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EDITED BY A. W. HASLETT

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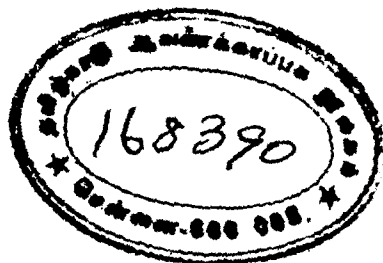
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CONTENTS

REPORT FROM GENEVA A. W. Haslett	7
MORE FLUORINE OR LESS? Joan Banus	27
DIVERSION OF DRAINAGE BY ICE G. H. Dury	48
THE PHYSIOLOGY OF SURVIVAL AT SEA G. R. Hervey	72
THE ALGEBRA OF PERMUTATIONS H. A. Thurston	90
RESEARCH REPORT A. W. Haslett	101
CORRESPONDENCE	117
SOME BOOKS RECEIVED	124
ABOUT OUR CONTRIBUTORS	128

14 illustrations are between pages 64 and 65

REPORT FROM GENEVA

A. W. HASLETT

THE best comment on the Geneva conference on the peaceful uses of atomic energy was given by the president, Dr. H. J. Bhabha, in his final statement: 'Knowledge once given cannot be taken back'. From the early stages of planning to the end of the conference there was a developing cascade – a chain reaction of confidence. The owners of information were at first cautious. Two or three months before the conference a lead was given by the United States. By the end of the preparatory stage it was clear that there would be more than the minimum of information on which judgements could be formed. Within a few days from opening, data were being released on the spot, as seemed useful or necessary.

A case in point was the exchange of information that was effected on the fission process. Before Geneva, the best figures generally available were those obtained by workers in the Dutch-Norwegian organization and freely published. The effect of Geneva was to bring together sets of figures which were of a higher order of accuracy. As a simple but dramatic illustration, the total energy released in fission was shown to be higher by 12 MeV, or about 7 per cent, than the previously published figure. To the surprise and pleasure of those who had obtained them, the different sets of figures were in such evidently close agreement that they need not before have been secret. The experience recalls that after the war when research done by the German photographic industry was made forcibly public, and firms in other countries found that various of their favourite secrets were neither their own property nor secret. There has been a continuing benefit ever since in photographic publication.

There was freedom also in discussing the relative roles of uranium and thorium as nuclear fuels, actual and possible types

of reactors, and the general lines of development which might be expected. But there remained technical details – of kinds which in normal industrial practice might be held temporarily secret – on which disclosure was not complete. An example from the Soviet Union was the reply given by Professor D. I. Blokhintsev to an inquiry as to whether the alloy of uranium used in the fuel elements of the first Russian power reactor might be a molybdenum alloy; his reply was that 'it could be'. More bluntly, a British speaker, Sir Christopher Hinton said that the exchange of information did not mean that 'we tell everybody in the world as soon as we know it ourselves – or even a little before we know it ourselves'. He was secretive on a similar point, the sheathing of uranium in British reactors.

There were also in the background the possible use of fusion reactions to yield energy under controlled conditions and, associated with this, the question of possible military use. Fusion reactions were not intended to be discussed at the conference. They were introduced by Dr Bhabha in his presidential address, presumably to draw comment from others. Thinking of a number, he predicted that a way would be found 'for liberating fusion energy in a controlled manner within the next two decades'. He drew the comment that he wanted in two embarrassed press conferences held successively by the British and American delegations. The sum of it was as might have been expected. Apart from published pure physics, everything to do with thermonuclear reactions has been, and is, subject to military security. All that could be said, therefore, was that work on the subject was proceeding, that there might even have been some progress, but that the obvious difficulties were real ones and that no one would like to put a date on it, however vague.

The question of military use was also touched on by Dr Bhabha. 'The rise of an atomic power industry in many parts of the world,' he stated, 'the development of which is necessitated by the growing demands for energy, will put into the hands of many nations quantities of fissile material, from which the making of atomic bombs will be but a relatively easy step. A widespread atomic power industry in the world will necessitate an inter-

national society in which the major states have agreed to maintain peace.' The positive form of this statement may be noted. It had seemed already before Geneva that a war between major powers was in course of being recognized as no longer practical. The line of thought was also developing that the design and operation of a nuclear reactor, and the separation from it of plutonium, would soon be within the compass of most countries, and that control by an international atomic agency was therefore unlikely to be effective. Hence the need for agreement between major states to 'maintain' peace, and not merely themselves to avoid war.

On the main issues before it, the conclusions from the Geneva conference can be shortly summarized: that atomic energy will need to be developed on a large scale if future power requirements are to be met; that known supplies of uranium and thorium are already ample on a world scale; that the cost of electricity generation in nuclear power stations is already within sight of being economic without need for special assumptions; that the disposal of radioactive fission products should present no insuperable problems; and that, while safe operation during the next ten to twenty years can be assured on entirely conservative standards, there is need for an intensive programme of research designed to obtain as direct evidence as possible of the long-term genetic effects to be expected in human populations from a given increase in background radiation. Again quoting from Dr Bhabha: 'This is too important a matter for any risk to be taken. . . . Our first duty as scientists is to establish the truth'.

Not all aspects of the conference will be covered in these notes. There was an enormous mass of material – 8½ pounds by weight – on the uses of radio-isotopes. This brought to light some intriguing new examples, including the sending back of a prototype aircraft for modification after tests of fuel-system safety, but was away from the main theme of the conference. In another direction, the effects of radiation on materials are of considerable long-term interest to physics, as well as being immediately important in the technology of reactors, but these, too, will be omitted. A third gap is processing, which is necessarily of rather specialized appeal. For those interested in these and other aspects, the full

proceedings of the conference, including discussion, should be available, in multi-volume form, early next year. The present notes will be limited to reactor types and fuel cycles, cost, health aspects, and the disposal of fission products.

REACTOR TYPES AND FUEL CYCLES

A situation in which there are many hundreds of possible lines of development of nuclear energy, as was stated by J. V. Dunworth and F. W. Fenning of the Atomic Energy Research Establishment, Harwell, leads to obvious difficulties in assessment. They are made greater by the fact that technical preferences – or, in the words of the same speakers, ‘intelligent guesses’ – may be made right or wrong at different stages according to the overall rate of nuclear power development.

A simple – perhaps deceptively simple – proposition is that for countries without facilities for the partial separation of uranium-235, the fissile isotope of uranium, ‘natural uranium must be the raw material’. If the use of heavy water is excluded for the present on the ground that nearly a ton of water would be needed for each installed megawatt of electrical capacity, this leads directly to the gas-cooled type of reactor in which the neutrons released in fission are slowed down, or moderated, by graphite, and the fuel elements consist of a heterogeneous assembly of rods – that is, to the type adopted for the first stage of the British power programme. But even here there are reservations. One is that a reactor even of this type will give a better performance if the uranium of its outer rods is slightly enriched with uranium-235 or, as might be done in later operation, with plutonium obtained from the earlier working of the reactor. The reason for this is that the flux of neutrons, and therefore the rate of fission and so of heat release, is greatest near the centre of the assembly. But for the transfer of heat to the gas used for cooling, it is better that the rate of heat release be as nearly uniform as possible in all rods. A more important reservation is that the degree of advantage possessed by reactors starting with natural uranium depends on the rate of early development of atomic energy. And this cannot yet be predicted.

Prototype power-producing reactors of commercial scale are

due for completion in Great Britain and in Russia during 1956, and during 1957 in the United States. Not until the early 1960s can stations of proved design be expected. There is then an obvious dilemma: whether to proceed at once with a power scheme, for the sake of beginning early to accumulate plutonium, which can then be used in more advanced types of reactors, or to wait on further developments. There are also differences in needs as between one country and another. In Great Britain, the need is urgent already; in India it will become urgent in 10 to 20 years time; in the United States, the effective criterion is likely to be cost for some time. So far as can be judged from Geneva, the most general reaction may be to wait for two years at least before any serious development is considered.

If total requirements are not excessive, there will then be a choice for practical purposes between the British gas-cooled reactors and types in which cooling is by ordinary water. Cost is expected to be higher at first in the latter. The combined needs of avoiding corrosion and reducing absorption of neutrons lead to the use of zirconium alloy as the material in which the uranium rods are sheathed. This adds to cost, and may lead also to a bottleneck in production. But the possibilities of general improvement are greater, especially in the efficiency with which the heat released is used, and so the time factor is again relevant. A variant, probably at one further remove, is the ‘boiling water reactor’, in which steam is raised within the reactor itself, instead of in a secondary circuit. Critics of this system want to be satisfied by its large-scale operation, though it has obvious attractions in simplicity. An American enthusiast, with arguments to defend himself, was here met by a combination of Russian and British criticism.

A general point applicable to all these types is the possibility of getting more use out of them than was at one time expected. The distinction between ‘first stage’ and ‘second stage’ reactors seems already to be blurring. By recycling uranium plus plutonium several times, up to 10 per cent of the theoretical heat output from the uranium can be realized in the British gas-cooled type of reactor, and the residual material – mostly uranium-238 – kept

for conversion to plutonium in later 'fast breeder' reactors. If 10 per cent sounds not much as a first target, it is still a useful proportion compared with the 1 part in 140 of already fissile uranium-235 which is all that natural uranium contains.

Another possibility was discussed both by Dunworth and A. T. Weinberg of the United States Oak Ridge National Laboratory. This is to rearrange the fuel system so that part of the new charge is plutonium, and the rest thorium (not itself fissile), from which further fissile material (uranium-233) would then be produced. The reactor could then proceed to a thorium/uranium-233 cycle. This might be self-sustaining or nearly, and it would matter little which, according to Dunworth. If the reactor were 95 per cent self-sustaining, the remaining 5 per cent could be supplied by plutonium. And this could be obtained indefinitely by having a small section of the reactor operating permanently on natural uranium. Here the once despised heterogeneous (or fuel rod) types of reactor would have an advantage, since they could be adapted to mixed cycle operation with a minimum of chemical complexity. There seems support here for the suggestion by Weinberg that 'the main reason for obsolescence of conventional power generation devices - low thermal efficiency - will hardly operate. Rather nuclear plants ought to be much more like hydro-electric plants: if they have sufficiently low overall operating costs . . . then it is at least not obvious why they should become obsolete any more than dams become obsolete'.

Dunworth gave a general analysis in terms of degree of enrichment and conversion factor (C), the number of plutonium atoms produced from uranium-238 per atom of uranium-235 consumed. He pointed out that a reactor using natural uranium was not necessarily more economical in nuclear fuel than was one designed for enriched fuel. He considered the case in which the used uranium is recycled with an amount of plutonium which is constant from one cycle to the next. Then, in the end, $C/(1 - C)$ atoms of uranium-238 would be used for every 1 atom of uranium-235. This proportion is 4 to 1 with a conversion factor of 0.8; but, if the conversion factor is 0.9, it is increased to 9 to 1. Hence a reactor designed for an initial enrichment, say by a factor of 2,

will do as well in ultimate fuel economy provided that accumulated conversion is also doubled. A comparison of different methods of cooling and moderating gives 'no definite indication at present of one system . . . yielding a better conversion factor than another. All seem capable of achieving a conversion factor of 0.9 initially provided that enriched uranium is used in the fuel'. Ultimate fuel economy depends on the overall use made of the neutrons produced in fission, not on initial enrichment. If atomic energy expands rapidly, this will remain a long-term argument; one must first acquire the material needed to enrich with. On a more leisurely time scale, it would allow a wider freedom to designers.

The position about the use of thorium is also intriguing. Its probable use in early-type reactors at a later stage has already been mentioned. But neither in the heterogeneous (rod) type of reactor, nor in the homogeneous reactor in which fuel is circulated in solution, could the use of thorium as such be made the basis of rapid expansion. In the former case, the provision in this way of the extra generating capacity required yearly in Britain would imply the use of 6 tons of fissile material a year for the initial fuelling of the reactors. That this is only about half the quantity estimated to be consumed yearly by the end of the century is beside the point. What matters is the high rate of commitment that would be needed in relation to power generated. Even British requirements would be significant in relation to the probable American military stock-pile; translated on to a world scale, they could not be met. In the case of a homogeneous reactor, requirements of fissile material would be smaller, say by a factor of three, but the general argument would apply.

A further point made by Dunworth may affect the time-scale of development in the case of the homogeneous class of reactor breeding from thorium, and using slow neutrons to maintain the reaction. There are many possible variants: in most of them the fertile material (thorium) and the fissile material are kept separate. Within this group again the fissile material may be circulated either as a solution in water, or as a suspension of oxide, or as a molten metal, such as uranium-bismuth alloy. Dunworth's point

was entirely general. It may be highly inconvenient for chemical processing to start with an initial feed of plutonium, which need not later be provided for. But if fissile uranium is to be used from the beginning, this must be either uranium-235, separated in a diffusion plant, probably in limited supply, or uranium-233, made in the heterogeneous type of reactor. There may thus be an interval before homogeneous reactors using thorium can be developed conveniently apart from experiment and on a small scale. Their long-term future seems, however, promising, and they are perhaps the most attractive of all general types of reactors.

There remains the 'fast breeder' type of reactor, which has long made an appeal to the imagination, and which it seems will still have an essential part to play. Its characteristic is the concentration of the reactor proper in a small central core, mainly of fissile material, in which fission is maintained by fast neutrons, that is without moderating material, and round this a blanket of fertile material. The latter may be either uranium rejected from heterogeneous types of reactors or, again, thorium. It was news at the conference that the small experimental fast reactor 'Zephyr', which has been working at Harwell since early in 1954, has achieved a 'breeding ratio' of 2. By this is meant the ratio of the quantity of plutonium produced (when working with uranium) to the combined consumption of uranium-235 and plutonium. The problem of extracting heat at the required rate from the reactor core remains formidable, and the supply of liquid metal (sodium or sodium-potassium alloy) must be reliable. The design for the Dounreay power reactor in northern Scotland was described by J. W. Kendall and T. M. Fry. The primary circuit for the extraction of heat is to be completed by 24 branches which will exchange heat to the same number of independent secondary circuits. These also will use liquid metal, and each will be driven by an electro-magnetic pump. As a further precaution against breakdown, the pumps will be grouped in pairs, and each pair will have an independent electric supply. The secondary circuits will be followed by a steam circuit and a condenser cooled with sea water.

It may be useful at this point to summarize the probable

general sequence of development. The earliest power reactors, however cooled and moderated, will have to use either natural uranium or uranium slightly enriched. These will be of the slow neutron type, using heterogeneous fuel elements. There will be a transition in cooling materials, from gas (in the British type), through water, possibly to liquid metals in the long run – though for this class of reactor the latter is by no means yet certain. The motive in these developments will be the quest for higher working temperatures and therefore greater thermal efficiencies, coupled with bigger units. As between cooling by gas and water, the balance of advantage in the early stages will be affected by the speed of initial development. Demands for extra capacity must for some time be met primarily by reactors of this class. But individual reactors may be converted progressively to a fuel system based on thorium in place of uranium. They may be then used in turn to provide the initial charge for homogeneous types of reactor using thorium. In the thorium fuel cycle, breeding at a slow rate may be effected, without need for fast breeder reactors; alternatively, the combination of both types using thorium may come out about level on balance. Use of the uranium cycle – which is essential – will lead, however, to an accumulation of uranium-238, which it seems will be usable in full only in fast breeder reactors. The overall system is bound, therefore, to be complex in its final development, with interlocking between at least three main types of reactors and scope for variety in each type.

There will also be a contrast at all stages between the consumption of uranium, and later thorium, on the basis of a steady demand for electricity, and requirements of raw materials to provide additional capacity. It has been estimated that world requirements by A.D. 2000 might be met by consuming between 1,000 and 3,000 tons of uranium or thorium a year. This would also be the rate at which fission products would be produced on a world scale. But requirements for nuclear raw materials would be greater in proportion, if conditions of expanding development are assumed and if nuclear reactions based on fusion had not in the meantime entered the picture.

ESTIMATES OF COST

Too much attention should not be paid to estimates of cost made at the present stage. The most important reservation is that no high-power nuclear reactor has yet operated. All estimates of useful life are therefore uncertain, in spite of many tests made on materials and components.

The guesses made by three countries were given on the second day of the conference. A Russian speaker, Professor D. I. Blokhintsev, thought that the overall cost of the first major Russian power station would not be much more than that at a good coal-fired power station. But this was a passing reference in a paper which was primarily an account of the small existing Russian power station. A British speaker, J. A. Jukes, amplified the approach made in the British White Paper. This involves making estimates of the future value of plutonium. His own efforts in this direction he described as 'crystal gazing rather than scientific analysis'. However, the effect of this source of uncertainty proved to be less great than might have been expected. In conjunction with the ideas on fuel cycles summarized earlier, Jukes's analysis supported the general proposition that assumptions about the price of plutonium need not be important on a long view. An American speaker, J. A. Lane of the Oak Ridge National Laboratory, gave estimates of six different types of reactors now being developed as part of the American programme of prototype power reactors. His method was to combine published estimates of capital cost with fuel and operating costs projected 15 to 20 years into the future. Clearly, this, too, involves crystal-gazing, and of the same kind.

