisticated and probably would have disarmed some, but not all, criticism. However, I would still have stressed the "genetic" features of my analysis and the holistic approach inevitably gives more weight than my commentators would allow to the time (and cost) that is required in order to achieve significant change in a system as complex as the Indian economy.

Allowing for the fact that there will always be areas of disagreement on matters of interpretation, the critical task is to widen the area of agreement about the actual career of the economy. Earlier I suggested various types of research that could contribute to this objective. With reference to evaluating the economic role of the State, let me indicate an important matter of priorities. I would urge economic historians to choose problems that concentrate more on the actual effect of the State as an economic agent and less on issues of policy development. I suggest this for two reasons. First, preoccupation with policy formation tells us nothing about the consequences of policy, and, second, concern with policy usually ends up as a discussion of policy-makers' intentions.[158] Although this may be of great interest to the political or intellectual historian, it contributes nothing to our knowledge of the actual performance

157. Chaudhuri, "India's International Economy in the 19th Century," *op. cit.*

158. For example, the work on land tenure policy reveals nothing substantive about actual behavior of the rural sector. Nor do the discussions of tariff policy tell us anything about how the cotton textile industry developed.

of the Indian economy.[159]

What we need are studies of the State treated as a major economic actor that supplies and demands resources and thus affects the performance of the system. For example, what were the specific economic effects of various parts of the tax structure? What were the specific effects of government expenditures? These questions do not disregard the issues that my commentators think are most important, rather they suggest ways of posing them that would contribute more effectively to our knowledge of the economic system's actual performance.

VI

By now it should be apparent that I do not consider my original essay to be a conclusive piece of work. My use of *per capita* income as a measure of economic performance was certainty not intended to foreclose other kinds of work. In fact, *per capita* income was intended to serve only as an index that might symbolize and summarize the complex social process of economic change that interests all of us. On the basis of the very limited evidence that scholars have examined, it is not improbable that the movement of *per capita* income 1800-1947 is in the direction I have suggested. But the specific conclusion is not crucial. What is more important is that in the course of developing the implications of that conclusion and from the variety of remarks made by my commentators, it should have become obvious that the Classical assumptions and conclusions need much more thoroughgoing investigation than they have so far received. There is now sufficient evidence and an adequate logical case to induce economic historians both to broaden

159. My awkward way of putting the point in my original essay (p. 12, footnote 34) has deservedly got me into trouble with all my commentators. I did not mean to be interpreted as saying that "the general object of the *raj* was the welfare of the society." Rather, I was trying to suggest that its intentions could be ignored. The relative slowness of economic growth did not have to be explained in terms of the evil intent of imperial policy. Even if we assumed that the intent of the *raj* was the welfare of the society, the results were not very good. In other words, by eschewing the quagmire of motivations, we could proceed more effectively to explain the behavior of the Indian economy. See R. W. Weiss, "Economic Nationalism in Britain in the Nineteenth Century," in *Economic Nationalism in Old and New States.* Edited by Harry E. Johnson. (London, 1968), pp. 31-47.

their range of questions and extend their techniques as well as to be more critical of the conclusions with which they have too long been content.

In the course of this response to my commentators I have tried to indicate the tools that could be used. Economic theory and quantitative methods can be employed to help formulate questions and hypotheses with greater clarity and precision, to make the analysis more systematic and to enable us to be more critical in testing results. I think I have also shown that the use of specific theoretical techniques need not necessarily bias the answer in one direction or another. Rather, the combination of precise formulation and quantitative orientation makes it possible to enrich our knowledge of the processes at work in the complex Indian society. If my essay contributes to this result, if it has shown that Indian economic history is an open and exciting field, it has served a truly useful purpose. After all, as Piet Hein wrote :

Problems worthy of attack
Prove their worth by hitting back.

Opportunism of Free Trade: The Sadasheogarh Harbour Project, 1855-1865

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In their now well-known article published some years ago, Professors J. Gallagher and R. Robinson pleaded that the British governments in the nineteenth century, particularly during the midfifties, basically pursued a policy of economic imperialism. In spite of their professed faith in the free trade doctrines these governments, the authors argued, in dealing with the colonies deviated from their main tenets to satisfy the economic needs of the mother country. The "Imperialism of Free Trade"—the name given by Gallagher and Robinson to the whole process—has set off a lively debate, and some recent writers have joined the fray.[1] The present article is an attempt to re-examine the Gallagher-Robinson thesis in terms of a project to construct an international port at Sadasheogarh on the west coast of India.

I

Situated in the North Canara district of the present Mysore State, the small village of Sadasheogarh[2] had some advantages as a natural

1. J. Gallagher and R. Robinson, "Imperialism of Free Trade", *Economic History Review*, 2nd series, VI (1953), 1-15. Also see P. Harnetty, "The Imperialism of Free Trade : Lanchashire and the Indian Cotton Duties, 1859-1862", *Ibid.*, 2nd series, XVIII (1965), 109-25 ; R. J. Moore, "Imperialism and Free Trade Policy in India, 1853-54", *ibid.*, 2nd series, XVII (1964), 135-45 ; P. Harnetty, "India's Mississippi : The River Godavari Navigation Scheme, 1853-71", *Journal of Indian History*, XLIII, pt. III (1965), 699-732. For the opposite view see O. MacDonagh, "The Anti-Imperialism of Free Trade", *Economic History Review*, 2nd series, XIV (1961-62), 489-501. For a plea to modify Gallagher-Robinson thesis see D. Tripathi, "Opportunism of Free Trade : Lancashire Cotton Famire and Indian Cotton Cultivation", *Indian Economic and Social History Review*, IV, No. 3 (1967), 255-263.

2. The place took its name after Raja Sadasheo Rao of Soonda who had built a fortress (garh) there. The East India Company had a factory at the village from 1663 to 1752. The place had been selected for its central location from the point of view of trade in pepper and cardamom, and also because of excellence of anchorage in

harbour. It stood at the mouth of River Kali which formed the Karwar Creek only a short distance away from the village before meeting the Arabian Sea at a bay known as Sadasheogarh Bay. Near the southern bank of the river at its mouth was Bhatkal Cove which might facilitate the entry of the ships into the river which connected Sadasheogarh with the interior.

As early as 1855, these natural characteristics of Sadasheogarh had attracted the notice of an officer of the Indian Navy, Lieutenant Taylor. North Kanara then was a part of Madras Presidency. So Lt. Taylor submitted a proposal to the Government of Madras to develop the harbour. His plan envisaged throwing out two break-waters to keep off the swell caused by the winds and the erection of wharves, warehouses, and lighthouses. The Madras Govenment, having collected some additional information, recommended the proposal to the Court of Directors the next year.[3] While the proposals of the Madras Government were still under consideration, the Cotton Supply Association of Britain got interested in the project. The Association had been formed in 1857 to strive to augment the imports of raw cotton from sources other than the United States which supplied nearly 80 per cent of the raw material used in the British mills—about 3000 in number. The textile manufacturers had become conscious of their too great dependence on a single source, and India was a principal substitute, which, if properly developed, they argued, would provide cotton in great quantity and good quality. The development of a major port at Sadasheogarh, they believed, would facilitate the export of cotton from India.[4]

This belief had a reasonable basis. Dharwar, a rich cotton producing region, was only eight miles from Sadasheogarh, and Hublee, the principal cotton mart, only 90 miles. Experiments carried on during the last two decades had shown that the soil and climate of Dharwar were suitable to the production of improved American varieties which the British mills needed.[5] In the absence of suitable harbours in the vicinity, the Dharwar product was first taken to Kumta, a small port 25 miles in the south of Sadasheogarh, and thence to Bombay, more than three hundred miles in the north, in open primitive boats. This operation on the one hand increased the cost of transport; on the other hand, it resulted in the deterioration of the staple which remained exposed to all sort of dirt and rubbish during its transit to Bombay. The development of a new port at the doorstep of cotton fields would certainly help India help British mills. The Cotton Supply Association, therefore, represented to the Court of Directors on behalf of the harbour scheme. The Court, as a matter of routine, forwarded the Association's representation to the Government of India which passed it on to the Government of Madras.[6]

Only a few months later, however, the Court of Directors rejected the Taylor plans on the ground that they "were not sufficiently matured to be submitted to the Government of India" and decided to abstain from "entering into a discussion of the entire project." The Court, however, favoured the construction of a lighthouse.[7] Implicit in this action was the sad state of Indian finances which had further deteriorated as a result of the Mutiny.

However, before the Court's decision had been announced the harbour project caught the attention of the Government of Bombay Presidency which had jurisdiction over the Dharwar region. One of its engineers, William Parker, who had been asked to assess the potentialities of Sadasheogarh was impressed by "the possible future site of a great national harbour." The Governor of Bombay in his minutes to the engineer's report favoured the development of

the bay. In 1692, the East India Company had sent three ships from the port directly to England. Later the Company had to suspend its operations because of the opposition of the local raja and his people and the intrigues of the Portuguese and the Dutch. See C. Markham's note encl. with Secy. of State to the Govt. of Bombay, July 31, 1863 (P. W.), No. 21.

3. Government of Madras to Court of Directors, Oct. 7, 1856 (P. W.), No. 22.

4. A. W. Silver, *Manchester Men and Indian Cotton, 1847-1872* (Manchester, 1966), 58-98; H. B. Hammond, *The Cotton Industry* (American Economic Association), 251; W. O. Henderson, *The Lancashire Cotton Famine, 1861-65* (Manchester, 1934), 35.

5. S. Leacock and D. G. Mandelbaum, "A Nineteenth Century Development Project: The Cotton Improvement Programme", *Economic Development and Cultural Change*, III (July, 1955), 334-51.

6. Court of Directors to Governor General, Oct. 14, 1857 (P. W.), No. 42 *Parliamentary Papers* (House of Commons), No. 81, 8.