The most significant point in the British estimate is the relatively small proportion – just over one fifth – of total cost which is left to be covered, or not, by plutonium. Jukes's figures relate primarily to the earlier British reactors, and lead to the following conclusions: (1) with no allowance at all for plutonium, the cost of electricity from nuclear power stations would be 0.76d. per unit, compared with 0.6d. per unit for coal-fired stations; (2) if the plutonium is used wholly for recycling in the same reactors, the

cost from nuclear stations may be reasonably taken as 0.65d per unit; (3) with such allowance for plutonium as could be afforded for the initial charging of later types (thorium-using heterogeneous reactors, fast breeder reactors, or heterogeneous reactors using more highly enriched uranium), the cost from early nuclear stations would be between 0.43d and 0.59d per unit, depending on the cost of separation assumed. Of the two latter figures, he thought the lower one the more reasonable.

The effect of Lane's analysis was that, in 15 to 20 years' time, all but one of the reactor types which he considered should be capable of producing electricity at a cost in the region of 0.4d per unit. On somewhat varying assumptions, he arrived at around the same figure for pressurized water reactors using ordinary and heavy water, for fast breeder reactors, and for the type of homogeneous reactor in which the nuclear fuel is in solution in water. He was less optimistic about the use of liquid-metal cooling in the heterogeneous type of reactor. But even in this case he thought that somewhere around about 0.55d per unit might be attainable. Bearing in mind the different conditions and stages of development to which they relate, these estimates are probably in as good agreement as could be expected. They support the conclusion that for countries in early need of development there is a case for beginning with the gas-cooled reactor; while for those that can wait the pressurized water reactor will offer the better prospect. For the reasons given earlier, the other types mentioned can contribute appreciably only at a later stage of development.

HEALTH ASPECTS

The problem of the long-term effects which may be produced in large populations by continued exposure to ionizing radiations has been widely discussed since the 'hydrogen bomb' test at Bikini during the spring of 1954. It arises also, if in lesser degree, in relation to the treatment of the radioactive materials that will be produced in increasing quantities as fission products through the operation of nuclear power reactors. By the end of the present century these may amount to between 2,000 and 3,000 tons a year. Questions of fuel cycles are here irrelevant; on any basis,

the quantity of energy released will be proportional to the quantity of fission products formed.

The problem has several aspects: the safe design and operation of nuclear power stations, considered as a problem in engineering; the extent of local hazard to be expected in the event of accidental routine disposal of fission products, some of which will, however, be used; and the extent to which increased general exposure to radiation can be tolerated.

The first point—that of safe design and operation—has been examined by an advisory committee of the U.S. Atomic Energy Commission, and was discussed by its chairman, C. R. McCullough. In the normal course of events, there should be no danger. The primary cooling circuit will operate on a closed cycle in every type of reactor. Purely from an engineering standpoint leakage would be wasteful; and, in a properly designed station, leakage during routine operation should be negligible. The interest is in the abnormal and its prevention—and in its results when it happens, as sometimes it must. The question is a difficult one to present in proportion. No one has yet been killed by a runaway reactor. It is unlikely that the risks, either locally or through the dissipation of radioactive material, would be significant on a world scale, even in large-scale operation. Yet the problem is a real one, and public opinion on it is real too.

The starting point is the 'nuclear runaway', a sudden and uncontrolled increase in the rate at which energy is released. This cannot, it is true, develop into bomb-like behaviour. But it could so damage the structure of the reactor as to allow the leakage from it of radioactive materials. Or it could lead to chemical reactions between different components within the reactor, and these in turn might release heat, possibly violently. Finally, although the 'runaway' must in itself shut down the reactor, the release of heat from fission products would go on. As to the effects in practice, there was a difference in emphasis between speakers. A destructive experiment on a small experimental boiling water reactor was described by J. R. Dietrich of the U.S. Argonne National Laboratory. The results looked impressive in photographs, and fragments of metal plate were thrown as much

as 200 feet from the reactor. On the other hand, practically the whole of the fuel from the reactor could be accounted within a radius of 350 feet, and no large fraction of material entered the atmosphere. On a larger scale of operation, W. G. Marley and T. M. Fry of the Atomic Energy Research Establishment, Harwell, envisaged the temporary evacuation or restriction of people living within a distance of 5.7 kilometres downwind from the place of the accident, and thought that milk from pastures up to 20 kilometres away might, for the time being, be seriously contaminated. McCullough, more dramatically, spoke of the possible necessity 'to evacuate a large city, to abandon a major watershed'. He was arguing for attention to safety in design and for continuing close supervision in operation. Fortunately, the reactivity of most types of reactors is reduced by a rise in temperature, and control systems can be duplicated. But on a world scale of operation, reactor safety can no longer be regarded as of local interest only. Whatever standards of tolerable general increase in radioactivity are adopted, the safe operation of reactors will be linked with the permissible disposal of fission products, and there will be a case, therefore, for agreed codes of practice.

The order of the problem—which does not, in fact, look alarming—can be simply calculated. On the basis of 1,000 tons output of fission products a year, and assuming only one year of storage before discharge—less than anyone concerned would think of—the eventual build-up of radioactivity in the oceans, if used as the receptacle, would be about equal to that contributed by the radioactive potassium-40 present in sea water. At 2,000 to 3,000 tons output, it would be twice to three times as great. This is not a practical calculation: on the one hand, the period of storage would be longer; on the other, if certain components were not removed, the biological effects would be greater in proportion, especially in vertebrates, and there is the possibility of concentration in marine organisms. The point for the moment is that the onus is placed on biologists, and especially geneticists, to provide the information on which long-term standards can be based.

On a short-term view, it can be said at once that the quantities involved will be insignificant on a world scale during the next 10

to 20 years; while, on a local scale, the real limitation has been set so far, not by radiological standards, but by public opinion, which has been sensitive. There is thus a breathing space for research.

Genetic Risk

The present 'official' view of the increased exposure to radiation which would be tolerable in the general population in relation to genetic risk is that expressed in a recommendation of the International Commission on Radiological Protection: that large populations should not be exposed to a dosage exceeding 0.03 roentgens a week, that is to one tenth of the level accepted for occupational workers. This is equivalent to a dosage of about 50 roentgens per generation of 30 years, the level which has been found in a variety of organisms to double the mutation rate. However, as the secretary of the commission, W. Binks, pointed out, 'large populations' should not for this purpose exceed 10 per cent of the whole population; or alternatively a bigger reduction factor should be used.

In a general review, T. C. Carter of the Medical Research Council's unit at Harwell quoted A. H. Sturtevant for the statement that 'no scientist interested in exact quantitative discussion would touch the subject, were it not that its social significance leaves us no alternative'. He summarized his own analysis of the application of mouse data to man as leading to a 'colossal' (thousand-fold) uncertainty, though possibly still not big enough to include the truth. 'Exposure to chronic irradiation at a dose-rate of 50 roentgens per generation would result in the induction of genetic deaths amounting to somewhere between two tenths and two ten-thousandths of all deaths'. To say that the answer lies in research is to state the obvious. But it has not been obvious that the obtaining of more directly applicable data was at all likely. Carter's proposal is for research at three levels, undertaken on an international scale, and taking ten years or more to complete. Studies of mutations as such should be spread over many types of cells in many different organisms, including vertebrates, and should include 'studies of the effects of ionizing radiations

when administered at very low dose rates over a long period'. The point is whether the effects are strictly additive under these conditions. Secondly, Carter pointed out that research on population genetics has lagged far behind population genetic theory. He proposed a large programme of research to determine the factors involved, especially the effects of assortative mating, and to develop a more adequate theory. Thirdly – and this is perhaps his most interesting point – Carter proposed a direct attempt to determine the relation between radiation exposure and mutation rate in man by making use of the appreciably higher background radiation in granitic areas of Great Britain and Scandinavia, compared with that in granite-free areas of the same countries. Whereas the determination of absolute (as opposed to comparative) mutation rates has been regarded as difficult enough, he thought it 'within the bounds of what is technically possible today to make estimates of the mutation rates of a few clear-cut autosomal dominant or sex-linked recessive conditions [of which haemophilia is the best known] with sufficient accuracy to enable us to set useful fiducial limits to the mutation-rate-doubling radiation dose in man'. Preliminary attempts in Sweden have not been encouraging. But that is not to say that the approach is impossible, given sufficient effort. Before the publication of these notes, it will have been discussed at an international meeting which it was announced later during the conference was to be held by the Medical Research Council at Harwell during September.*

If a programme of international research is thought practical, there will be a case for an international umbrella at the research stage, since it will be necessary that world confidence be commanded. It seems that the World Health Organization, as now constituted, has not the powers needed. They could be expanded for the purpose. Alternatively, as suggested earlier by R. M. Sievert, chairman of the International Commission on Radiological Protection, and again at Geneva, a further body could be established to cover 'the general radiation problems of the world'.

*At this meeting a programme on the above lines was thought to be difficult but possible.

LONG-TERM DISPOSAL OF RADIOACTIVE MATERIALS

The question of the long-term disposal of fission products raises further points. It is not solely a question of safety, although safety must predominate. An integrated scheme based on the combination of use, special storage, and disposal – probably at sea – was put forward by E. Glueckauf of the Atomic Energy Research Establishment, Harwell, as 'likely to be economical and self-paying'. On the basis that between 2,000 and 3,000 tons of uranium or thorium would be used a year, there would, on his figures, be a total volume of between 10 million and 30 million cubic metres of initially dilute solution to be disposed of.

There is also a special problem from the presence in fission products of the radioactive isotope krypton-85 'which, being a rare gas, is in the normal course of events released into the atmosphere'. On the basis of 1,000 tons total output, the figure to which Glueckauf worked, the equilibrium activity which would be reached would still be less than that constantly dissipated at sea level by cosmic radiation. He thought therefore that the gradual release of krypton-85 could be considered innocuous for some time to come, though at the highest levels of production its storage may become necessary. There is also a short-lived isotope of a second rare gas xenon which would be worth while to separate as a by-product. With these exceptions, the problem is how best to treat solutions.

Glueckauf's scheme begins with the concentration of fission product solutions by evaporation to one-hundredth part of their original volume. The separation of strontium-90 and caesium-137 follows. This is for the combined purposes of use and to get rid from solution of two long-lived isotopes which would contribute heavily, if released with the rest, to the total build-up of radioactivity. The recovery of caesium-137 for use in medicine and industry as a source of gamma-radiation is expected to pay for its own separation and also for the initial process of concentration. Strontium-90, though usable on a short-term basis in industry, would be relegated in the long run to use as a heat source. Both, after 50 years, would be disposed of in the ground or sea in the

form of baked clay units, from which the activity could not be dissolved, though for 1,000 years it would still be significant. The concentrated solution of other fission products would be stored for 13 years. One of Glueckauf's most interesting proposals then follows. The rare metals ruthenium, rhodium, and palladium, now of low activity, would be separated for use, after further storage if necessary. He placed their annual value at £250,000, and saw a further contribution to costs from this source.

The residual solution, if dispersed uniformly in the oceans, would have an equilibrium activity less than one-thousandth part of that of natural potassium-40. 'One of many possible suggestions would be to drop these solutions in plastic containers into the deepest parts of the ocean, where because of their high specific weight (greater than 1.5) they would sink and become embedded in the mud of the sea bottom. As the activity is no longer sufficient to cause destruction of the resin, the release into the ocean by occasional leakages would be so slow that no appreciable local build-up of activity would be expected'. Glueckauf's figure for equilibrium activity would be increased by a factor of 2 or 3 on the basis of the higher estimate of total output already mentioned; and there will remain for consideration the question of biological concentration, on which not enough work has yet been done. But on any reasonable basis of tolerable increased level of general irradiation, it looks as if there need be no hindrance from disposal to the full development of nuclear energy.

CONCLUSION

The general outlook seems to be set fair for atomic energy, although there are some reservations which must be made. One is about schemes of hydro-electric development. It seems that water power, where most easily harnessed, will be cheaper for a long time than atomic energy, and perhaps will always be so. There is therefore no case for holding up on immediately promising schemes. Another is about oil and coal. Both are raw materials as well as fuels, and their uses as such are increasing. Both also have advantages as mobile sources of energy, and in direct heat-

ing, with which atomic energy looks unlikely to compete. Neither, therefore, can be neglected. It may also be of advantage to combine oil or coal with atomic energy. A nuclear reactor might be used as a first source of steam, and this might then be superheated, using heat from an oil- or coal-burning furnace. In this way, the nuclear reactor could operate at a lower working temperature without loss in final efficiency. A last reservation is about the so-called under-developed countries. Both in trained staff and in capital, a power programme based on atomic energy is more immediately exacting than is one based on coal or oil. But neither trained staff nor capital are plentiful on a world scale, and the first effect of atomic energy may well be to increase differences rather than to reduce them. In no one of these points is there anything against the early development of atomic energy, which in relation to total energy requirements will soon be necessary.

Organization was outside the scope of the conference, but was discussed informally. It was clear in advance that the odds were against those who had hoped for a strong international agency responsible for all aspects of the peaceful development of atomic energy. This impression was reinforced strongly at Geneva. In the first place, none of the three countries most concerned seemed to have wanted an agency 'with teeth'. In the case of the United States and Great Britain, there was an element of commercial competition; this was most clearly evident in an exhibition organized under independent auspices in the city of Geneva while the conference met on the slightly more remote height of the Palais des Nations. In the case of the Soviet Union, there was a wish to help their own friends in their own way, as might be said also of the United States, and on a smaller scale of Great Britain. Secondly, the case for a strong agency was shown to be less good than some had supposed. Two of the reasons for this are implicit in the reservations mentioned in the previous paragraph. From a national point of view it is impossible to consider one source of power independently of others; they are essentially related. Neither can power stations be considered independently of power-using industries. This applies especially to the under-

developed countries, for which the limiting factor is the total effort that can be made. But it is true also, for example, of Great Britain. A third reason is technical. The path from first-stage reactors, which produce plutonium, to later reactors, which will use it, has become both easier and more complicated. Indeed the whole idea of distinguishing rigidly between one and another stage of development is becoming unreal, as the section on reactor types and fuel cycles will already have shown. The importance of the value to be placed on plutonium is thus less; and so, too, is the need for assurances to cover the first stage of development. It is unlikely also that plans for large-scale development could be hurried. Britain, with the reactor type which in theory can be put most quickly into service, is limited in practice by resources; the United States, with more ambitious types, almost all of them involving the use of zirconium, is likely to be limited by shortage of this material;* and the Soviet Union – like the United States at the stage of prototype design – is unlikely to be better placed. In short, there is neither the need for inducement nor, if inducements were offered, could they be made quickly effective on a large scale.

What is emerging is a mixture of individual arrangement and international co-operation: individual arrangement on the supply of research or demonstration reactors, and of technical assistance and information; international co-operation on the supply of fissile materials, with which different countries could experiment on a small scale with various of the more advanced types of reactors described at Geneva. Discussions on the form which an international agency might take were begun immediately after the conference and have since been continued. One proposal (September, 1955) is that there should be a non-profit-making organization, with a committee representative of sixteen nations, including the 'atomic energy countries' – Canada, France, Great

*Output of zirconium in the United States during 1955 is provisionally estimated at 350,000 lb. On the basis of present plans, output during the period 1957-60 will be increased to some 600,000 lb. per year 'to meet increasing requirements'. But this will still be less than the output reached in 1954, when a government-owned plant was in operation.

Britain, the Soviet Union, and the United States. As well as the allocation of fissile material, and arrangements for its proper use, it is envisaged that the proposed organization should act as a centre for information on atomic energy; and – a point on which there is general agreement – that the exchange begun at Geneva should be continued through further conferences.

MORE FLUORINE OR LESS?

JOAN BANUS

ONE of the less publicized objects of the alchemists was to find Al Kahest, the Universal Solvent. A good many workers, one would imagine, soon lost heart when it occurred to them that they might find difficulty in storing it. The early workers with fluorine thought themselves to be in a similar predicament, when they found that fluorine gas sets fire to rubber or wood, reacts with glass and with all metals, and takes asbestos to red or white heat. Nowadays elementary fluorine is handled in metals or alloys such as brass or steel which form a protective coating of the fluoride, and these were used, for example, on the atomic energy project, where uranium hexafluoride, which is volatile but also very reactive, was used in diffusion processes for separating the isotopes of uranium. Oils and greases and gaskets resistant to fluorine were also required, and this led to the development of the highly inert and chemically resistant compounds of fluorine, the fluorocarbon oils, and the polymer polytetrafluorethylene. So we begin with the paradox that fluorine, the most reactive of all the elements, forms many compounds that are among the most inert that we know; the fluorocarbons have been compared to the rare gases in this respect.

The importance, and the danger, of fluorides in the body, has been the subject of recent controversy. This is not a question of the reactivity of elementary fluorine; common salt has none of the lethal properties of chlorine gas (which is in any case used quite safely in small quantities as a disinfectant in water supplies). But fluorides, such as sodium fluoride, in large doses (5 to 10 grammes) are lethal, in somewhat smaller regular doses are dangerous to health, causing crippling fluorosis, whereas a small regular intake increases the resistance of teeth to decay. The problem at issue is, how much, and what to do about it?

FLUORIDES IN NATURAL WATERS

The commonest mineral fluoride is fluorspar (fluorite, calcium fluoride), which is mined in Derbyshire, where it is called Blue John when it is blue or purple, although it is usually colourless and known as Derbyshire Spar. The name 'fluores' was used by Agricola in 1529, and comes from the Latin 'fluere', meaning to flow, as calcium fluoride has a relatively low melting point, 1330° C, and is used as a flux in the refining of metals such as copper, lead, and gold, and also in the making of certain kinds of glass. Other mineral fluorides are cryolite, which is found in Greenland, and fluoroapatite, calcium fluorophosphate, which is present also in teeth and bones.

The fluorine content (measured as fluoride ion) of natural waters varies very widely. Recent figures for drinking water in different districts in England range from nil in, for example, Cardiff, Edinburgh, and Oxford, or 0.1 to 0.35 parts per million (ppm) in London, depending on whether the source is the Thames, the Lea, or the New River, to 0.6 ppm, for example, in Harrogate. Sea water contains about 1.15 to 1.30 ppm. One of the highest concentrations in the world is 24 ppm in the Salt-pan areas in South Africa. The low solubility of calcium fluoride usually places an upper limit on the amount of fluoride in solution in natural waters. Distilled water can dissolve up to 16 ppm of calcium fluoride (7.8 ppm of fluoride), but hard waters do not contain more than about 2.5 ppm of fluoride. These figures are relevant, as will appear, to the health and appearance of teeth. One can have too little or too much fluoride even in natural waters.

FLUORIDES AND TEETH

Although the disease known as dental caries (tooth decay) affects 98 per cent of the population of Great Britain, we still have no precise knowledge of its fundamental causes, nor is there any one means of preventing it. Its incidence is reduced by fluorides in the water supply, but this is only one factor out of several.

For many years the commonly accepted explanation of dental caries was the 'chemico-parasitic' theory put forward in 1890.

According to this, oral micro-organisms such as *Lactobacillus acidophilus* (isolated in 1925) decompose carbohydrates to form acids which attack the enamel and, when this has been breached, the underlying dentine. The process is known as decalcification, since the acids dissolve away the calcium hydroxyapatite and carbonate. The hydroxyapatite accounts for 95 to 98 per cent of enamel, and for about 75 per cent of dentine, the remainder being water and organic matter. But the structure of the tooth affects the susceptibility to attack, and for this reason and others, many workers have come to believe that the organic constituents of the enamel are first attacked (proteolysis), and that decalcification follows.

Diet is very important, for the structure of teeth is weakened by shortage of calcium, phosphorus, or vitamin D. In addition, excellent raw material for the production of mouth acids is afforded by refined carbohydrates, as in biscuits or cake, or by 'the constant sucking of lollies and other sweets', as the Ministries of Health and Education have lately expressed it. The incidence of caries has been found to be less in some areas of chronic malnutrition, for example in parts of Africa. The explanation in some of these cases is that, although the customary food may be low in tooth-building factors, it will be self-cleansing, lacking the more 'civilized' refined carbohydrates. And a further factor of importance is fluoride, through its effect on hydroxyapatite, about which more will be said later.

As early as 1874, a Dr Erhardt, of Emmendingen in south-west Germany, described an experiment in which a molar tooth was drawn from a dog, which was then given small doses of potassium fluoride for four months, after which the opposite molar was pulled out, and found to be denser and harder. Dr Erhardt recommended the sucking of one fluoride pastille a day (these could be obtained from the Apotheker Gebrüder Keller in Freiburg for a mark a box) for protection of the teeth against decay. He stated that such a practice had been recommended for several years in England, but in view of the lack of references in the medical literature before 1892 this seems unlikely. In that year Sir James Crichton Browne, in an address to members of the British Dental

Association, stated that a supply of fluoride while the teeth were developing was essential to the proper formation of the enamel, and suggested that fluoride in suitable form should perhaps be added to the diet.

These doctors were considerably ahead of their time, for the connexion between fluorides and teeth was not established until the early 1930s. In 1916 G. V. Black, and after his death F. S. McKay, a dentist at Colorado Springs in the United States, began to investigate the mottling of teeth that was observed in 80 per cent of the permanent inhabitants, though not in those who had come to live there as adults. The classical example in this country is at Maldon in Essex, described between 1928 and 1933 by N. J. Ainsworth, who found that dental caries was less common than elsewhere both in deciduous teeth (milk teeth) and in permanent teeth, although according to him only the latter were mottled. In the early 1930s workers in many different localities, in England and America, showed mottled enamel to be a result of a certain concentration of fluorine, in the form of soluble fluorides, in the local water supply, 2.5 ppm at Colorado Springs and 5 ppm at Maldon. In the United States the phenomenon was first noticed in immigrants from Naples, a volcanic area with a high fluoride content in the drinking water. The condition is now known to be widespread; mottling of teeth is found, for example, in areas (usually rich in phosphate) in California, Florida, South Carolina, Tennessee, Texas, and South Dakota; and in the Argentine, North Africa, southern China, southern India, several South Sea Islands, and the West Indies.

The connexion between fluoride in water supplies and low incidence of dental caries was established during the same period, the early 1930s. Figure 1 shows the inverse relation between the number of decayed, missing, or filled permanent teeth (known as the DMF rate) per 100 children, and the fluoride concentration in the public water supply; it is taken from the studies of H. Trendley Dean and his co-workers on 7,257 school-children, aged 12 to 14, in twenty-one American cities.* This work was officially

*The Ministry of Health has recently announced that the DMF rate for 5-year-olds in Great Britain is now 5.

sponsored and was written up in U.S. Public Health reports, 1933 to 1942. Figure 1 shows, firstly, that the DMF rate was 50 to 60 per cent higher when the water supply contained little or no fluoride than when it contained 1.0 to 1.5 ppm; and, secondly, that there was no appreciable advantage in raising the concentration above 1 ppm, at which level mottling of the enamel is absent or can only be discerned by expert examination.

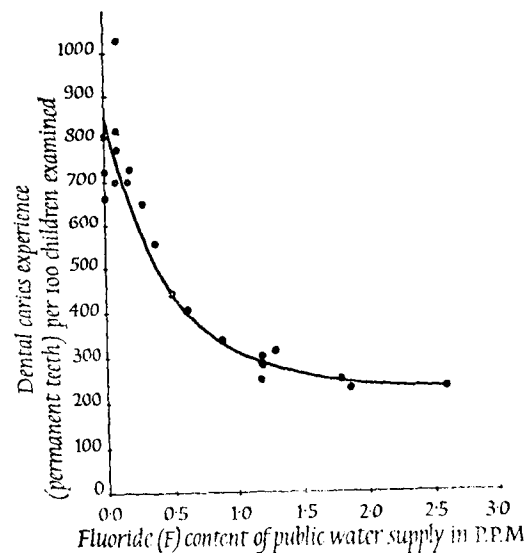


Fig. 1. Relation between the amount of dental caries (in permanent teeth) in 7,257 selected 12-14 year old white school-children of 21 cities of 4 states in the U.S.A. and the fluoride (F) content of the water supply. (After Trendley Dean.)

There have been no English studies on this scale, partly because the variation in fluoride content of natural waters is much smaller here. However, R. Weaver, in 1944, made an interesting comparison between two groups of children in South Shields, where the water supply then contained 1.2 to 1.8 ppm of fluoride, and two equivalent groups in North Shields, where it contained

0.25 ppm. He examined 500 children aged 5 and 500 aged 12 in each case, and found only half as much caries in both the deciduous and the permanent teeth of the children of South Shields as in those of North Shields. Only two of the 1,000 South Shields children showed staining of the teeth. At West Hartlepool, where the water contained 2 ppm of fluoride, there was less caries again, but here there was mottling of the enamel which was in some cases disfiguring.

The danger of dental caries, and the benefit derived from fluoridation, are greatest in newly erupted teeth, that is, when the enamel is most permeable to mouth fluids. But there is evidence, too, that the resistance noted in children carries on into adult life. Figure 2, which is also from Trendley Dean, shows that tooth loss per person at Colorado Springs is much lower at all ages up to 44 years than the standardized rates for the United States. Weaver found the benefit in adults to be less great. But in North and

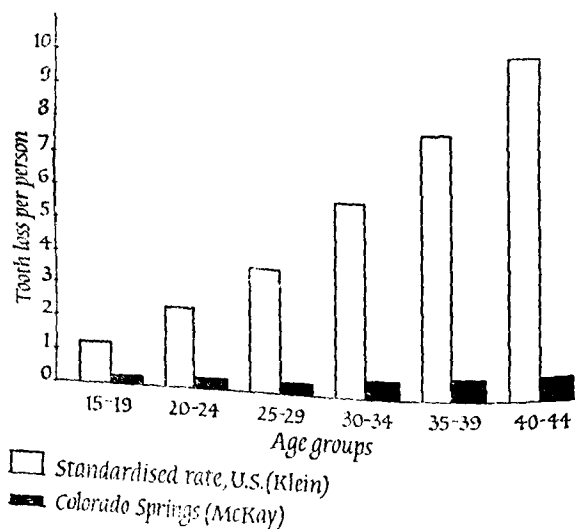


Fig. 2. Tooth loss at Colorado Springs compared with standardized rate for U.S.A. (After Trendley Dean.)

South Shields in 1951 – when the fluoride contents were respectively 0.82 and 0.07 ppm – J. R. Forrest and her co-workers assessed the effect on adults in South Shields as equivalent to a delay of ten years in the progress of caries. The DMF rate was reduced by 38 per cent at ages 21 to 25, and by 16 to 17 per cent at ages 36 and over.

Mottled enamel in its mild form is known as white mottling. It develops while the teeth are still wholly in the gums and is correlated with 2–4 ppm or more of fluoride in the drinking water. Opaque white spots or streaks can be seen on the teeth, although often only by the trained eye. With high fluoride concentrations, usually above about 5 ppm, the mottling becomes brown and unsightly, and in cases of severe mottling the teeth may be black, and pitted and deformed. Staining occurs soon after the ‘cutting’ of teeth, and is thought to be due to the fixation of coloured metal ions, such as iron or manganese, fluorosis rendering the teeth more permeable in this respect, although more resistant to caries.

The part played by fluoride in teeth is still a matter of controversy, and much work on it continues. It is known that fluoride ion can replace hydroxide ion isomorphously (i.e. without affecting the structure), and reversibly (i.e. if the percolating water contains less fluoride, some of the fluoride already absorbed will be washed out again). This occurs in minerals such as silicates (e.g. topaz, some micas) and hydroxyapatites, which partially form fluosilicates and fluorapatites, and also in bones and teeth. This process is exploited in the fluorine dating of bones, which helped to prove that ‘Piltdown man’ was a hoax.* The extent of replacement in living teeth depends on the amount of fluoride ingested, and is related to the appearance of the teeth. ‘Normal’ enamel contains about 0.02 per cent of fluoride, and dentine 0.025 per cent; mottled teeth in Maldon averaged 0.032 per cent and 0.072 per cent respectively. The proportion in pure calcium fluoroapatite is 4 per cent. This limit is never approached in teeth, and so long as the enamel remains permeable, replacement can continue. One effect is on solubility; calcium fluorapatite and fluoride in enamel or dentine are less soluble in water or acid than are the

*See, for example, *Science News* 31, pp. 117 (1954).

hydroxyapatite and carbonate. There may be an effect also of surface structure, and fluorosed teeth, although harder and denser, are more brittle than 'normal' teeth.

TOO MUCH FLUORIDE

Endemic fluorosis

It is only in recent years that chronic fluorosis has been recognized as the cause of certain endemic diseases the symptoms of which have been known for centuries; for example El Darmous, affecting animals and men in certain parts of Algeria, Morocco, and Tunis, where there are phosphate deposits containing fluoride. Similarly in Kweichow, in southern China, where the well waters contained up to 13 ppm of fluoride (the surface waters little or none), and a whole village is said to have moved, because the people suffered from 'stiff backs', sometimes even from the age of 10. Bones are more labile than teeth, and the replacement of hydroxide by fluoride goes further. A case is recorded in Kweichow of a man aged 37 who broke his neck falling off a low rock, and whose bones were found to contain between 14 and 20 times the 'normal' amount of fluoride. In this sub-tropical climate the water consumption is high, which would increase the fluoride intake still further. Endemic fluorosis was also studied in the Nellore district of southern India, where investigators concluded that serious manifestations were evident when the water supply contained about 7 ppm or more of fluoride. Malnutrition is an aggravating factor in these areas. In the United States, with generally better nutrition, concentrations of 8 or more ppm have been known, but only one case of 'stiff back' has been reported.

Industrial fluorosis

Fluorides can be absorbed also through the lungs as fine dusts or vapour, and are thus a health hazard in industries which use them. The most important fluorine compound in industry is hydrofluoric acid, which is made from fluorspar. It is used, for example, together with aluminium hydroxide and sodium carbonate, to make synthetic cryolite, of which the only major deposit, in Greenland, is insufficient to meet the demand. Cryolite is used

in the electrolytic process for making aluminium metal, and also in the wood and glass industries. Other important uses of hydrofluoric acid include the etching of glass, since it dissolves silica, and the removal of sand from castings, for the same reason. It is used also for the production of organic compounds of fluorine. Among these are inert, non-toxic compounds used as refrigerants, highly active and toxic substances developed as insecticides and rat poisons, and the polymer 'Fluon' ('Teflon' in the United States), which is thermally and chemically resistant, has good electrical properties, and also low adhesiveness. Of inorganic fluorides, some are used as antiseptics and preservatives; others, as already mentioned, as fluxes in the metal and glass industries, and in the making of enamels and opal glasses. C. O. Sappington in 1943 listed twenty-eight occupations in which workers were exposed to fluoride hazards. They may be expected to become less serious in practice as knowledge of them is spread, and is applied from the beginning in the design of safe industrial plants. This is true already in relation to the more 'modern' uses of fluorides.

The body has three mechanisms by which it disposes of excess fluoride: firstly, vomiting and diarrhoea, for large doses; then excretion (urine, faeces, and sweat), which is rapid and selective (i.e. fluorides behave differently from chlorides and bromides); and thirdly skeletal deposition. At fairly high dose rates one half to one third of the fluoride ingested is deposited in the skeleton, but at lower rates of intake this is lost slowly, over weeks or months, by excretion. Urinary analysis for fluorides is accordingly useful in diagnosis.

A study of cryolite workers in the aluminium industry in Copenhagen was published by K. Roholm in London as long ago as 1937. He described skeletal changes, such as immobility of the ribs (which leads to difficulty in breathing), and the condition known as 'stiff back' or 'poker back', both of which were due to the intake of large quantities of fluoride. The amounts of fluoride in the enamel and dentine of their teeth were 0.2 and 0.83 per cent respectively, roughly ten times as much as had been found in mottled teeth at Maldon. In bone, with more rapid replacement, up to 1 per cent was found (cf. 0.2 per cent for the average

Londoner at 40 years of age). On X-ray examination the bones showed increased density, osteosclerosis (abnormal hardening), and exostoses (extra growths). Four out of five workers suffered from gastritis. However, we still have little knowledge of the effects of high intakes of fluoride on parts of the body other than tooth and bone, and it is to be hoped that more attention will be given to this aspect of the subject. Professor M. Murray and her co-workers at Bedford College in London have recently demonstrated that the production of hydrochloric acid in the stomach of the cat when dosed with histamine is inhibited by fluoride (in large doses), and that this also leads to kidney trouble in rats.

As well as the uses of fluorides as such in industry, there is a large incidental production of fluoride in the manufacture of superphosphate fertilizers, and it was calculated in 1946 that more than 4,400 tons of fluoride was being emptied into the atmosphere of the British Isles annually from this source alone. The roasting of certain iron ores and clays has been another source of contamination.

When cattle graze on pasture exposed to air polluted with fluorides, their intake of fluorine can be very large; they show symptoms of fluorosis similar to those of fluorosis in man. Lactating cattle are the most vulnerable, and cows with acute fluorosis of the bone will kneel to eat. Near a brickworks in Bedfordshire where fluoride-bearing marine clay was burnt, fluorosis in cattle was reported in 1941, and there were respiratory troubles among farm workers. A well-documented case is that of Fort William, which was investigated by the Medical Research Council after a crofter in the neighbourhood of an aluminium factory had complained that an unknown disease was attacking his sheep. It was found that volatile fluorine compounds and cryolite-containing dust in the smoke from the factory were responsible, and also that many of the furnace men showed symptoms of fluorosis; thus 18 per cent of them had stomach troubles, and 25 per cent abnormal radiograms of the bone. In the neighbourhood there was slight mottling of teeth. Similarly in a district in Lincolnshire, where iron ore containing 1,200 ppm of fluoride was calcined in the open with coal containing 100 ppm, there was fluorosis of the

bone in inhabitants of a local farm, and the windows were found to be etched by the hydrofluoric acid.

Apart from the individual efforts of manufacturers, there is now a legal limitation: the Alkali, etc. Works Regulation Act of 1906 has been extended to include fluorine in the list of noxious or offensive gases, the discharge of which into the atmosphere is controlled.

LEVELS OF FLUORIDE INTAKE, AND THEIR CONSEQUENCES

The titles and tables of contents of three symposium volumes of the American Association for the Advancement of Science (the A.A.A.S.) give a good indication of the progress of the deliberations in that country as to the optimum level of fluoride intake. *Fluorine and Dental Health*, in 1942, gave a description, aetiology (study of causation), and epidemiology (study of the incidence in populations) of mottled enamel, and described the positive correlation between the amounts of fluoride ingested during tooth development and increased resistance to dental caries. *Dental Caries and Fluoridation*, in 1944, recorded the beneficial effects of the ingestion during tooth development of optimal amounts of fluoride from communal water supplies, and discussed the feasibility of the systematic introduction into water supplies of fluoride to an optimal level of about 1 ppm. The most recent volume, *Fluoridation as a Public Health Measure*, 1954, discusses fluoridation to about 1 ppm as an established fact in many parts of America.