7. Court of Directors to Government of Madras, May 18, 1858 (Marine), No. 15.

the port on grounds of commercial and military needs. Subsequently, the Dharwar and Sadasheogarh Road Commission declared that the harbour though serviceable "even in its present unimproved state" was "capable of being made superior to any other port on the western coast of India excepting *perhaps* Bombay." (italics added). Both the Governor and the Commission, however, stressed that under the existing financial stringency, no expensive projects could be undertaken. They preferred to wait till the return of more favourable days. The utmost they could recommend was the construction of an approach road.[8]

Though uncomfortable at this unwarranted encroachment of the Bombay Government on its sphere,[9] yet undaunted by the rejection of the Taylor plans, the Government of Madras in the meantime went ahead with its own survey work. Its engineers prepared an estimate of the total expenditure which amounted to a minimum of Rs. 8,30,000. They however recommended the construction of a pier as the first stage at an estimated cost of Rs. 21,500 exclusive of contigencies. The Madras Government accepted these proposals in April 1860, but since there were no funds available in that financial year, the items were included in the next year's budget. In the meantime, the Madras Government had decided to construct an approach road at a cost of Rs. 10,000 via Khyga Ghat up to the boundary between North Kanara and Dharwar. The Bombay authorities, though inclined to a different route, were to complete a section of the Khyga Ghat road since it passed through their territory.[10]

While these schemes were awaiting the approval of the India Office, the Cotton Supply Association drew the attention of the Secretary of State for India, Sir Charles Wood, to the need for an

8. Government of Bombay to Court of Directors, May 4, 1858 (P. W.), 17, see enclosures; Secy. of State to Government of Madras, August 16, 1860 (P. W.), No. 50, enlosures.

9. Government of Madras to Secy. of State, June 10, 1858 (P. W.), No. 13; Government of Madras to Government of India, June 10, 1858 (P. W.), No. 1094.

10. Government of Madras to Secy. of State, May 16, 1860 (P. W.), No. 28; Secy. of State to Government of Madras, Aug. 16, 1860 (P. W.), No. 50; The Government of Madras to Secy. of State, Oct. 4, 1860 (P. W.), No. 65; Secy. of State to Government of Bombay, Nov. 13, 1862 (P. W.), No. 19. The cost of the pier, though variously mentioned in official documents, is given on the basis of this despatch from the Secy. of State.

early completion of the works. The cotton interests were contemplating the formation of a joint stock company which could purchase Indian cotton of improved varieties and ship it to Britain. Assuring the Association that "orders have been issued for the construction of a landing and shipping pier at Sadashegar" [sic], the Secretary urged the Madras Government to complete the works. Later when the Association decided to launch the proposed company, the Secretary counselled the Madras authorities in August and December 1860 to give the company "every encouragement which can properly be afforded by Government" and stressed the need to construct the Khyga Ghat road without any loss of time.[11] About the same time, the Governor of Bombay, Sir George Clerk, underlined his interest in the project by paying a visit to Sadasheogarh. In a minute recorded after the visit, the Governor expressed his concern for the allotment of more money than the Madras Government had done for the construction of the Khyga Ghat road.[12]

All these plans and activities were confined to papers only. Till the end of 1860, five years after the project had been mooted, nothing tangible had been achieved. The proddings of Manchester and the enthusiasm of the local governments in the face of acute financial deficits and a cumbersome and disorganised bureaucracy, achieved but little in terms of practical results. Even the site for the pier was yet to be selected and the first pickaxe on the Khyga Ghat road had yet to be applied. As a matter of fact, in spite of numerous communications, the whole project, as the Governor of Madras later observed, "appears to have been left in abeyance altogether."[13]

11. Secy. of State to Government of Madras, Aug. 31, 1860 (P. W.), No. 53; Dec. 15, 1860 (P. W.), No. 63; Secy. of State to Government of Bombay, Nov. 13, 1862 (P. W.), No. 19. For a systematic study of the Manchester Cotton Company see P. Harnetty, "India and British Commercial Enterprise: The Case of the Manchester Cotton Company, 1860-64", *Indian Economic and Social History Review*, III, No. 4 (1966), 396-421.

12. See Clerk's minute, Dec. 31, 1860 encl. with Government of Bombay to the Secy. of State, Jan. 12, 1861 (P. W.), No. 3.

13. Government of Madras to Secy. of State, June 14, 1861 (P. W.), No. 55, see enclosures.

II

The possibility of the Civil War in America invested the Sadasheogarh project with a new significance. By the beginning of 1861, the rupture in the American supplies consequent on the conflict was fairly in sight and the cotton interests pinned their faith on India as a substitute source [14] Anticipating the pressure from the cotton interests, Wood wrote to Viceroy Lord Canning in January, "I shall concentrate all my efforts on improving Sadash ghur [sic] making a good road to it ... and connecting the growing dists. with this main road."[15]

The impending crisis stirred up the Manchester people. The proposal for the cotton company which had failed to attract even a modest nominal capital of £100,000 after four months of persuasion and exhortation was revived again in January, and the Manchester Cotton Company was floated amidst great enthusiasm and fanfare at a nominal capital of £1,000,000. One of the earliest decisions of the Company was to set up an agency in Dharwar, Bellary, and adjacent areas to procure improved cotton, and a pressing and shipping establishment at Sadasheograh. In order to make local arrangements the Company decided to send its Secretary G. R. Heywood, who was also the Secretary of the Cotton Supply Association, to India. These decisions had been taken on the advice of an official of the Bombay Government, then on leave in England, G. F. Forbes, who informed the Company that a road connecting the harbour would be ready for traffic by the end of 1861. A few days later, the Company informed Wood of these decisions adding that a ship with pressing and cleaning equipment to be installed at Sadasheogarh would be sent "in the course of few weeks," and urged a speedy construction of the planned pier. The Secretary, on the basis of a communication from the Government of Madras

14. Silver, *Manchester Men and Indian Cotton,* 59-98 ; *Cotton Supply Reporter* (Manchester), Sept. 1, 1858, Dec. 1, 1858, Dec. 1, 1860, June 1, 1861. Also Alexander Mackay, *Western India : Reports Addressed to the Chambers of Commerce of Manchester, Liverpool, Blackburn, and Glasgow* (London, 1853) ; Forbes Royle, *On the Culture and Commerce of Cotton in India and Elsewhere* (London, 1851) ; John Chapman, *The Cotton and Commerce of India Considered in Relation to the Interests of Great Britain* (London, 1851).