The effect of different levels of fluoride intake are summarized in the table below, which is from the 1954 volume.

The Effects of Fluoride Intake at Different Levels

Category	Dose Frequency	Amount	Time	Clinical Effect
(1) Acute poisoning ..	Single	5-10 g	2-4 hr.	Death
(2) Chronic high-grade poisoning	Daily	20-80 mg	10-20 yr.	Crippling fluorosis
(3) Chronic low-grade poisoning	Daily	2-8 mg	1st 8 yrs. of life	Mottled enamel
(4) Preventive dentistry	Daily	1-1.5 ppm in water	"	Decreased dental caries

The factors of safety, when water is fluoridated to a level of 1 ppm, are estimated for categories (1) to (3) to be 2,500 to 5,000, 50 to 100, and 2 to 5, respectively. Several questions suggest themselves. What about the hazards of industrial fluorosis which have just been discussed? What about other sources of fluoride, such as food? And is the factor of safety for mottling big enough?

On the question of industrial fluorosis, it is a possible argument that, if some sections of the population are exposed to a fluoride hazard while at work, this should not be increased by adding fluoride to water supplies. One answer is that industrial fluorosis is a problem, or series of problems, in its own right, which must in any case be dealt with. Another is that, in relation to category (2), the amounts of fluoride added to drinking water are too small to be significant.

The question of food is more simply answered. Although fluorine is a ubiquitous element, most foods contain rather little of it. Exceptions are tea and sea foods, especially tinned sea fish, of which the softened bones are edible and contain more fluoride than do the bones of land animals. Tea leaves, depending on the type of tea, may contain between 10 and 180 ppm of fluoride. Those who take their whole fluid intake in tea of high fluoride content can just about obtain the required quantity independently of water supplies. This is so also in those few places, of which the island of Tristan da Cunha is one, where the diet consists largely of fish. But independent estimates made in the United States and in Britain suggest that the total daily intake from food is likely to be about 0.3 milligrammes of fluoride, or about one seventh of the intake recommended (e.g. in the 1954 A.A.A.S. volume) for resistance to caries without dental fluorosis. The deficiency would be made good by 1 ppm of fluoride in the drinking water.

As to the remaining question, the factor of safety for incipient white mottling is indeed low. However, the appearance of slightly fluorosed teeth is claimed by enthusiasts for fluoridation to be an improvement on the yellower colour of 'normal' teeth. It is pointed out, too, that water supplies containing 1 to 5 ppm of fluoride lead to no detectable alteration in bone structure; and that such minor changes, e.g. increased opacity to X-rays, as have

been found in a small percentage of people drinking water containing 8 ppm of fluoride, are not known to be harmful.

Recent English studies show that, when the fluoride level in the drinking water is 1 ppm, about 95 per cent of the ingested fluoride is excreted, leaving only about 5 per cent to be deposited in bones and teeth. But even if twice as much were deposited cumulatively, this would still amount to less than 0.01 per cent of fluoride in the bones of a man of 70. Here again, supporters of fluoridation suggest that, far from being a hazard, a reasonably high fluoride level in bone may possibly be beneficial, for example in reducing the demineralization that occurs with advancing age, and which makes bones more brittle. Foetuses and babies are protected from fluoride: foetuses by the placenta, which seems to act as a barrier, and babies by the small amount of fluoride which passes into the milk even if the mother has a high intake of fluoride.

The medical and dental effects of different levels of fluoride intake are illustrated in the results of two American surveys, one in the towns of Bartlett and Cameron in Texas, the other in Kingston and Newburgh in New York State.

Bartlett and Cameron are neighbouring towns which had fluoride concentrations of 8 and 0.4 ppm respectively in their water supplies. About 120 adults from Bartlett and a similar number from Cameron, all of whom had been in residence for at least fifteen years, were subjected to medical, dental, and X-ray examination in 1943 and again in 1952, after which the Bartlett water was defluoridated to a level of 1 ppm. The only significant differences that could be found were that the incidence of dental fluorosis (mottled teeth and increased resistance to caries) was markedly higher in Bartlett than in Cameron, and that cardiovascular abnormalities were slightly higher in Cameron, although in this case there was no pattern of association to the effect of fluoride. It was concluded that there were no clinically significant physiological or functional effects from prolonged ingestion of water containing excessive fluoride (in 1953 the people that formed the Cameron sample had been using water with 8 ppm fluoride for an average of thirty-seven years), except dental fluorosis.

In Newburgh and Kingston, cities of 30,000 people, the water

supplies contained very little fluoride prior to 1945, after which the Newburgh water was fluoridated (1.2 ppm). Up to 500-600 children from each city were given intensive examinations before fluoridation and afterwards. These examinations continue. No indication has been found of any systemic effects, adverse or beneficial, of the fluoridation of the Newburgh water, except that by 1949 there was a 32.5 per cent reduction in the DMF rate for 6-12 year olds, and a 115 per cent increase in the number of 5-6 year olds whose deciduous teeth were free from caries.

Similar comparisons have been made in Marshall and Jacksonville, Texas, in Grand Rapids and Aurora, Michigan, in Brantford, Ontario, and so on. Other studies have compared growth-rates and experience of bone fractures in high and low fluoride areas, as well as the death rates from diseases such as diabetes, cancer, nephritis, and those of the heart. No significant differences have been found. A similar survey in 1954 of sixty-four cities in sixteen states, in many of which areas the fluoride content of the water was several ppm, added to this list intercranial lesions and cirrhosis of the liver.

OTHER WAYS OF SUPPLYING FLUORIDE

Assuming that an extra supply of fluoride is accepted as desirable, a choice is still possible between different ways of supplying it. Various foods have been considered, as well as table salt, but all have drawbacks. One difficulty is to provide adequately for infants and young children without the possibility of too much fluoride being given. Another is that communities with enough fluoride in their water supplies should not be given extra in food, although only a few miles away there may be need for a supplement. In the 1954 A.A.A.S. symposium volume, it is concluded not only that public water supplies are the best vehicle, but that, where there is no public supply, bottled fluoridated water is the only procedure that is worth taking seriously. But the United States is more used to bottled drinking water than we are in Britain.

There have also been many studies of the 'topical' or external application of fluorides to the teeth, for it is not clear to what extent fluoride is incorporated in the teeth during tooth forma-

tion, as opposed to being taken in from outside soon after eruption, when the teeth are most permeable. The latter process is certainly known to take place; for example, a group of Japanese children who moved from an area with 0.1 ppm fluoride in the drinking water to another with 3 ppm showed a 60 per cent increase in caries resistance in previously erupted non-carious teeth. This is confirmed in Newburgh, and in Brantford, Ontario (40 per cent increase).

Experiments on the absorption of fluoride from outside can be done on individual teeth in test-tubes. They confirm that fluoride, absorbed in this way, reduces the solubility of the tooth. Electron microscopy, and X-ray and electron diffraction, show that fluoride picked up from dilute solutions is fixed as fluorapatite, even in severely mottled enamel, while fluoride picked up from concentrated solutions is fixed as calcium fluoride.

It is possible also that fluorides, which are often used as anti-enzyme agents, may reduce the incidence of caries by inhibiting the production of acid from carbohydrate foods by lactobacillus, and by enzymes known to be associated with this reaction. There is, however, little evidence that fluorides reduce the growth of micro-organisms in the mouth, although a recent study in Illinois showed that lactobacillus counts of untreated carious surfaces were lower when the water supply contained more fluoride.

Dentists have tried treating the teeth with fluoride solutions, and B. G. Bibby and other workers in America report that resistance to caries is increased 40 per cent by say four applications of fluoride solutions, over a period of a year, to the teeth, if they are scaled and polished first. Only the outer layers of the enamel are affected. The process is most effective in newly erupted teeth, but is also beneficial to adults. The Fluoride Working Party of the Ministry of Education has been conducting similar experiments (with fluoride solutions) on schoolchildren of Dundee, Edinburgh, and Fife, and has noted a resultant reduction in the caries rate, but 'not as much as was originally hoped'. A longer time is needed before final conclusions can be drawn.

Naturally enough, the use of fluoridated toothpaste, tooth powders, mouthwashes, pastilles, and even chewing gum, has

been canvassed, but there are objections to all of these, especially the last. It is difficult to sell a product containing enough fluoride to be effective without running the risk of excess by large consumers, especially when these are children. Radioactive fluoride has been used to show that teeth will pick up fluoride from toothpaste, but Bibby, in a two-year study, found no reduction in caries from the use of fluoride-containing toothpastes, and the Ministry of Health has lately advised that their use should be discouraged.

FLUORIDATION IN AMERICA

In the last few years many communities in America have been fluoridating the public water supply, usually to a level of 1 ppm. This process is most advanced in Wisconsin, where, by 1953, 80 per cent or more of the population was consuming fluoridated water. It is estimated that by the end of 1955 30 per cent of the inhabitants of the United States, or 60 per cent of those receiving public water, will have 0.9 ppm or more of fluoride in their water. Of these only 4 million receive water that contains 1 ppm of natural fluorides. In some localities, notably in South Dakota and Texas, fluoride has to be removed; the legal maximum is now 1.5 ppm. In Charlotte, North Carolina, the fluoride is reduced gradually from 1.1 ppm in the winter to 0.6 ppm in the summer, when water consumption is high owing to the heat and humidity.

However, the question has led to public controversy in the United States, and this has been described in the report of a United Kingdom mission that went in 1952 to study the fluoridation of domestic water supplies in North America as a means of controlling dental caries. In the United States a fluoridation project has to be voted in, community by community, by public referendum, and pressure groups may be active. Matters became heated, for example, in Seattle, Washington State, which in 1952 voted nearly 2 to 1 against. The report comments that much of the public reaction seems to be bound up with questions of social philosophy rather than of medical opinion, and refers to deeply felt issues such as 'mass medication'. There is no lack of watchdogs on the look-out for creeping socialism.

The Delaney Committee, a select committee of the House of Representatives, conducted a public enquiry into the safety and efficacy of fluoridation, and concluded that 'a sufficient number of unanswered questions concerning the safety of this programme exists as to warrant a conservative attitude. . . . This method of wholesale indiscriminate administration of what is undoubtedly a highly toxic substance is improper, and constitutes a complete break with the medical tradition of individual care and responsibility'. Nevertheless, very soon after the committee's report was published, fluoridation of the water supply of Washington, D.C., which is administered by the Federal Government, was started.

The nature of the fluoride used is unimportant; sodium fluoride or fluosilicate are the commonest. The fluoride ion in the water is identical in any case with 'natural fluoride'. The cost is 5 to 15 c. per head per year, or 4d. to 1s. at the present rate of exchange. Lieutenant-Colonel E. F. W. Mackenzie, who until his recent retirement was Director of Water Examination to the Metropolitan Water Board, gave a comprehensive review of fluoridation in its various aspects in a Chadwick lecture in 1952. In view of the savings that might reasonably be expected in the cost of the dental service, and the relief of much suffering and inconvenience now caused by caries, he concludes that 'it would seem to be not so much a question of whether we can afford fluoridation but rather of whether we can afford to do without it'.

Outside the United States and Canada, fluoridation projects are now under way in some communities in South America, Australia, New Zealand, and Japan.

FLUORIDATION IN ENGLAND

The 1952 mission to North America considered many possible deterrents to fluoridation, such as affections of the bone, predisposition to cancer or kidney disease, toxic effects due to cumulative dosage, and any repercussions among industrial users of water, from brewers to launderers; but found nothing to suggest that any harm might ensue. In their report, published in 1953, they concluded that fluoridation to a level of 1 ppm was beneficial, safe, and easy, and recommended that its adoption in Great

Britain should be considered, but that there should be a number of preliminary projects in which a full medical and dental examination would be carried out on the selected community at all ages before and after fluoridation, and if possible that comparable communities be used as controls.

These recommendations have been adopted, and studies are planned or in progress in Anglesey, Darlington, Kilmarnock, and Watford. It seems likely that fluoridation on a national scale will eventually be adopted in Great Britain, if no hazards are discovered during investigations here. Out of 350 natural waters, only seven were found to have more than 1 ppm of fluoride. Assays of the present level of ingestion are being carried out, together with experiments on methods of control.

'HANDS OFF OUR DRINKING WATER'

A controversy as to the merits and ethics of fluoridation of the public water supply has been appearing in the last year or two in the British medical press. It is pointed out that as well as eating more sea foods, and drinking more tea, we live in a more polluted atmosphere than do the Americans; that fruits and vegetables may bear residual dusts from fluoride-containing insecticides; that the effect on old people, particularly on the kidney function, has been insufficiently studied (if the kidney is not functioning properly fluoride may be held up in the body instead of being excreted); that the consumption of tea may be highest among old people; that we should rather eat a low-carries diet, i.e. 'self-cleansing foods' (which do not leave refined carbohydrates in the teeth); that caries will only be reduced, not eliminated, by fluoridation; that if fluoride can be made available to children not drinking a public water supply, then it can be made available to only those people who want it; that there may be a connection between fluorosis and goitre; that white mottling ascribed to fluorosis has been observed in people whose water supply contains less than 1 ppm fluoride. All of these points will have to be taken into consideration.

Other objectors write about 'Medical Big-Brotherism, with its insistence on invading every home through the water tap', the

pattern of American objection repeated. There have been some wild statements: 'In its affinity for calcium its [fluorine's] action recalls that of lead and it is of interest that both substances are again associated in the production of high octane aviation fuel as hydrogen fluoride and lead tetra-ethyl. In addition to its violent chemical affinities, fluorine is highly reactive to electromagnetic radiation . . .'. And so to the use of fluorides as phosphors, for example, in television sets.

The vice-chairman of the British Housewives' League writes that 'if doctors and dentists will help to stop the adulteration, deprivation, and gassing of our food, then our bones and teeth would be as Nature intended. Mass medication is immoral . . . cannot be justified by quoting enforced additions to our food in the past. If we are to be given fluorine, we must also be given full information on easy household methods of removing it'.

The popular press has joined in. An article entitled 'Hands Off Our Drinking Water' appeared recently in a national illustrated magazine; the subtitle was 'Is This Mass Dosing an Assault on Freedom, a Gamble with Health?' The answer seems to be Yes. The editorial introduction says: 'Do you want to be a "guinea pig?" . . . I certainly do not. The aim is to "protect" children's teeth. But there is no proof that the measures to be taken will have this effect'. The author of the article happens to contradict this contention, although he is staunchly with his editor in protecting our civil rights. The experiment, within a year, 'will mark a new stage in the attack on the liberty of the subject, and the arrogance of scientific and medical experts. . .'. He agrees - with many qualifications - that 'fluoridated water will reduce or delay tooth decay in children . . .', but considers fluoridation to be 'a child of defeat', the defeat of the medical profession by dental caries. He reviews the evidence. Some of his statements are untrue: 'No U.S. surveys include effects on adults' . . . 'it is not known if fluoride is passed to breast milk' . . . 'if it [the chemical form of artificial fluoride when put into water] differs from natural fluorides it may not have the same effect on the body'. He comes to this conclusion: 'Fluoridation enthusiasts admit a "calculated risk". I call it an incalculable risk, and one that we should not take'.

Supporters of fluoridation take up the cudgels, usually by quoting the results of surveys such as have been described. They do not claim certainty on every point; the object of experiments such as those in Anglesey, Darlington, and the rest is to improve our calculations.

CONCLUSIONS

There is still doubt as to the precise requirements of many food factors, such as calcium, or the vitamins A to D, but we supplement our bread, margarine, or children's foods with these; this is presumably known as mass medication. Our medical experts do not usually err on the side of recklessness. During this controversy the editorials in the medical press and most of the workers in the field have spoken up in favour of fluoridation (to a level of 1 ppm), at least in the experimental areas. The conclusions of the Board of the Faculty of Dental Surgery of the Royal College of Surgeons of England, which was asked by the Ministry of Health to report on fluoridation, were as follows: 'The evidence to date shows a reduction of dental caries incidence of 40 to 50 per cent in children of the youngest age groups . . . [this for 1 ppm fluoride] . . . In a temperate climate, with a population of adequate nutritional standard, fluoride at a concentration of 1-1.5 ppm produces an occasional white fleck in the enamel, but this cannot be detected without expert examination, and is found in only 10 per cent of the children. The appearance of the teeth is excellent. There is no scientific evidence of toxicity or of harmful effects in districts with the water supply fluoridated at this level. . . . The Board considers that there is very strong evidence in favour of fluoridation, and recommends the proposal that this procedure should be adopted in the first place in some selected districts that can be used as study centres'.

Some people consider the official attitude unduly cautious, and that sufficient evidence is available to enable us to begin general fluoridation now. Lieutenant-Colonel Mackenzie asked 'are we to . . . withhold the probable benefit from another generation?'; the *Manchester Guardian* remembered that 'we were twenty needless years too late in introducing immunization against diphtheria'.

However, the fluoridation of public water is rather a new idea, and in the next few years we should have the answer to many problems. The estimation of fluoride in natural waters is not simple, and it will be necessary to arrange for efficient control and frequent sampling.

When the time comes, we may remember the story of the inhabitants of Charlotte, North Carolina. On the appointed day for the beginning of fluoridation, 1 April 1949, a flood of complaints began, of unpleasant tastes, ruined photographic films, stained clothing, and the death of goldfish. But the fluoridation had been secretly postponed, and when this was made public the complaints stopped. On 25 April fluoridation was started, unannounced, and there were no complaints.