15. Wood to Canning, Jan. 23, 1861, Halifax Collection (India Office Library, London), India Office Letter Book (cited hereinafter as IOLB), VI, 74.

received in May 1860, assured the Company that the pier "would be ready in time for the road down Khygah Ghaut [sic] leading direct to harbour." Simultaneously, in a despacth to the Madras Government he stressed the need for the early construction of the pier in view of the Company's decision.[16]

The Madras Government was alive to the situation and thought that any delay in "this highly important project" was "very undesirable." But no sites for the pier were selected till April and when one was selected it was different from the one contemplated earlier. Progress on the Madras portion of the Khyga Ghat road was equally disappointing and by March the Madras Government had given up hopes of completing it in 1861. The Governor of Madras aptly summed up the situation : "we are now in May 1861 just in the same position as we were in April 1860."

The Madras Government blamed and censured the chief engineer of North Kanara for failure to select the site for the pier and prepare the plans in time. Shortage of labour and money was attributed as the reason for tardy progress on the road. Wood regretfully appreciated these reasons and expressed his hope that "every exertion will be made" to complete the works "at the earliest possible period."[17]

In the meantime, the harbour scheme had become complicated in view of a proposal to transfer North Kanara from the Madras Presidency to Bombay. Beginning 1829, when the idea was first mooted, the question had been considered several times. In 1860 "before there was any reason to expect a disturbance of the supply of cotton from the United States" Clerk asked Canning to consider the question. Canning was sympathetic, but reluctant to take action before consulting Madras. Under the mounting cotton crisis, Wood favoured the transfer on the ground that the cotton growing areas and the port as well as the link road should all be

16. *Cotton Supply Reporter,* Feb. 1, 1861, Forbes to the Directors of Manchester Cotton Company, March 5, 1861, *Cotton Supply Reporter,* March 15, 1861 ; Secretary of State to the Govt. of Madras, April 16, 1861 (P.W.), No. 16, see enclosures ; Secy. of State to Govt. of Bombay, Nov. 13, 1861 (P.W.), No. 19.

17. Govt. of Madras to Secy. of State, April 19, 1861 (P.W.), No. 40, see enclosures; same to same, June 14, 1861 (P.W.), No. 55 ; Secy. of State to Govt. of Madras, Aug. 8, 1861 (P.W.), 36.

under the same administration. Caring little for administrative niceties, he asked Canning in January 1861 to effect the transfer "forthwith" if there were no insuperable difficulties."[18]

Difficulties there were and the most insuperable of them all was the attitude of Governor William Denison of Madras. Reluctant to give up a part of his territory, he first pleaded that the Government of Bombay would have little interest in developing a port in opposition to Bombay whereas Madras had a direct interest in the project. Almost in the same breath, however, he condemned the whole scheme as "ludicrous", "ill-conceived, and useless." An engineer by training, he stressed that the bay was open to westerly and north westerly winds which would render the vessels unsafe. The idea of breakwater, he imagined, was a "delusion people who know nothing of such matters talk of."[19] With his dilatory tactics Denison was trying to withstand the combined pressure of Wood, Canning, and Clerk. The exasperated Viceroy suggested to both the governors to settle the affair between themselves.[20]

Unaware of the change of mind on the part of the Madras Government towards the works, Heywood reached Bombay in the last week of August. On Wood's instructions and at government expenses Forbes had accompanied him to India. The Secretary of State had already asked the authorities in India to accord proper treatment and active cooperation to the Secretary of the Cotton Company. He had also instructed the Madras Government to stop the sale of land at Sadasheogarh on the ostensible ground that it might interfere with the future planning of the harbour.[21] The real

18. Canning's Minute on the Transfer of N. Canara, Indian Papers of Lord Canning (Central Library, Leeds), Volumes of Manuscripts 1861-62; Canning to Frere, Jan. 27, 1861, *ibid.*, Letters to the members of the Council; Canning to Clerk, Jan. 30, 1861, *ibid.*, Letters to the Governor of Bombay; Wood to Canning, Jan. 23, 1861, Halifax Collection, IOLB, VI, 74; Secy. of State to the Govt. of Bombay, Feb. 28, 1861 (P.W.), No. 10; Canning to Wood, March 4, 1861, Indian Papers of Lord Canning, Letters to the Secy. of State.

19. Denison to Canning, May 20, 1861, Indian Papers of Lord Canning, Letters from the Governor of Madras; Canning to Wood, July 8, 1861, *ibid.*, Letters to the Secy. of State.