ACKNOWLEDGEMENTS

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FURTHER READING

- Fluoridation as a Public Health Measure*. Symposium Volume of the American Association for the Advancement of Science. Washington, 1954.
- Report of the United Kingdom Mission (February-April 1952). *Fluoridation of domestic water supplies in North America as a means of controlling Dental Caries*. London, H.M.S.O., 1953.
- Mackenzie, E. F. W. 'The Prevention of Dental Caries by the Administration of Fluorine in Public Water Supplies'. *Lancet*, 1952, i, 960; published also as a pamphlet by the British Waterworks Association.
- Industrial Fluorosis*. M.R.C. Memorandum No. 22. London, H.M.S.O., 1949.
- Roholm, K. *Fluorine Intoxication*. H. K. Lewis, Ltd, London, 1937.

DIVERSION OF DRAINAGE BY ICE

G. H. DURY

EXAMPLES of the diversion of drainage through the agency of ice in Great Britain have been known for many years. Recent work, however, has revealed – and is still revealing – new and impressive instances to set beside the classic cases, and has also involved the application of principles which have long been neglected in this country. Current studies, moreover, are conceived on a large scale. They relate not so much to single diversions as to the disruption of whole river systems. In consequence they are of first importance to students of landscape. More generally, the investigation of glaciated highlands which is now in progress is highly successful in clarifying the processes of land sculpture by ice; anyone with an interest in the physique of landscape can readily understand, on the spot, how the ground acquired its existing form, when account is taken of the re-interpretations described below.

Notable early work includes that of Jamieson, who as long ago as 1863 correctly identified the Parallel Roads of Glen Roy and adjacent valleys as the littoral features of ice-dammed lakes. The water-level was controlled by cols successively unblocked during deglaciation. Marr, Derryhouse, Raistrick, and Miller have described many spillways in the Lake District, the Pennines, and the Welsh Border country; Lapworth, followed by Wills and Miller, traced the obliteration of part of the Dee and the diversion of the upper Severn through the Ironbridge gorge. Probably the best-known of all diversions is that of the Derwent, described by Kendall: it involved the impounding by ice of Lake Pickering and the reversal of the river. Striking though these changes were, however, alterations to the courses of the Thames and the Warwickshire Avon have proved even more spectacular, while glacial events in North Wales resulted in the complete dismemberment

of a drainage system which has been reconstructed only in very recent years.

It is clear that diversion of drainage through the agency of ice occurs in widely contrasted settings. When climate deteriorates, so that glaciers form in mountain valleys and ice-sheets spread widely over the land, ice occupies country that was formerly shaped by normal erosion – that is, by running water in its various forms of surface wash and of streams. Not only is the land surface modified by glacial sculpture, so that its general aspect after deglaciation contrasts strongly with its pre-glacial form: drainage is inevitably disorganized. At the least, rivers are temporarily replaced by tongues of ice. Elsewhere entirely new landscapes are produced by glacial erosion or glacial deposition, and drainage systems are initiated on them very different from those of pre-glacial times.

A common set of factors derives from the slowness of ice-movement. A valley glacier or an ice-lobe is far wider and thicker than the corresponding stream of meltwater, so that valley glaciers extending below the snowline obstruct the drainage of lateral tributary streams, while ice-sheets respond far less readily than do rivers to irregularities of relief. Moving ice is not restricted, as is running water, to an organized system of downward slopes.

It is reasonable to distinguish between temporary diversions that cease to operate when deglaciation occurs, and diversions that have longer-lasting effects. Diversions in highland and in lowland environments need separate consideration. Again, it is often convenient to contrast diversions due mainly to ice erosion with those due primarily to obstruction of water by ice or by glacial deposits. These three sets of alternatives lead between them to eight possible combinations, as shown diagrammatically in Figure 1 (*overleaf*).

The descriptions of British landscape which follow relate mainly to studies published since the war, and are arranged under the headings of glaciated highlands and glaciated lowlands respectively. It is to be expected that the principal diversions, in highland areas, which have persisted into post-glacial times should be associated with the displacement and breaching of watersheds

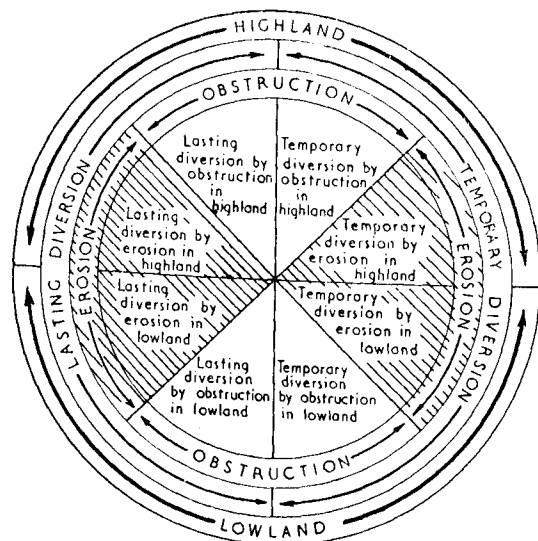


Fig. 1. Types of drainage diversion by glacial action.

by glacial erosion, although the details are often complicated by deposits of moraine and by alluvial fans; for it follows from the strength of the relief and the usual depth of glacial troughs that most post-glacial trunk streams are confined within deep valleys with steep sides. Temporary diversions in highlands are recorded in the multitude of abandoned spillways; it is exceptional for a spillway to retain its stream after the ice melts. In lowland areas, ice and ice deposits have both rearranged the network of drainage. Near to the centre of former ice-caps glacial scour has extensively modified the land surface, and the typical product is a terrain of low rocky hummocks with the intervening shallow depressions filled by lakes, bogs, and interlacing streams. At and near the former positions of the ice-fronts, as in Lowland Britain, it is fair to say that obstruction and deposition by ice have been more effective than has erosion in changing the course of rivers.

DISPLACEMENT OF DIVIDES IN GLACIATED HIGHLANDS

During maximum glaciation valley glaciers become so large that in addition to obstructing stream drainage they may themselves rise above the sill level of lateral cols, so that ice spills across the divide from one valley into the next. The process is called glacial diffuence, on the analogy of the diffuence of channels in a river delta. The most favourable set of conditions for diffuence are those which include roughly parallel trunk valleys with deep cols in the divide between them, or alternatively lateral cols developed at the heads of powerful back-cutting streams on the far side of the watershed, together with a large glacier in one valley well nourished by snows on high ground. The level of the ice is likely to rise most when the outlet of the glacial valley is restricted, either by the narrowing of the valley itself or by the entry of a large glacier at a confluence of valleys.

Diffuence was named by Albrecht Penck in 1909, and its effects on landscape expounded by W. M. Davis in 1912. D. L. Linton has been highly successful in demonstrating its action in the Scottish Highlands and other glaciated parts of Britain, and in re-interpreting the evolution of drainage in such a way as to allow for its effects.

A diffuent tongue of ice erodes the cols through which it moves. The details of the glacially modified surface vary with the nature of the rock and with the duration of diffuence, but the result to be expected is a displacement of the divide within the pass towards the main glacier. When deglaciation occurs, therefore, stream drainage entering the pass is likely to find an outlet in the direction of diffuence, and the stream pattern will appear anomalous on the map.

Two easily accessible passes which carried diffuent ice are located in the Snowdon district (Figure 2). The first of these, the col at the head of Llyn Cowlyd, allowed ice to overflow north-eastwards over the side of the Llugwy valley (Figure 3). Hereabouts the main valley is cut in relatively weak rocks, and had already been considerably widened before glaciation occurred. It therefore provided a basin with the natural outflow to the east

impeded by constricting high ground near Capel Curig, and in it there accumulated the ice discharged by the corries on the eastern Glyders and the Carnedd's. The Cowlyd col leads into the valley of the Afon Ddu, which joins the deep, broad Conway valley only eight miles from the open sea. During glaciation the col functioned as an overflow route for ice, with the result that it now appears as a glacial trough of typical form. The divide within it has been displaced at least half a mile to the south-west.

The second example lies west of Snowdon, connecting the valley of the Gwyrfaï at the head of Llyn Cwellyn with that of the headwater of the Llyfni (Figure 4). Ice moved north-westwards through the Cwellyn basin; but there is a constriction where stronger rocks outcrop above Bettws-Garmon, and the supply was augmented from the great corries on the western flank of Snowdon. Consequently diffluent ice passed over the Drws-y-Coed saddle some two miles east of Nantlle, converting it into a deep trough. The present watershed stands a mile east of the old line, and is so ill-defined that a low dam makes the two small lakes drain to the Llyfni instead of to the Gwyrfaï. The forms of the ground in this pass are striking and unmistakable (Inset 12). In and around the lakes are knobs of rock, scoured on the east and plucked on the west; by far the most prominent is Clogwyn-y-garreg, a hill in the centre of the pass more than a hundred feet high, which escaped destruction by the moving ice only because its rocks are greatly strengthened by multiple injections of quartz veins.

Whereas glacial diffluence commonly results in the displacement of a watershed by a matter of a mile or less, the effects of transfluence are far more imposing. The term transfluence is again due to Penck, and the application of the concept to British landscapes mainly to Linton. Glacial transfluence involves the passage of ice in quantity across the lines of pre-glacial watersheds and the severe erosion of the passes. It occurs where exceptionally high ground nourishes glaciers so large that the valley system cannot contain them, or where an ice-cap is hemmed in by a rim of mountain and finds outlets through the intervening valleys. The necessary conditions represent an exaggeration of those which

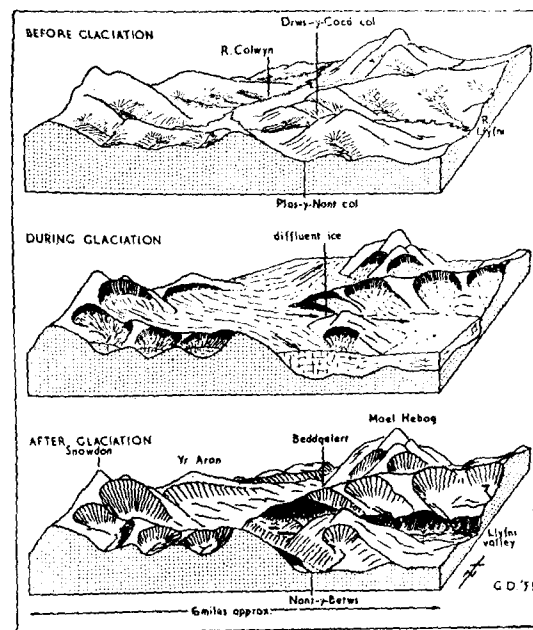


Fig. 4. Diffluence through the Drws-y-Coed col. Note that the Plas-y-nant col was also breached, but by transfluent ice.

bring about diffluence; the erosive powers of the transfluent ice are correspondingly increased, and its mark on the post-glacial scene proportionately greater.

At the present time transfluence is occurring at the edge of the Greenland ice-cap, for peaks project as nunataks in the marginal areas, while the outlets are subjected to scour by the outspilling tongues of ice. The Scandinavian ice-cap of the Pleistocene was similarly confined on its western side by the high divide of the peninsula, and as was recognized many years ago glaciers flowed westwards through the gaps, removing projections in the sides and grinding down the floors. In the formerly glaciated highland areas of Britain, transfluence is well documented in the Scottish

Highlands and in North Wales. The excess of ice necessary to choke the valley systems was provided by snowfall on the heights of the Snowdon and Ben Nevis districts and on the high Grampians, while the ice-cap centred west of Moray Firth was related to the North-western Highlands in the same way that the Baltic ice was related to the main Scandinavian divide.

Glacial transfluence can produce valleys through the pre-glacial watersheds so deep that post-glacial drainage follows them. In consequence the present stream net is to some extent dissociated from the pattern of distribution of highland, and includes bold angular bends which for many years have been interpreted with some difficulty as the result of river capture. When account is taken of former glacial transfluence, however, the existing drainage becomes much easier to understand. Former watersheds can often be reconstructed along the ridges and across deep gaps through them, and the displacement of divides is often seen to amount to several miles. Sizable areas have been transferred from one river basin to another. The indeterminate water-partings in the floors of glacial troughs, consisting perhaps of alluvial fans, belts of moraine, or merely of peat bogs, appear as the natural consequences of highland glaciation.

The Spey has often been cited as a piratical stream, which by successive captures has greatly enlarged its catchment and diminished the adjacent basins of its weaker competitors. The supposed captures depend on the interpretation of sharp bends in the traces of streams, where the upper reaches of rivers seem to have been turned from their earlier courses. The idea of capture, however, ignores two considerations – the nature of the underlying structure and the field evidence of glacial transfluence.

The structural pattern of the Spey basin, as of so much of the Scottish Highlands as a whole, consists of intersecting systems of rectilinear lines of weakness. Streams adjusted to such a pattern are bound to turn through sharp bends as they pass from one alignment to another, but the angles do not correspond to the elbows of capture in scarpland country: the concept of landscape evolution in a scarpland cannot apply in the Highlands. More important, as the field evidence is examined afresh and the sup-

posed captures are reassessed, many of them are shown indubitably to be instances of drainage diversion through glacial action.

For instance, the upper Geldie, at one time tributary to the Dee, has been diverted across the line of the old watershed (Figure 5). During glaciation ice from the Cairngorms and the Grampians accumulated in this head-valley, overtopped the col above the source where the sill level was about 2,200 feet, and overflowed into the valley of the Feshie. The col was lowered, the highest point displaced towards the source of origin of ice, and a deep glacial trough cut where a much shallower pass had been. The upper Feshie now runs through the pass; it narrowly approaches the head of the shortened Geldie, but is separated from it by part of an alluvial fan. At this point the actual watershed consists, therefore, of a post-glacial accumulation of stream-laid debris. An especially satisfying feature of this diversion is that the form of the old valley can be reconstructed, for portions of it remain visible on the lower flanks of the hills.

A second diversion by means of transfluence, essentially similar in principle but more elaborate in detail, concerns the Banffshire Avon. The uppermost fifteen miles of the river are aligned with

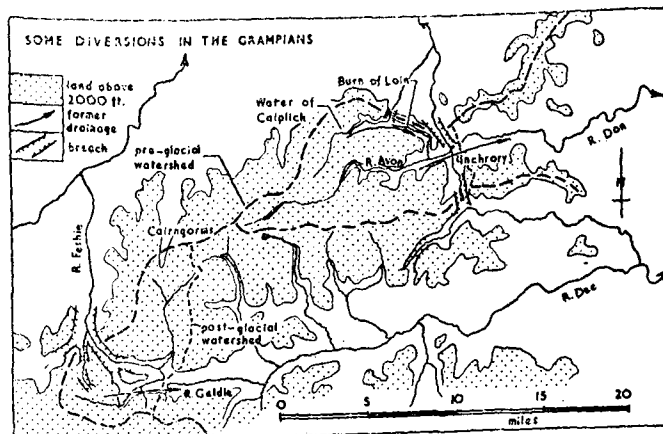


Fig. 5. Some diversions in the Grampians. (Re-drawn after Linton.)

the headwaters of the Don (Figure 5), whose valley continues that of the Avon. Instead of joining the Don, however, the Avon turns abruptly northward at Inchrory and flows through a narrow breach in a belt of high ground, eventually joining the Spey. Here also the breach is the work of transfluent ice. The remaining saddles along the line of the old watershed stand at about 2,000 feet; that which existed on the site of the Inchrory breach seems to have been rather lower, probably because of erosion by the head of the pre-glacial Avon on the far side of the watershed. Hence when deep ice gathered in the upper Don valley it was able to cross the divide, and while the outflow continued erosion was so powerful that a trough was cut 800 feet deep. It is clear that the discharge here was unusually powerful; the reason lay in an extra supply of ice which came from the Dee valley, breaching two more watersheds on its way.

West of the Inchrory breach lies the Caiplich gap. It contrasts markedly with the glacial trough, for it is a V-shaped gash in the watershed 250 feet in depth. It is identified as the former spillway of an ice-dammed lake, which has – exceptionally – retained its stream after deglaciation. The valley of the Water of Caiplich is continued by Glen Loin, flat-floored, peaty, and obviously glaciated. At one time the Caiplich flowed along it, forming the headstream of a longer Loin. But at a stage when the ice in the Caiplich valley had melted back, the lower Loin valley was still obstructed by the main glacier. Impounded water spilled over a col towards the north, eroding the outlet so rapidly that when the Loin was unblocked the Caiplich was already flowing at a lower level and persisted in its diverted course.

Both ice and overspilling water have contributed to the cutting of the defile of the Callop near Glenfinnan (Figure 6). This piece of country may serve to exemplify the many localities where westward-moving ice passed over the major divide of the North-western Highlands; it is likely to be the most familiar of these, for the defile carries the main road and railway from Fort William to the west coast. The Callop rises on the eastern side of a line of hills and enters the head of the Eil valley, but instead of joining the Dubh Lighe it turns sharply towards the west and pierces the

ridge in a narrow gap, flowing into Loch Shiel on the far side. In this area, as in other glaciated highland country, the former direction of ice movement is indicated by the disposition of plucked-and-scoured rocks: the many rock-knobs are all scoured on the east and plucked on the west, showing that ice moved *up* the Eil valley. Under the influence of great increments of ice from the heights of the Ben Nevis group, the Eil glacier flowed westwards, receiving additions from the lateral valleys on the north and the corries on the south, and overrode the col in the valley head. The sill level was reduced to some 250 feet above present sea level. When melting set in, the lateral valleys were freed of ice more rapidly than was the main valley, as would be expected from the comparative heights of the source areas, and lakes were formed in them. Temporary spillways are indicated by notches which lead into the Finnan valley at various levels, but the principal interest concerns the defile itself. This is so narrow that it too is likely to have originated as a spillway. Readvance of the main glacier carried ice through the defile, for the rocks within it are striated, plucked, and scoured. Final retreat is recorded in the bands of stadal moraine which encumber the floor of the Eil valley west of the loch, and one belt of mounds serves to turn the Callop westwards through the gap instead of eastwards directly to tidewater. The Dubh Lighe enters down the slopes of an alluvial fan, on which it has at times swung westwards to unite with the Callop, but at present it happens to be slightly incised into its own alluvium and therefore to be fixed, for the time being, in the system of eastward drainage.

The sites chosen for description represent only a few of the very many occurrences of glacial transfluence which might be quoted from the Scottish Highlands. Before attention is turned to another area, however, it is well to mention a remarkable group of breaches where the passes have been so severely scoured that ribbon lakes now lie athwart the former watersheds. The area concerned lies east of Ben Nevis, where the level of ice in the enclosed basin of Rannoch Moor was raised by the discharge of corries in the Ben Nevis Group, Mamore Forest, and the highlands south of Glen Coe. The divide which ran east from Ben Nevis was pierced in

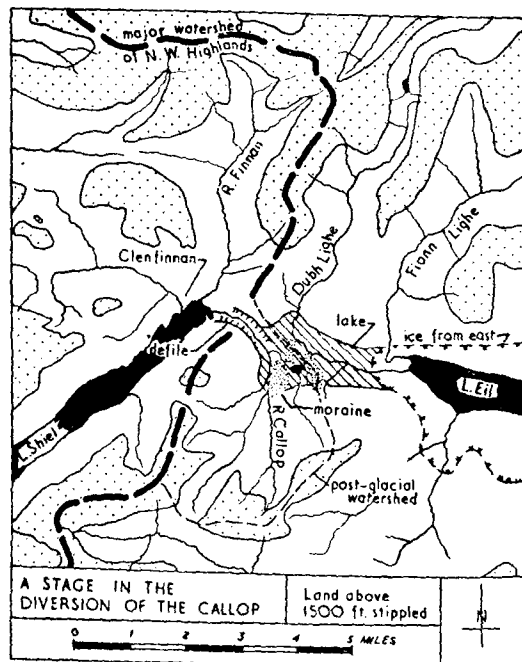


Fig. 6. A stage in the diversion of the Callop. (Re-drawn after Dury.)

several places; Loch Treig, Loch Ossian, Loch Ericht, and the Pass of Drumochter mark the lines of some of the pre-glacial saddles, which in certain cases have been converted to deep basins. It is noteworthy that, in this area also, the basin of the Spey has been extended by the ice which crossed the divides into it.

In the Snowdon district, too, wholesale changes in the pattern of watersheds have taken place. At the present day the area is dominated by valleys leading generally north-westwards towards the sea. Linton has reconstructed an older drainage system, showing that a group of streams once flowed eastwards off the hills and united to feed the ancestral Dee, which discharged through the Llangollen gap and at one time flowed still further to the east as

the main headstream of an early Trent: the obliteration of the immediately pre-glacial lower Dee has already been mentioned. Within the Snowdon district glacial diffluence has replaced the earlier sheaf of head-water valleys, with its integrated system of streams, by a miscellaneous collection of glacial troughs, an angular stream plan, ungraded rivers, and anomalous and indeterminate watersheds.

At maximum glaciation the uplands to the east were largely buried by ice, and the few trunk valleys could not provide outlets for it all. The depth of ice was increased locally by heavy snowfall on the Snowdon group, and also at times by the presence of an ice-sheet in the Irish Sea basin, where the thickness was so great that Anglesey was completely overridden and marine shells were carried above the present 1,000 feet contour on the flanks of the upland. It is not surprising that glacial diffluence and transfluence occurred, or that spillways functioned from time to time (Inset 13).

The relations between Nant Francon and the Ogwen valley are particularly clear and informative; the locality moreover is familiar to many (Figure 2). A view along Nant Francon towards the head (Figure 7) shows that the valley ends in a rock-step about 200 feet high; water from Llyn Ogwen descends the step in a waterfall, and the road from Bethesda climbs obliquely up the valley side to reach the top, for the tread of the step is the floor of the pass into the Ogwen valley. Downstream from the rock-step the sides of Nant Francon have been shorn away by ice; glacial plucking on the face of the step and the orientation of plucked-and-scoured rock-knobs in the Francon valley indicate a northward movement of ice within the trough. It is evident that ice came through the pass in quantity. However, the view from the north shows that the valley heading in the Devil's Kitchen slopes not towards Nant Francon but towards the Ogwen valley, and the smaller corrie next on the north is aligned in the same direction. Within the Ogwen valley drainage west of the Llugwy passes to Nant Francon, and is separated from eastward-flowing waters by an ill-marked watershed located among morainic hummocks on the floor of the trough. The Llugwy drains towards Capel Curig with a gradient gentle at first, but farther downstream it becomes

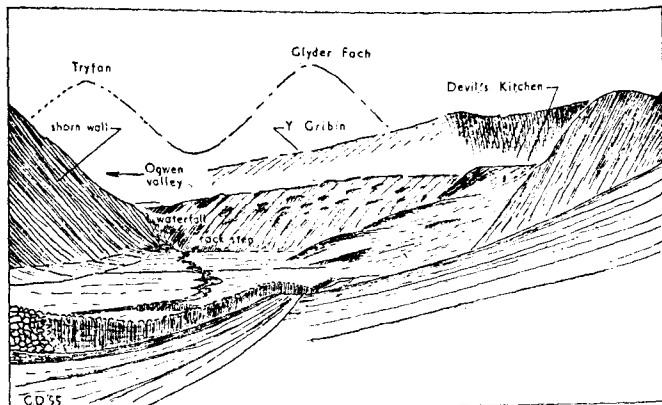


Fig. 7. The end of the Francon trough, looking south.

incised into glacial debris where it is affected by the steep fall to the floor of Nantgwryd.

The gap at the head of Nant Francon is yet another glacial breach in a pre-glacial watershed. The divide formerly lay at the position shown in Figure 8. Ice freely supplied from the great series of corries on the eastern side of the Glyders filled the wide middle section of the Ogwen valley. There was some discharge eastwards, as can be seen by the arrangement of plucked-and-scoured rocks near Capel Curig, and also northward through the Cowlyd col, but the saddle at the head of Nant Francon was overridden and was so greatly lowered that it provided a main outlet (Inset 14). The ice-fall established there retreated southwards as the underlying rock was cut back, with the result that the rock-step no longer stands on the line of the former divide. The watershed itself was still further displaced, and now lies well beyond the head of Llyn Ogwen (Figures 3 and 8). Pluck-and-scour is abundantly displayed in this section of the Ogwen trough, and indicates movement of ice towards, and through, the pass into Nant Francon. The disruption of the Trent's ancient headstreams is complete; the highest sources are now represented by the corrie at the head of the Llugwy, which drains only to the Conway, and

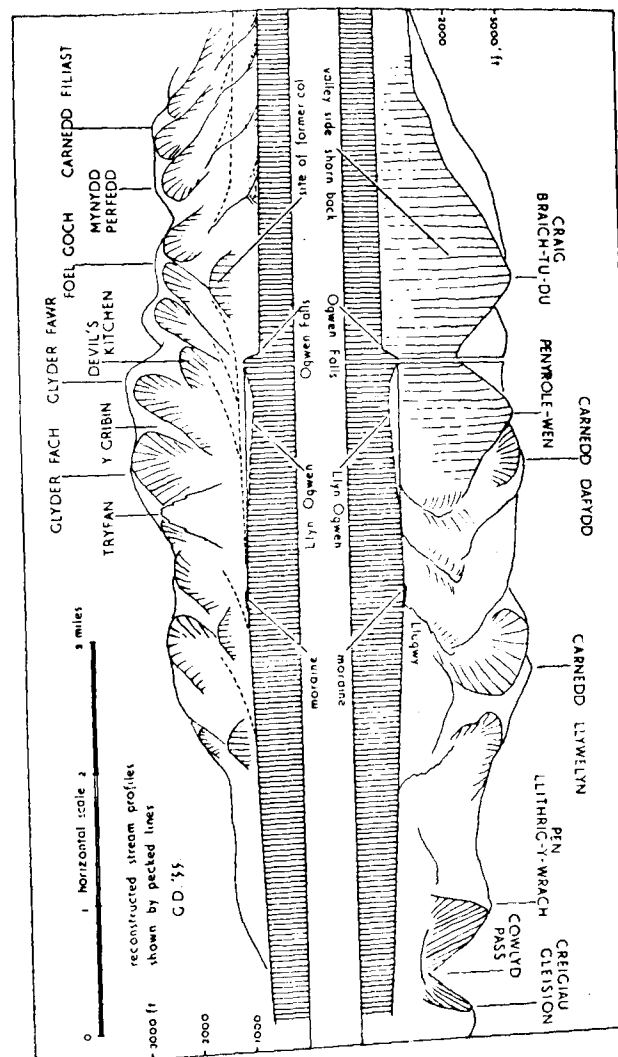


Fig. 8. Lateral profiles of the Ogwen Valley and part of Nant Francon: above, south-west side, below north-east.

by the Devil's Kitchen which finds the most direct outlet to the sea near Bangor.

DRAINAGE DIVERSION IN GLACIATED LOWLANDS

In lowland areas where ice-caps formerly stood, every gradation can be traced from minor and temporary derangements of the stream system to the complete obliteration of the pre-glacial landscape. It is well known that in East Anglia the superficial deposits laid down behind the ice-front are very thick, so that the land surface which is now visible lies well above the concealed surface of the solid rocks beneath. In these circumstances the net of post-glacial streams may have little relation, or none at all, to that of former times. So much is obvious on general grounds. Research has also revealed, however, that noteworthy changes occurred in areas of Britain close to the extreme limits of glaciation, and it is proposed to discuss the diversions of the Kennet-Thames and of the Warwickshire Avon as exemplifying a wide range of possibilities.

The Kennet-Thames is thought to have been one of the greatest of those master streams which, draining from west to east across Britain, can still be identified from their surviving remnants. The Kennet rises in a district that was subject to periglacial climate* during glacial maxima, and that undoubtedly received considerable snowfall, but was not entered by the ice itself. It is true that the headwater country of the upper Thames basin was partly covered by an ice-sheet, but little is known of any river diversions that may have occurred at that early stage, when in any event the valleys were not yet deeply cut. A later advance brought ice-lobes into the heads of the Cherwell and Evenlode valleys, but drainage appears to have suffered little disturbance. By contrast, the valley of the Thame was buried by ice and blanketed by glacial drift; but it will not be considered here, as it has not yet been studied in detail in this respect. The effects produced there are likely to have been similar, in a general way, to those which occurred in the lower valley of the Thames itself.

Between Henley and Marlow the Thames is incised into the

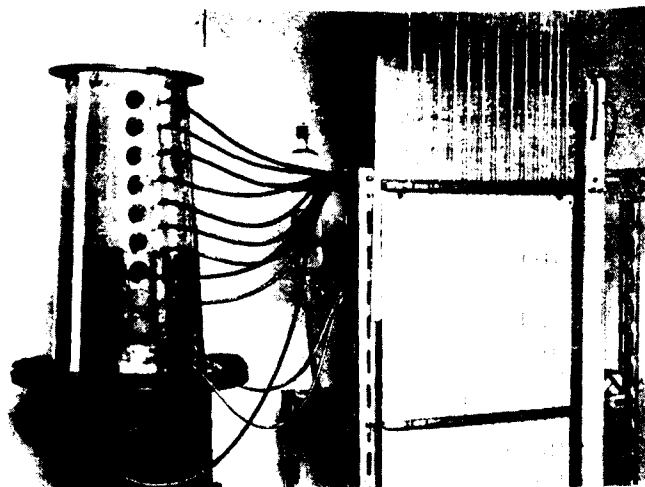
*See article in *Science News* 33 (1954), pp. 69-70 especially.

IRON AND STEEL RESEARCH

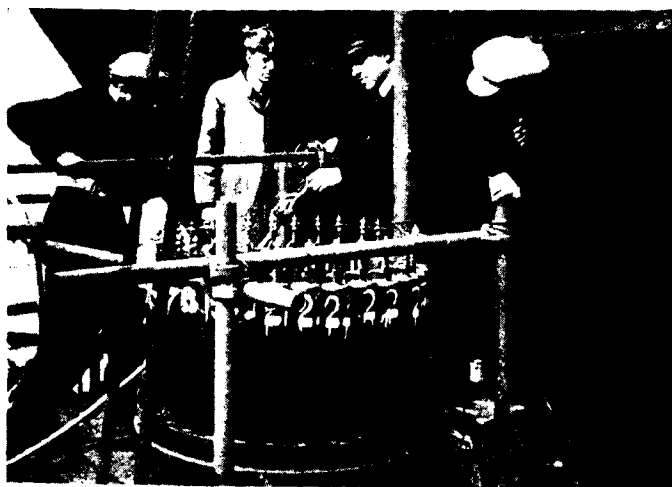
Research in the iron and steel industry has been much expanded since the war, largely through the work of the British Iron and Steel Research Association. Examples of current and recent work are illustrated in the following photographs.



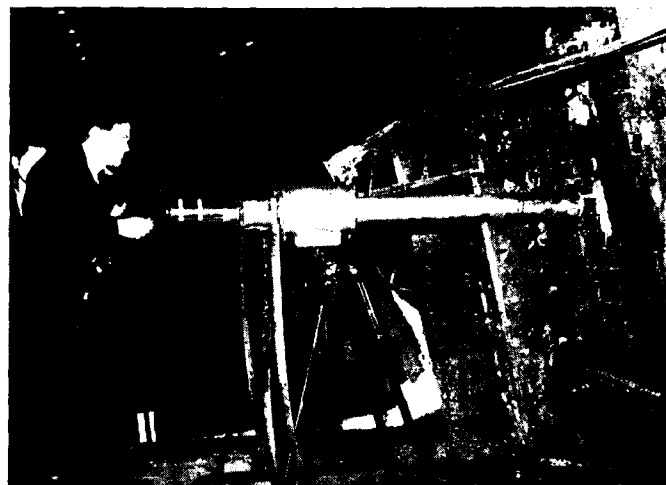
1. Continuous casting of steel on pilot plant at the association's Sheffield laboratories. Results on an experimental works plant have so far been encouraging.



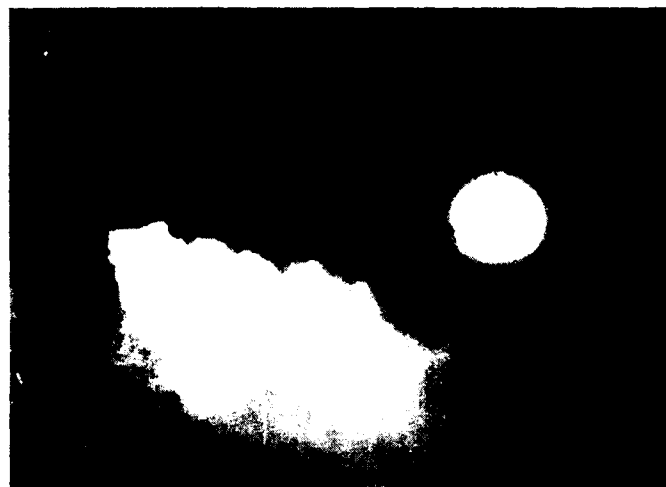
2. Research on blast furnaces: the study of aerodynamics in a laboratory model of a blast furnace.



3. Research on blast furnaces: experiment on gas velocity in a blast furnace at Corby. Radon has been projected into the furnace, and samples collected at one-second intervals. (By courtesy of *Stewarts and Lloyds, Ltd.*)



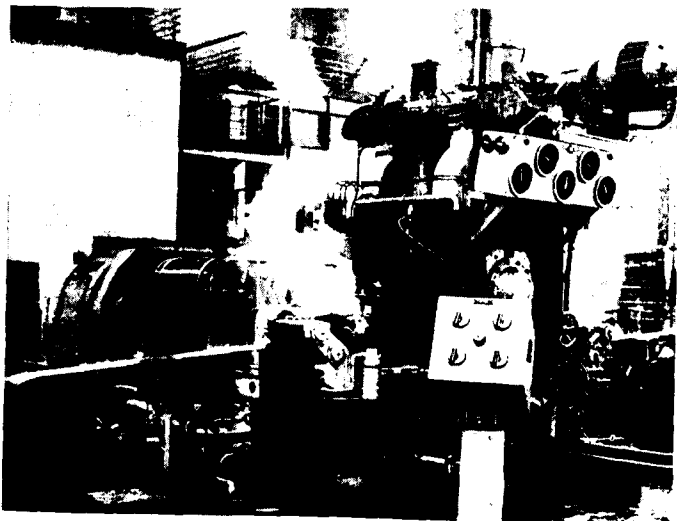
4. Research on open-hearth furnaces: periscope for photographic observation of flame conditions in furnace.



5. Research on open-hearth furnaces: photograph showing flame distribution in furnace. The white circle on the right is caused by the optical system. (Photographs on this and next page by courtesy of the *British Iron and Steel Research Association.*)



6. Mechanical working: experiment on the rapid heating of forging ingots. The heated ingot is being positioned in a 200-ton press.