20. Wood to Morehead, March 4, 1861, IOLB, VI, 238; Telegram from Clerk to Canning, Oct. 18, 1861, Indian Papers of Lord Canning, Private Secratary's Papers, Register of Telegrams Received, 1856-62.

21. Annual Report of the Manchester Cotton Company quoted in *Cotton Supply Reporter*, Sept. 1, 1862; Wood to Canning, Aug. 30, 1861, Halifax Collection, IOLB, VIII, 244; Wood to Clerk, Sept. 14, 1861, *ibid.*, VIII, 270; Wood to Morehead, March 4, 1861, IOLB, VI, 238.

reason for the action, however, seems to have been his concern to save the company from land speculation and help Heywood acquire the best possible site.[22]

Heywood was surprised to find the harbour project, on the progress of which depended the success of his Company's operations, in a deplorable state. The work on the pier had hardly made a beginning, North Kanara was still a part of Madras Presidency, and although the Bombay Government was confident of completing its own portion of the Khyga Ghat road, it would be useless as a link between the harbour and the cotton districts unless the Madras portion was ready. Heywood started a barrage of correspondence with the Chairman of the Company, John Platt. Supported by Forbes, he attributed the lack of progress on the pier and the road to failure to transfer North Kanara to Bombay.

Platt saw no hope for the completion of these projects by February or March of 1862. Demanding an immediate transfer of the territory to Bombay, Platt informed Wood that the Cotton Company was shipping machinery to Sadasheogarh "in a few days" and that there would be a second shipment in December. Wood assumed that the orders for the transfer had already been issued.[23]

Denison, however, had held on to his ground. In October 1861 he went to Sadasheogarh and met Clerk to settle the problem of transfer. The meeting achieved nothing.[24] Denison inspected the site for the pier and in a letter to Lord Canning summed up his reaction :[25]

22. Accusing Wood of partiality towards the Cotton Company, the Bombay Chamber of Commerce passed a resolution against the official attitude. See Harnetty, "India and British Commercial Enterprise", 402; Forbes to Chairman, Bombay Chamber of Commerce, Oct. 19, 1861, *Cotton Supply Reporter*, Dec. 2, 1861.

23. Secy. of State to Govt. of Madras, March 7, 1862 (P.W.), No. 4, see enclosures.

24. Clerk's telegram to Canning, Oct. 18, 1861, Indian Papers of Lord Canning Private Secretary's Papers, Register of Telegrams Received, 1856-62.

25. Denison to Canning, Oct. 28, 1861, *ibid.*, Letters from the Governor of Madras.

"Sadasheghur [sic.] is a fair harbour but it will never be the outlet of a large district; it is shut in by Goa to the north and there will be 70 miles of road from the cotton districts in the rear about Dharwar; a few ships will bring and take away all that these districts either want or can spare. I should not, therefore, recommend any large outlay at the harbour, in fact little is required beyond what we shall be able to do with the money already at our disposal."

The Governor pointed out that even if the pier were to be constructed 400 or 500 yards into the bay, there would be still insufficient water for big vessels and a mere boat pier would be unnecessarily expensive. He thought that in view of the fact that Dharwar produce could be shipped from Madras which was going to be connected to the cotton districts through railways only the next year, it was useless to develop Sadasheogarh.[26]

Heywood had reached Sadasheogarh while Denison was still there. They met on October 12 and 14. Denison discussed his objections to the pier with Heywood and apparently in his presence abandoned the idea of the pier and decided to build a wharf at some considerable distance from the site selected for the pier. This necessitated the construction of an additional mile of road. The Governor informed the Secretary of State of these changes and assured him that the district engineer expected "by April next to have a good 12 feet road open all the way to Sadashegar [sic]."[27]

Denison, in the meantime, continued his fight against the transfer of North Kanara. Though aware of the pressure on Wood, he was opposed to the transfer just to please the British cotton interests. He was also unhappy that Heywood was given special treatment and urged that he should be treated rather as "an agent for a Company of spectulators than encouraged to think himself of importance as representing a firm consulted and patronized by the

26. Govt. of Madras to Secy. of State, Dec. 4, 1851 (Revenue), No. 119, see enclosures.

27. Govt. of Madras to Secy. of State, March 7, 1862 (P.W.), No. 4.

Govt."[28] By December Wood's patience had exhausted. He could conceal his annoyance and anger no longer. He urged Canning to effect the transfer and wrote to Clerk: "I can not stand any delay about N. Canara [sic] ... I am very angry because both publicly and privately I had put matters as strongly as I thought civil . . ."[29]

Denison's resistance by this time was breaking down. He agreed to give up the district except the South Talook, rebuking Wood at the same time for dealing "with India with special reference to Manchester Cotton spinners" who in his opinion were essentially narrow-minded and selfish. Since the formal transfer would take some time, the Government of India placed the public works in the district under the Government of Bombay. In April 1862, North Kanara with the exception of Kondapur became a part of the Bombay Presidency.[30]

III

The Bombay Government inherited the works in an extremely unsatisfactory state. The work on the wharf which Denison had ordered had made very little progress and yet 500 labourers employed there had been removed to the road in January on the ostensible ground that they were not trained in blasting operations. The Madras portion of Khyga Ghat road—a "good 12 feet road" according to Dension's engineer—was nothing but a "narrow pathway of loose earth." It ought to have been at least 27 feet wide to be serviceable, but the Madras Government had made no attempt in this direction. There was a 12 feet wide trace, and Madras probably had no plan to do anything more since as late as January 1862 no lime, brick, stone, or metal were in sight. As a result, the road was "unfit to carry any considerable traffic." On the basis of