7. Continuous gauging: a 14-inch rolling mill, equipped with automatic gauge-control system.

PHYSIOLOGY OF SURVIVAL AT SEA

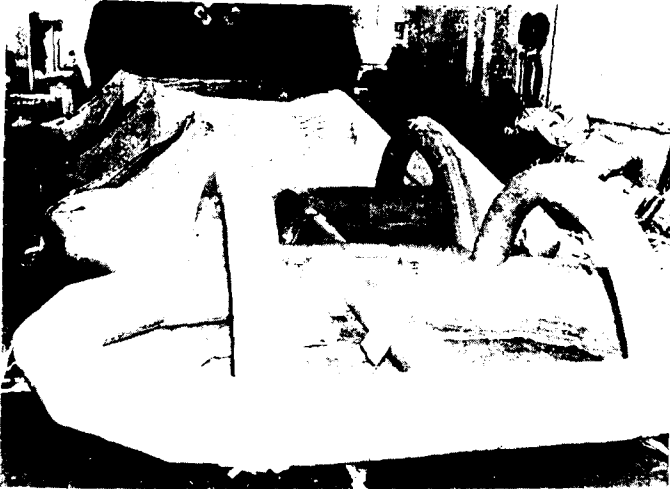


8. A 20-man Mark III naval life-raft in its valise; painter at end. Installed on a ship, the end of the painter is attached to a strong point, and the cord lacing replaced by a lighter line.



9. The same type of raft inflated on the water. The sleeve entrance is partly closed, and the boarding ladders can be seen.

DIVERSION OF DRAINAGE BY ICE



10. Rafts under construction, showing the arrangement of the buoyancy tubes and arches. These are an earlier pattern (Mark I), the type used in the arctic and tropical trials.



11. Higgins solar stills in the water. They are made of translucent plastic and pack into a small space.



12. A spillway near Port Dinorwic, Caernarvonshire.



13. The Ogwen valley, looking towards the head of Nant Francon.



14. View through the northern arm of the Drws-y-Coed col; compare Figure 4 of article.

chalk of the Chiltern plateau, and closely follows its earliest known line – although, of course, at a lower level than that of the old valley floor. Between Marlow and St Albans occurred a complex sequence of glacial diversions. Further still to the east, the old trace is concealed by the boulder clay, sands, and gravels of Essex. The setting is therefore comparable to that of the north German rivers, which were also obstructed by ice-sheets advancing up the valleys, and were subjected to diversion at and near the ice-front.

A great deal is now known – principally from the work of S. W. Wooldridge – about the evolution of the Thames drainage between the Marlow bend and the line of the Lea, and the account which follows is necessarily of a summary nature. An early course for the river has been identified, continuing the run of the Henley-Marlow reach far to the north-eastward (Figure 9). From Marlow to the neighbourhood of Watford the evidence consists of water-laid gravels; beyond this area the old valley bottom disappears beneath glacial deposits, and the form of the sub-drift surface is employed as a means of tracing it. Before the ice arrived the course of the Thames was that roughly indicated in Figure 9 (upper diagram) by the more northerly bold arrow, i.e. along a line through the sites of Marlow, Watford, the Vale of St Albans, Ware, Harlow, and Chelmsford. Ice advancing from the east blocked the valley as far west as Watford, and the river was diverted to the south. It found a new course round the low plateau of South Hertfordshire, into the valley of a tributary which led north-eastwards from the site of Finchley. But this new course (shown in both parts of Figure 9) was no more than temporary.

Decay of the ice was followed by the re-establishment of rivers, and the Vale of St Albans was deepened by tributaries of the Lea. But ice again entered, reaching at the maximum something very close to the extreme limits of its earlier advance, and the stream system was again disrupted. One lobe moving up the Finchley valley completed the displacement of the Thames to the southward. A second lobe in the Vale of St Albans turned some of the feeders of the Lea towards the Colne. For instance, the old valley of the Mimms Hall Brook is still filled by the more southerly of the

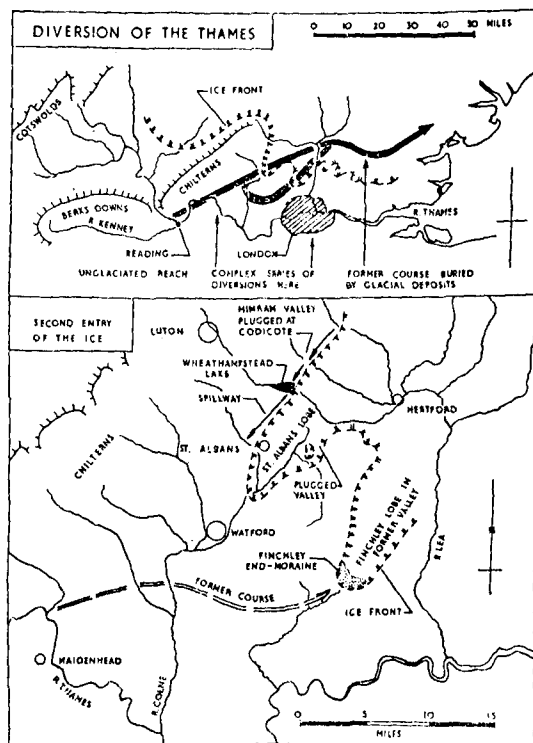


Fig. 9. Diversion of the Thames. (Based on the work of Wooldridge and others.)

two drift plugs indicated in Figure 9 (lower part). On the north-western side of the great tongue of ice, all the streams draining from the Chilterns were blocked. Among the various lasting diversions may be noted the plugging by drift of part of the Mimram valley at Codicote. Temporary disturbances, which endured only so long as the ice remained, included the formation of spillways near the ice-margin leading south-westwards from one valley to the next. Associated with this system of outlets was an ice-dammed

lake in the valley of the Lea near the site of Wheathampstead. The final outlet, the spillway which passes into the valley of the Ver at St Albans, is Beech Bottom, noted for the defensive ramparts of Iron Age date contained within it.

The glacial deposits within the Vale of St Albans are much worked for concrete aggregate. Although for this reason the exposures are mainly of sand and gravel, boulder clay is also to be seen, together with the characteristic varved clay of pro-glacial lakes. Varved (banded) clay consists of a succession of double laminae, interpreted as resulting from one year's sedimentation for each band. The typically irregular surface of frontal deposits makes it probable that many temporary lakes will form as the ice decays. Lake sediments are not in themselves any cause for surprise; where they occur in the valley of the Ver, south of St Albans, it seems likely that the lake in which they were deposited was dammed up between the front of the decaying ice and the terminal moraine left at the limit of advance. If this is so, the moraine has been destroyed by later erosion, or at least has not yet been located. Conditions in the Finchley valley are different. After deglaciation the terminal moraine survived as the water-parting between two streams, and is represented by the crescent of boulder clay and outwash upon which Finchley stands.

As in Essex, so in the Midlands the form of the sub-drift floor has been discovered and the old lines of drainage reconstructed. In the eastern part of the area, borings for ironstone provided some 2,500 records. They show that although some pre-glacial valleys have been re-excavated, and a number of them substantially cleared of glacial deposits, others remain plugged and do not carry post-glacial streams. When allowance is made for plugging - with the drift as much as 100 feet thick in places - it is found that the present stream net derives from a system of eastward-flowing streams, modified to some extent by capture before the onset of glaciation, and partially disorganized by glacial deposition itself. In some parts of the area the existing array of drainage lines gives very little indication of the arrangements of valleys and divides that had resulted from stream competition in non-glacial conditions.

The series of changes in the basin of the Warwickshire Avon was traced in detail by F. W. Shotton in 1953. It involves a major reversal of drainage and a great displacement of the watershed between streams flowing to the Bristol Channel and those flowing to the North Sea.

Before the third of the four complex maxima of Pleistocene glaciation the Warwickshire Avon did not exist. The area now included in its basin drained north-eastwards, apparently by way of a River Soar far longer than the present stream of that name. That is, the piece of country in question was included in the basin of the Trent (Figure 10). Ice approaching from the north-east, north, and west dammed in a lake between the ice-front and the rising ground to the south-east, where sandy limestones of Jurassic age form a dissected but perceptible scarp. The glacial lake has been named Lake Harrison, after the geologist who noted lake deposits within the area in 1898. At its greatest extent it had a surface area of about 750 square miles.

While the lake existed, the lower part of the old Soar valley was buried by ice and the upper part was drowned beneath the lake waters. Drainage escaped south-eastward through cols in the scarp, exemplifying temporary diversion of the kind associated with the Glen Roy lakes. In this Midland area, too, there are ancient shorelines. The chief of these, lying between 400 and 410 feet O.D., consists of a bench as much as a quarter of a mile wide and persists for a distance of thirty-five miles. It can be traced into the gap at Fenny Compton, which is taken therefore as having provided a spillway for the lake at the 400-foot level. There is possibly another spillway at the same level leading through the Watford Gap into the basin of the Nene, but frontal ice-deposits here are very thick and extensive, and if a channel does exist it is concealed beneath the drift. Such conditions are quite probable, for, after the formation of the lake, ice advanced in oscillatory fashion over the whole area.

Along the flanks of the lake the water seems to have been shallow, but in the north-east, where the old Soar valley was deepest, the floor of the basin lay 200 feet below the water surface. There was, however, a sufficient supply of debris – lake deposits,

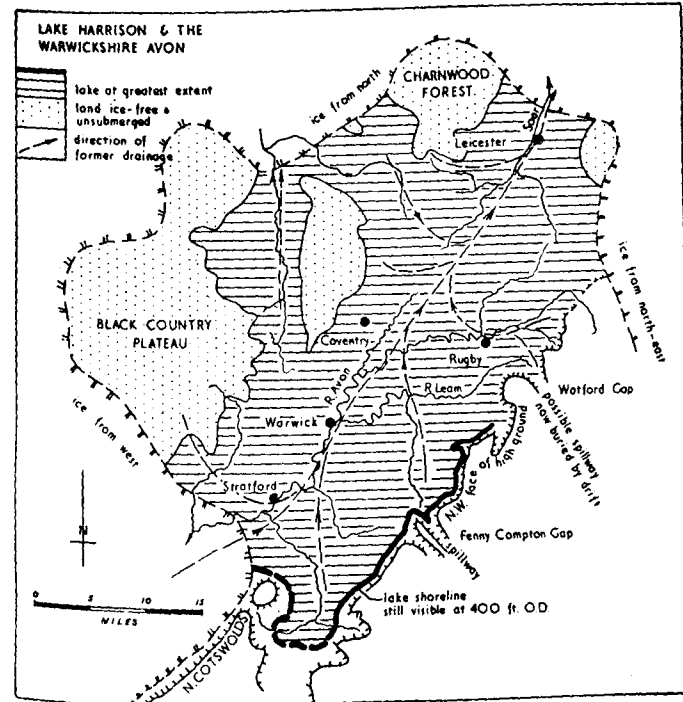


Fig. 10. Lake Harrison and the Warwickshire Avon. (Re-drawn after Shotton and Dury.)

boulder clay, and sandy and gravelly outwash – to fill the basin completely, so that in the end the lake consisted only of a thin skin of water overlying a level bed of drift. Part of the fill consists of varved clay which is now the subject of analysis.

When the ice-sheet decayed, the drift-filled basin – no longer a lake – came to be drained once more by rivers. The several spillways, including the wide gap at Fenny Compton, were abandoned. But since the south-western end was first freed from ice, the stream which formed on the exposed lake bed drained in that direction. It rapidly extended itself headwards, becoming the

Warwickshire Avon as we know it today. Thus the drainage of the Avon basin has been twice reorientated through glaciation, and has ended by taking a line diametrically opposite to that of pre-glacial times. From an early north-easterly direction, the discharge was first temporarily diverted towards the south-east, and then permanently towards the south-west. The displacement of watersheds is more spectacular in this area than anywhere else in Britain.

Although much remains to be done in the way of identifying individual diversions of drainage, especially in the Highlands of Britain, it is fair to claim that the principles involved have been applied with great success to landscape study in this country. It is also evident that the absorbing theme of the evolutionary development of rivers cannot be properly examined, in respect of formerly glaciated areas, without due regard to the possible influence of ice. Fortunately the genetic study of the physical landscape has now long passed the stage of map-reading, which has been responsible for some sad errors in the past, and relies on the investigation of the ground itself.

FURTHER READINGS

The suggestions following are limited to recent papers; references to earlier work are given in their bibliographies.

For diversions in the Scottish Highlands:

Linton, D. L.: papers in *Scottish Geographical Magazine*, 65 (1949), 67 (1951), 70 (1954), and in *Transactions, Institute of British Geographers*, 15 (1949).

Dury, G. H.: *Scottish Geographical Magazine*, 69 (1953) (diversion of the Callop).

Breaches in North Wales are referred to in:

Linton, D. L.: *Advancement of Science*, 7 (1951).

The history of the Thames is traced in:

Wooldridge, S. W., and Linton, D. L.: *Structure, Surface and Drainage in South-east England*. London, Philip, 1955 (includes references to individual papers). See also

Wooldridge, S. W., and Henderson, H. C. K. H., in *Transactions, Institute of British Geographers* (forthcoming number: in the press).

The evolution of the Warwickshire Avon and the littoral bench of Lake Harrison are described in:

Shotton, F. W.: *Philosophical Transactions, Royal Society*, B 237 (1953) and

Dury, G. H.: *Proceedings, Geologists' Association* 62 (1951).

THE PHYSIOLOGY OF SURVIVAL AT SEA

G. R. HERVEY

LARGE-SCALE disasters at sea are rare in peacetime, and the annual loss of life from accidents to British ships has declined steadily over the last hundred years. It is unlikely, however, that accidents will ever be entirely eliminated, and in smaller ships which range far afield, such as trawlers, they are still not so uncommon. In wartime sinkings at sea have caused much loss of life, and the 'ditching' of aircraft has added to the numbers of castaways.

Attempts to improve equipment for saving life at sea are not new. An impressive list can be made of the ideas which have been put forward: one of the most 'modern', that of using inflatable equipment, can be traced back to the ancient Babylonians! Nevertheless the design of life-saving equipment has advanced comparatively slowly, and the casualties which have occurred, even in recent years, suggest that there is still room for progress. In 1945 an Admiralty committee was set up to examine the whole question of naval life-saving. Its report has led to a new attack on the problem, and in particular to a study of the physiological aspects. A clearer understanding of these has in turn led to radical changes in the design of equipment. Some practical problems remain to be solved, but the work done already holds out good hopes that the new methods will much improve the outlook for future castaways.

THE PHYSIOLOGICAL PROBLEM

It seems clear from the accounts of survivors that, if H.M. ships sunk in action are excluded, even in wartime only a minor proportion of casualties at sea were caused directly by explosion, fire, trap-

ping in the sinking ship, injuries sustained in leaving her, and so forth. Most of those who lost their lives probably died in the water. It is natural simply to ascribe all such deaths to drowning, the obvious hazard to a man in the water, but this is only a part of the story. In many cases the primary cause of death was not drowning, but 'exposure'. Even when drowning was the immediate cause of death, it occurred because the victim was by then helpless from the effects of cold or exhaustion. Survivors' accounts confirm that many men died after they had reached some kind of support in the water. Protection from the environment is also needed if a man is to have a good chance of survival. A shipwrecked man may be faced by many dangers; but there is little doubt that the commonest cause of death after sinking at sea has been exposure to an environment in which the temperature of the body could not be maintained.

The importance of cold is not a new discovery. Men on the arctic convoy route were quite familiar with the dangers of immersion in ice-cold water, sometimes even exaggerating the risk. Going a little further back, Laurence Beesley, one of the survivors from the *Titanic*, who wrote a very clear account of the disaster, had no doubt that cold was the cause of death of those who were left struggling in the water when the ship sank. The *Titanic* only had sufficient boats to accommodate about a third of her complement, but Beesley pointed out that lifebelts (which they had) would keep those in the water afloat for hours, and the night, though cold, was perfectly calm: but the cries from the water ceased after forty minutes. It appears, however, that until recently all life-saving equipment was designed solely to prevent drowning, and protection from cold was given little if any consideration.

The temperature of the human body is normally held constant by a balance between the production and loss of heat. Heat is continuously being produced by the oxidation of foodstuffs, at a rate which is fairly constant at rest, but is much increased by muscular activity. Heat is lost from the surface of the body by radiation, conduction and convection, and also as the latent heat of evaporation of moisture. Over what may be called the comfort-

able range of environments, balance is maintained by adjusting the rate of loss of heat. This is accomplished by varying the flow of blood between the deep regions of the body and the surface, particularly of the extremities. In hot environments, or when the rate of heat production is raised by muscular exercise, secretion of sweat comes into play, and the heat lost by evaporation is greatly increased, provided that the environment is dry enough for the sweat to evaporate. In cold conditions, heat production is increased by involuntary muscular activity, first in the form of a general increase in muscular tension, which later develops into shivering. Either of these methods of coping with a wider range of environments is associated with some discomfort, even though heat balance is being maintained.