28. Denison to Wood, Oct. 21, 1861, Halifax Collection, Letters from Governor of Madras.

29. Wood to Denison, Nov. 26, 1861, *ibid.*, IOLB, IX, 132; Wood to Clerk, Dec. 3, 1861, *ibid.*, IX, 140.

30. Denison to Wood, Dec. 25, 1861, *ibid.*; Letters from Governor of Madras; Govt. of India to Secy. of State, Dec. 31, 1861 (Revenue), No. 20; Secy. of State to Govt. of India, Feb. 28, 1862 (Political), No. 16.

a report which the engineers had prepared after inspecting the site, the Bombay Government concluded that the hope of completing this portion of road by April, as Madras had hoped, was nonsense; it was "utterly impossible" to finish it in another year."[31]

The Bombay Government took up the works in right earnest. It employed extra labour on the Kanara portion of the Khyga Ghat road, and pending its completion, decided to open an alternative route in the south of the river Kali via Arbyle Ghat. The new road was slightly indirect, but easier to build. Disapproving of Denison's plans for a wharf on the ground that it would not be sufficient to land heavy cargo, the Bombay Government revived the project for the pier at the site which the Madras Government had originally selected for the purpose. But scarcity of labour impeded progress.[32]

In the meantime, the *Seringapatam* with the machinery and machines of the Cotton Company had left Liverpool in December and was expected to arrive in April. In order to facilitate the unloading, Forbes in cooperation with the Company's engineer and Heywood, and at the expense of the Company, prepared a makeshift landing place at the site of the wharf. However, when the ship arrived on May 6, the landing place proved inadequate and at one stage, Heywood was thinking of diverting the vessel to Bombay. With the assistance of Forbes and other officials of the Bombay Government, the machinery was finally unloaded.[33]

The operations of the Cotton Company proved a dismal failure. The principal reason was the lack of planning before the Company started its work. Its representative Heywood, who had no knowledge of Indian conditions and lacked authority to take independent decisions, committed costly mistakes.[34] His brusque manners and rude behaviour and the partiality shown to his Company by Wood

31. Govt. of Bombay to Secy. of State, April 2, 1862 (P.W.), No. 3. See enclosures also.

32. Govt. of Bombay to Secy. of State, May 5, 1862 (P.W.), No. 4; Govt. of Bombay to Secy. of State, Jan. 13, 1863 (P.W.), No. 2, see enclosures.

33. Govt. of Bombay to Secy. of State, Jan. 21, 1863 (P.W.), No. 37; see enclosures.

34. For the causes of the failure of the Cotton Company see P. Harnetty, "India and British Commercial Enterprise", 413-416.

alienated him from the European houses of Bombay who were equally interested to start operations at the new port. However, the Company held Wood and Indian authorities responsible for its failure. All would have been well, the directors thought, only if the pier and the roads were ready when their ship arrived. Wood rejected these charges. He was, however, conscious that he had not fulfilled his promises to the Cotton Company with regard to pier and the road and felt sorry for the "loss and disappointment" to the Company.[35]

Meanwhile, the new Governor of Bombay, Sir H. Bartle Frere, gave further push to the works. He had great sympathy for the cotton interests and was ready to go to any length to satisfy their needs.[36] In view of the "urgent demands made on this country for cotton" he authorised his engineers to spend whatever they could economically, fairly, and honestly "on such works as will facilitate the transfer of this year's cotton crop to the seaport." He wanted the port to be ready for the crop of 1862. "The money value of this crop to India is great," read a resolution of the Bombay Government, "but its value to England cannot be told in money; and every additional thousand bales which we can get down to the sea coast before the season closes in June may not only save scores of weavers from starvation or crime but play no unimportant part in ensuring peace and prosperity to manufacturing districts of more than one country in Europe." Frere's government also began the planning of a port town at Bhatkal near Sadasheogarh with Khyga Ghat road as the main street. The town to be administered by a municipality was to be the headquarters of the newly-ceded district. The Governor was also persuading some private companies to

35. Wood to High Mason of Cotton Supply Association, Nov. 5, 1862, Halifax Collection, IOLB, XI, 245-251; Secy. of State to Govt. of Bombay, Nov. 13, 1862 (P.W.), No. 19; *Cotton Supply Reporter*, Sept. 1, 1862.

36. Frere, as a member of the Viceroy's Council, had played a significant role in persuading Canning to issue a resolution on Feb. 28 1861, calling upon the local governments to do their utmost to facilitate the cultivation of cotton in and its export from India. For his role, see John Martineau, *Life and Correspondence of Rt. Hon. Sir Bartle Frere* (London, 1895), I, 398. Later when Frere pressed for increased grant for public works in Bombay Presidency in the budget of 1863-64, Viceroy Elgin accused him of ingratiating himself with Manchester at the cost of sound political economy. Elgin thought that Frere had "the fear of Bright and Manchester before his eyes." Elgin to Wood, Feb. 21, 1861, Elgin Papers (India Office Library, London.)

connect the new harbour with the interior through light railways or tramways.[37] Wood did not agree to the terms of the railway companies. But what shocked Frere most was a reported decision of the Government of India to reduce public works grants for Bombay in the next year's budget.

The action of the supreme government threatened the progress of harbour works. A furious Frere set every sail to wind.[38] After a visit to the port in February 1863, he was impressed by Sadasheogarh "as a great outlet of the cotton grown in Dharwar and the neighbouring districts of Bellary and Mysore." He was convinced that the capabilities of the new port far exceeded those of any port . . . on this side of India except Bombay and Kurachee." Even in its existing state he regarded Sadasheogarh as fully "equal to such a trade as that of Madras and capable of great improvement." He thought that the port with "its really good shelter for large ships combined with deep water" might eventually supersede Bombay. These considerations made North Kanara in his view the best district in the whole of India to spend money on roads. Pointing to these virtues of North Kanara and Sadasheogarh and in view of the special needs of the British cotton interests, he appealed to Wood and Elgin, the new Viceroy, for help.[39] After a great deal of effort and correspondence, Frere finally succeeded in extracting a promise from Charles Trevelyan, the finance member of the Viceroy's council, to give as much money as he could usefully spend on cotton roads.[40]

Frere had not let the works suffer in the meantime. Once financial uncertainty was over, he pushed them further. By the middle of 1863, Khyga Ghat road was ready for a limited amount of cart traffic and Arbyle Ghat was also partially complete. At the

37. Govt. of Bombay to Secy. of State, Nov. 24, 1862 (P.W.), 15 ; Resolution of Bombay Govt., Nov. 6, 1862 quoted in *Report of the Bombay Chamber of Commerce* for the year 1862-63, 268 ; on the planning of the port town. see Appendix L of the *Chamber Report*, pp. 273-275 ; Frere to Wood, June 11. 1862, June 26, 1862, July 25, 1862, Sept. 11, 1862, Halifax Collection, Letters from Governor of Bombay.

38. Frere to Wood, Jan. 27, 1863, *ibid.*, Letters from Governor of Bombay ; Frere also wrote a series of letters to Elgin, the Viceroy, and Trevelyan, the Finance Member of the Council.

39. Frere to Elgin, Feb. 26, 1863, Elgin Papers ; Frere to Wood, March 27, 1863, Halifax Collection. Letters from Governor of Bombay.

40. *Proceedings of the Legislative Council of India* for 1863, 80.

harbour, completion of wharfage made easy landing possible, and, according to the Bombay engineers, it was now possible to ship cotton from Sadasheogarh. But since the cotton presses of the Bombay houses or Cotton Company were not yet ready, the dealers as usual continued to ship their consignment from Kumta. The officials believed that the next season's crop would be shipped from Sadasheogarh.[41]

This hope did not materialise. Epidemics interrupted labour supply at Khyga Ghat road which as late as September 1864 was "barely passable" for carts. During the monsoon season of 1863, rains washed off portions of the Arbyle Ghat road. By January next year, although there was "no impediment to cart traffic" on this road, it needed another year to be made "well passable" for carts. By the middle of 1865, these works were nearing completion. The port also had a pier and a light house. Believing that the port would be of international importance and Sadasheogarh was too difficult for the Europeans to pronounce, the Government decided to name the port as Carwar [sic]. The improvements at the port and the roads, the Government of India believed, were enough for "any traffic likely to spring up within the next twenty years."[42]

The end of hostilities in the United States sealed the fate of the new port as an international harbour. As soon as the news of General Lee's surrender reached Bombay, the whole cotton trade was plunged in gloom. The confidence and buoyancy disappeared, yielding place to an unprecedented slump. Many Bombay houses, which had thrived on cotton speculation, disappeared.[43] Having failed in its Indian operations, the Manchester Cotton Company

41. Govt. of Bombay to Secy. of State, June 4, 1863 (P.W.), No. 26, see the enclosures.

42. *Report of the Bombay Chamber of Commerce* for 1862-63, 273-75 ; Memo regarding Present State of Public Works in Canara by J. M. Shaw Stewart, Sept. 13, 1864 in Halifax Collection, Miscellaneous Papers on Cotton Cultivation ; House of Commons, *Moral and Material Progress of India during 1864-65* (1866), 43 ; Secy of State to Govt. of Bombay, July 31, 1863 (P.W.), No. 21.

43. James M. Maclean, *Guide to Bombay, Historical, Statistical and Descriptive* (Bombay, 1880), 118 ; Frere to Wood, June 21, 1865, Halifax Collection, Letters from Governor of Bombay ; House of Commons, *Report of the Commissioners Appointed to Enquire into the Failure of the Bank of Bombay* (London, 1869), 28, 46 ; R.J.F. Sulivan, *One Hundred Years of Bombay : History of the Bombay Chamber of Commerce 1836-1936* (Bombay, 1937), 67-68 ; D. E. Wacha, *A Financial Chapter in the History of Bombay City* (Bombay, 1913), et passim.

had already closed its shop; after the peace in America, the Bombay houses also dismantled their establishments. They were useless there, for Britain would turn to its traditional source, the United States, for cotton and whatever Dharwar would export would go by the usual Kumta-Bombay route.

IV

Does the attitude of Wood and Indian authorities toward the Sadasheogarh harbour project justify the theory of economic imperialism? The nineteenth century free-traders included public works in the legitimate functions of governmental activities. As such harbour improvement was no deviation from their doctrine. This would be a superficial interpretation, since it is obvious that a project which had languished for five years for want of funds was vigorously pursued in the face of serious financial and logistic problems in order to satisfy the needs of the British textile industry. But to attribute the official interest in the port development wholly to the economic needs of the mother country would be equally superficial. The real causes lay much deeper.

At the very outset of the American War, the British Government on purely practical grounds had decided to remain strictly neutral, although a strong section of its population favoured recognition of the breakaway government of the American cotton states. The Southern Confederacy by suspending the export of cotton to Britain was bent upon coercing the British Government into recognizing its independence.[44] Since textiles were a bulwark of the British economy and provided support to a large part of the population, slackness on the part of the government to develop alternative sources of supply would have either resulted in the success of the Confederate diplomacy or the fall of the government. Manchester had a very strong lobby in Parliament and the coalition ministry of Lord Palmerston never felt too stable.

To placate the cotton interests and general public, Lord John

44. 3 *Hansard*, CLXX, 1209 (March 7, 1862); For an excellent account of the Confederate diplomacy see Frank L. Owsley, *King Cotton Diplomacy* (Chicago, 1959).

Russell, at the very beginning of the cotton crisis had assured the Cotton Supply Association that everything possible would be done to fill the rupture in American supplies.[45] Since India had become the "cotton hope" of Britain by 1860, his government had to prove that it was doing its best to enable India to ship its product for British use.

These were the considerations which accelerated the harbour project on the eve of the American War. Wood had a very poor opinion of the British cotton interests; in fact, he regarded them as a "hopeless and helpless set" and "worst fools he had met for a long time." But realising their political value, he was anxious to treat them politely and satisfy their demands to the best of his ability.[46]

The harbour works, thus, were less related to the economic needs of the mother country than to the political opportunism of the British Government. To this extent, the Gallagher-Robinson thesis needs modification.

45. British Foreign Office to President, Manchester Chamber of Commerce, January 19, 1861, *Cotton Supply Reporter*, Feb. 1, 1861. On the stability of Palmerston ministry see Silver, *Manchester Men and Indian Cotton*, 99-157; Writing to Wood on Sept. 2, 1861, Canning wrote that the continuing dislocation of supplies might lead to a "violent and unreasoning cry as regards cotton cultivation in India" and that under "a real cotton famine and its consequences with no prospect of early relief from America," the nation and Parliament might follow the cotton interests. Indian Papers of Lord Canning, Letters to the Secy. of State.

46. Wood to Denison, Aug. 18, 1861, IOLB, VIII, 206; Wood to Clerk, Aug. 30, 1861, *ibid.*, 244; Wood to Clerk, Sept. 14, 1861, *ibid.*, 270.

BOOK REVIEWS

B. M. Bhatia : *Famines in India, 1850-1965*, Asia Publishing House, Bombay, 1967 ; 389 pp ; Rs. 30/-.

The first edition of this book was reviewed in the July 1966 number of this journal (Vol. II, No. 3). The original text has undergone only minute changes, but some 20 pages have been added to it. Chapters VIII to X which dealt with the Indian economy, famines in India and administration and economic policy from 1896 to 1908, have now been extended up to 1914. There is also an additional chapter on India's food problem and the policies after independence, which consists of a rapid and not always accurate summary of some of the recent works on these problems. Professor Bhatia makes no attempt to answer a very important question that the post-Independence experience suggests, namely, to what extent it justifies his thesis that the famines of the British period could partly be blamed on the transformation of India into the "classical colonial pattern" by 1860. He does not deal with this point directly, and from indirect evidence his views on this seem rather confused. He says that the present food situation in India is "a mere extension of the famine problem that has been with us for long", but he also asserts that the most important cause of our difficulties has been the increase in the effective demand for food, which is hardly a continuation of past trends as he himself has analysed them.

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DHARMA KUMAR

Alan R. Beals and Bernard J. Siegel : *Divisiveness and Social Conflict, an Anthropological Approach*, Bombay, Oxford University Press, 1967, pp. x + 185, Rs. 37.50.

This book is one among several recent attempts by social anthropologists to evolve a conceptual framework for the study of social conflict. Like many others both within and outside the discipline, Beals and Siegel feel that social anthropologists have concentrated too much on the study of adaptive behaviour in stable societies. Where conflict has been studied the objective has generally been to discover some social function in it. The authors of this study try to approach conflict in a manner which will carry them beyond the "structural-functional" point of view.

It would be unfair to judge this work on the strength of its empirical material for this is an anthropological study with a difference. We are presented here with empirical material from two societies which are widely separated both geographically and culturally, viz., Namhalli, a village near Bangalore and Taos pueblo in New Mexico. As one would expect, the material is spread rather than thin.

The authors "do not pretend to offer a systematic theory of conflict or even of divisiveness" but they do present a rather large array of concepts. Among the interesting features of the book is a typology of conflict and the development of a distinction, made in an earlier work, between pervasive and schismatic factionalism. While in themselves these ideas have little novelty, those who are interested in the study of conflict in small communities will find in this work a useful compendium of concepts in current use.

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