We must now consider the effects of immersion in water. In air, even if a man is naked, he is not devoid of insulation. The layer of air in immediate contact with his body is stationary and 'stuck' to the surface. Air is also trapped in folds of the skin such as the armpits and between the thighs. With its low specific heat — that is, the quantity of heat which is required to raise the temperature of one gramme by 1°C — this layer of air can warm up to approach the surface temperature of the body without abstracting very much heat. The presence of a layer of warm air then reduces the gradient of temperature away from the surface of the body, and therefore the flow of heat to the environment. Most practical methods of heat insulation also depend on trapping air in some way. Clothing is an example: warm clothes greatly increase the amount of air trapped around the body, some of it being held between the layers of garments and some enmeshed by the fibres of the fabric. A man who is immersed in water at a temperature below that of his body loses heat much faster than in air at the same temperature. This is partly because water conducts heat more readily than air, but more important is the high specific heat of water. Far more heat would be needed to raise the temperature of a layer of water to near that of the body than is needed for a layer of air. Furthermore, in water the trapping effect is negligible compared with the effects of convection and other currents. Cold water makes effective contact with every

part of the body's surface, and is constantly being replenished before its temperature has had time to rise appreciably. The wearing of ordinary clothing gives little protection under water, for the air it encloses is displaced by water.

It is found that in general heat balance is not maintained in water below about 20°C (68°F), although heat production is increased by shivering. The temperature of the body steadily falls, and death eventually ensues. In water at 0°C the rate of fall may be 10°C per hour. The normal human body temperature is about 37°C ; the lowest body temperature which is likely to be survived is in the region of 25°C . It follows that in freezing water survival for a period of the order of an hour would be expected. Data collected by G. W. Molnar after the war show that experience bears this out. Shivering is unable to prevent the fall in temperature because it does not raise heat production enough, and because it only builds up gradually. It may take twenty or thirty minutes for shivering to reach its greatest output of heat. If during this time the body temperature is falling, the effectiveness of shivering is much reduced. What is true for the body as a whole is also true for a part of it. A single limb immersed will lose more heat than can be supplied to it, and prolonged immersion of limbs in cold water leads to injury or death of tissues, and long-lasting disability.

There is one means by which chilling in cold water may be prevented. This is swimming. The heat produced by the muscular effort of swimming has been measured, indirectly, from the rate of consumption of oxygen. Calculation suggested that the heat produced, even during comparatively slow swimming, would be enough to balance the heat lost in ice-cold water. Further experiments in which three men swam for a short time in ice-cold water showed that they did maintain normal body temperatures. These results suggested that people immersed in cold water would prolong their survival by swimming for as long as they were able. It has, however, since been objected that this may only be true for people with a fairly fat body build, who in any case are able to withstand immersion in cold water better than thin people. In thin individuals the action of swimming may increase the heat

losses as well as the heat production, and so destroy the advantage. This is because swimming increases the flow of blood through the muscles of the limbs, and much of the additional heat is produced in these muscles. In the absence of an insulating layer of fat, heat may rapidly be lost from the limbs to the water.

Further experiments are needed to settle this question. If it is confirmed that there is a difference depending upon body build, it will be important to know what proportion of people come into each category, and whether the difference still holds good for men who are wearing cold-weather clothing. Conversations with survivors have suggested that there are a considerable number who survived prolonged immersion in cold water, and who had to swim for one reason or another, such as not having a life-jacket. It is tempting to suggest that these men survived because they had to swim. There is also little doubt that Channel swimmers are able to stay in cold water for as long as they do because they are swimming; but they are nearly always people with a good layer of fat. Obviously swimming can only be a temporary expedient for most people, but from all points of view those who go to sea are probably wise if they become good swimmers, and keep in sufficiently good training to be able to swim for a useful length of time without becoming exhausted.

It is sometimes said that sudden immersion in cold water may cause immediate death from 'shock'. It is difficult to find evidence to prove this, but death from shock does not seem to have been observed in animals, and in man it must be an uncommon occurrence. Furthermore, even a man who has no special protective clothing and who does not swim takes an appreciable time to cool to a fatal level. There are accounts of men dying instantaneously or within a minute or two of immersion in arctic seas, but some of these probably exaggerate the rapidity of death, and even when the accounts seem to be reliable the cause of death remains a matter for conjecture.

In cold surroundings a castaway who is not immersed in water may still be unable to maintain heat balance. The rate at which heat is lost to cold air depends very much on the amount of air movement, because the layer of warm air on the surface of the

body becomes progressively thinner as air movement increases. A three mile an hour breeze doubles the cooling power of cold air, and a wind of 15 to 20 m.p.h. trebles it. In a lifeboat, snow and spray, leaks, and wet garments can all make matters worse. R. A. McCance and his collaborators have analysed the information collected from survivors of some 450 ships sunk during the war. The records show that where the sea temperature was below 5° C, 22 per cent of the men who were in lifeboats died, and 42 per cent of the men on rafts. These figures may give too favourable an impression, because many of the ships sunk in northern waters were in convoy, and other ships were in the vicinity. Many of the survivors suffered from injuries due to cold. Where the sea temperature was above 20° C the corresponding death rates were 5 per cent for men in lifeboats and 9 per cent for men on rafts.

In hot climates water economy is the important physiological consideration. Conditions over the tropical sea are never such that a normal body temperature cannot be maintained, but heat absorbed from the environment, particularly as solar radiation, is got rid of by the evaporation of sweat, and this expends water.

The body of a man weighing 70 kilogrammes contains about 60 per cent of water, that is, about 42 litres. This is normally maintained constant by a balance between losses and gains. At rest, and in a temperate climate, a man unavoidably loses about one and a half litres of water a day by evaporation from the skin and lungs and in his urine. Some water is formed within the body by oxidation of foodstuffs: this amounts to 0.3 litres a day on a normal diet. A daily intake of 1.2 litres, or about a quart, of water from food and drink will therefore just maintain an average man in water balance under the stated conditions. The sensation of thirst normally prompts a rather larger intake than the minimum required for balance, and the excess is added to the urine. If a man is deprived of water, or has an intake too small to balance his losses, the rate at which the water in his body will be depleted can readily be calculated. A deficit equal to 5 per cent of the body's weight causes discomfort and loss of efficiency, one of 10 per cent is likely to be disabling, and one of 20 per cent may be

lethal. It will be seen that even with no water at all a man might survive for between one and two weeks.

If sweating is taking place the deficit may become dangerous much more rapidly. It is possible to lose 2 litres of sweat in an hour by working in a hot climate. Even an inactive man lying on a raft exposed to the tropical sun may lose 5 litres during the day. It will seldom be possible to provide water on anything like this scale, and unless the losses are prevented there is a danger of death from loss of water within two or three days. Nevertheless, it must be made clear that in practice hot climates present far less hazard to castaways than cold ones. This is illustrated by McCance's figures quoted above. All the very long lifeboat voyages on record have been made in tropical latitudes. The longest in McCance's series lasted for 77 days, and took place in the Atlantic about 10° north of the equator. Rain is the saving factor which makes these voyages possible. With improvised means for catching it, however, rain is more help when the number of individuals in a craft is small; this, as well as the exceptional toughness of some individuals, may be one reason why the longest voyages have only been completed by one man or by a very small number.

THE NAVAL LIFE-RAFT

We can now see what requirements a life-craft ought to meet. The same equipment should be effective in any part of the world. Since the temperature of the sea is often below 20° C, a craft which supports the castaways completely out of the water is required. A waterproof and insulating buoyant suit would be the only possible alternative. Such a suit may be the solution in certain special cases, for instance for men escaping from a sunken submarine, but it cannot give the same degree of safety and protection as a life-craft, and is to be looked upon only as a temporary measure. The life-craft must also give adequate protection against cold weather. This again might be provided by special protective clothing. It would, however, be difficult to secure good enough protection with any practicable form of clothing, and there are other difficulties, in that it probably would not be

feasible to carry suits for the entire complement of a large fighting ship in an accessible position, nor would there always be time to don the suits before abandoning ship.

All cold-weather clothing, however, functions by trapping air. A tent will also do this, and is actually a much more effective means of keeping a number of men warm. The possibility of fitting a tent on to a life-craft was therefore considered. A tent for this purpose should be nearly airtight, except for controllable openings for ventilation, so that it will hold the same envelope of air around the occupants for a substantial time. Comparatively little heat will be required from their bodies to warm up this air, and thereafter little heat will be lost to it. The walls of the tent should be double, enclosing an air space, to prevent the air inside the tent losing too much heat to the outside world, and similar air insulation in the floor is very desirable. Twenty men at complete rest produce as much heat as a 'two-unit' electric fire; if this heat is retained around them by the tent instead of being immediately dissipated to the winds, the whole outlook for survival in cold climates is changed.

In the tropics, the first essential is to provide shade from solar radiation, but the castaway must remain exposed to any breeze there may be. The importance of these two factors was demonstrated in experiments carried out in the United States during the war by J. L. Gamble and A. M. Butler, who showed that if men also remained at rest, and kept their clothes moist with sea water during the day, evaporative water losses were no greater than under ideal conditions in a temperate climate. A tent will give shade, but to be useful in hot climates it must be capable of opening sufficiently to allow a free flow of air.

The first tented life-craft consisted of a bell tent rigged with a pole and guy ropes on a circular aircraft dinghy. The current life-craft which evolved from this is shown in the photographs reproduced as Inset 8-9. It is an oval inflatable boat made of rubberized canvas, 16 ft long by 10 ft wide, and seating 20 men in comfort. Packed, it occupies 5 ft by 2 ft by 1½ ft. The weight is about 200 lb. Initial inflation is from cylinders containing carbon dioxide and nitrogen. The inflation mechanism is operated by the

pull on the painter when the raft is thrown overboard, or when it floats up from a sinking ship. In the latter event, if the raft is lashed down on the ship, the lashing must have a release operated by water pressure a few feet below the surface. Unless the ship sinks, the inflated raft remains attached to it by the painter until the occupants decide to cut themselves adrift. The tent is automatically erected by arches which inflate with the rest of the raft. The ends of the tent are sleeve entrances which can be completely closed and tied, partially opened to any desired extent, or completely rolled back. The raft rides head to wind at a sea anchor. In the arctic the windward sleeve is kept closed and only the leeward sleeve is opened a small amount for ventilation. In the tropics both entrances are fully opened, and the outer sleeve can be supported on poles to extend the shade to men at the extreme ends of the raft. The tent and sleeves are double, and the floor is cellular and inflated. An inflated thwart runs across the width of the raft, and a seat round the inside of the buoyancy tube. Hand bellows are used to inflate the seat and floor and to 'top up' the buoyancy tube. In hot climates the floor is not inflated during the daytime.

The raft was put to practical trial during 1950-51 at Tromsø, N. Norway, in Great Britain, and at Singapore. These places were chosen to provide average arctic, temperate, and tropical climates. Two types of test were performed in each place. First, several rafts of different sizes and types, all fully manned, were exposed together to the same sea and weather, and the conditions inside the rafts were compared. These tests each lasted for about half a day. Longer tests were next done, in which the same team of eight men - seven sailors and one scientist - acted as experimental subjects throughout. At Tromsø and Singapore these tests lasted for 4½ and 5 days respectively; the eight men lived for these periods in the 20-man raft with double tent and floor and sleeve entrances. It was thus possible to study their physiological state in each place, and to compare the effects of the different climates. In England the longer tests were made in a room in the laboratory which could be kept at a constant temperature, and the party spent three periods each lasting three days in this room. This

made possible more elaborate measurements concerned with the effects of different rations.

In the arctic, the oval 20-man rafts were found to be warmer than circular 8-man ones. Double tents, floors, and sleeve entrances were found to be well worth while. During the short test, the temperature in the 20-man double-tented raft rose from 2° C to 31° C (88° F) in 40 minutes after boarding, with 20 men on board and the entrances completely closed. At this time the lack of ventilation had led to a carbon dioxide concentration which could not be tolerated any longer (6½ per cent); after adjusting the ventilation to what seemed comfortable a steady temperature of 23.5° C (75° F) was maintained, and the carbon dioxide was found to be at the harmless level of 0.5 per cent. Condensation of moisture was not noticeable inside the double-tented rafts but was unpleasant in the single-tented ones.

During the 4½-day test in the arctic, conditions outside the raft varied from -9.5° C, with a gale blowing, to +0.5° C and sunshine. With only eight men on board, the temperature inside ranged from 10.5 to 15° C (50 to 60° F). Such temperatures are not comfortable, and, to quote from the official report on the test, 'on the last day the subjects began to draw upon their reserves of energy and good humour'. But survival was never in doubt. It would have been most precarious in an open craft.

In these tests the occupants had boarded the rafts dry. This, of course, might not happen in real life, and a subsequent test was carried out to investigate what happened if the men's clothes were wet. Ten men boarded the 20-man raft after swimming to it. The results differed little from the earlier tests. Half an hour after boarding the temperature inside had risen from 3° C to 18° C (65° F), and it remained steady at this level. The men were comfortable and in heat balance. After they had boarded and the sleeves had been almost closed, the air in the tent rapidly became nearly saturated with water vapour. Thereafter little further evaporation could take place, and so evaporative cooling was negligible.

In the tropics the double-tented 20-man raft was the coolest. Air movement through the tent was a half to two-thirds of that

outside. During the five-day test the most significant finding was that the occupants of the raft lost exactly the same average amount of water by evaporation as they had in a comfortable room in England. This confirmed that providing shade, maintaining air movement, avoiding activity, and keeping clothes wet with sea water during the day reduced the loss of body water by sweating to negligible proportions. The chief physiological problem in the tropics can thus be completely overcome. As might be expected, the men were in very good condition at the end of the test. All returned to their normal activities within an hour or two of leaving the raft.

WATER AND FOOD

After cold, the castaway's most pressing problem is the supply of water. Men have survived for many weeks without food, but survival for two weeks without water is unlikely even under favourable conditions, and serious deterioration would occur much sooner. One might conclude that where space for rations is limited only water should be carried. This is not quite true, because water economy is affected by diet.

When the body is short of water, the kidneys conserve water by concentrating the urine up to the maximum concentration of which they are capable. ('Concentration' for this purpose means the total concentration of osmotically active particles – ions and molecules – which can be measured by a determination of the freezing point or a similar method). As a result the volume of urine which continues to be formed is determined by, and is roughly proportional to, the total quantity of soluble substances being excreted. The most important of these are urea, which is the end product of the breakdown of proteins in the body, and various salts. During complete starvation the amount of protein broken down is quite large, and therefore so also are the output of urea and the minimum volume which the urine can have. It has been found that giving a small amount of carbohydrate food reduces the breakdown of protein to much below the level found in starvation. In the experiment in the constant temperature room giving 100 grammes of carbohydrates a day, as barley sugar sweets,

reduced the average amount of protein broken down in the day by each man from 78 to 43 grammes; the average daily volume of urine was reduced from 680 to 360 millilitres. The mechanism of this effect is not fully understood. Its practical consequence is that a ration of 100 grammes of carbohydrate a day in any convenient form will save more than its own weight of water. It is therefore worth including this amount in life-raft rations even though the amount of water that can be carried is correspondingly reduced.

A minimum ration for life-rafts of 0.5 litres of water and 100 grammes of barley sugar sweets a day for each man has been proposed. This is provided for 20 men for 5 days, and is the chief item in an 'equipment pack' attached to the raft. The water is at present stored in tins, but these will probably be superseded by polythene bottles. The rations should not be started until the second day adrift, since on the first day the kidneys may not be conserving water fully. Beyond this, there is no advantage in spinning out the rations to make the total quantity last longer. Doing so will not lengthen the time for which survival is possible, and will mean that if men are picked up at an earlier stage they will be in a worse state than they need have been. The great majority of castaways in the last war were rescued within five days. At this time, an average man living in one of the current naval rafts, and provided with the above ration but with no other sources of water, should be fit enough to return to duty immediately. If not rescued earlier he would remain reasonably fit for over a week, and should survive for two weeks or more.

It may be mentioned here that tablets for preventing seasickness are also included in the raft equipment. The motion of a flexible raft is quite unlike that of a ship, and seasickness is therefore likely even among hardened sailors. It is important to prevent sickness, because of the loss of water it would entail. Various possible drugs have been compared in the rafts, and hyoscine has been found to give the best protection of those tried.

If food above the minimum scale is provided in rations the extra may well be largely fat, since fat provides the most energy in a given weight. The food must be in a form which can be eaten

by men with dry mouths. Toffees with a high percentage of fat, fudge, and sweetened condensed milk are all suitable items. Only very small amounts of protein should be allowed, and salt must be excluded. Both increase the output of solutes in the urine, and therefore increase the loss of water.

There are other possible sources of water besides the rations. Rain is the most important of these. It can be collected in the roof of the raft, which has a hole in the centre leading to a tube inside the raft; if this is pulled down the whole roof becomes an efficient funnel. The rain collected is stored in canvas saddle tanks stowed across the centre thwart. Sun stills (Inset 11) may also be provided. These are made of translucent plastic and pack into a small space. When inflated they produce a useful yield of fresh water by using the sun's radiant energy to distil sea water. In tropical sunshine a litre of water a day can be obtained from one still. Chemical de-salting kits are not used in naval rafts, as they are at present very expensive, they are somewhat troublesome to operate, and it is possible to carry an adequate ration of water as such. The kits will give about a fivefold gain in terms of space or weight, and have their application where this is all-important, for instance, in aircraft dinghies. Stills heated by fuel have been tried out, but at present a still which would be reliable and safe in a raft appears to be a remote prospect.

DRINKING SEA WATER

It cannot be stressed too strongly that castaways should not drink sea water. This piece of advice is based on very good evidence, but nevertheless is challenged from time to time, so it may be worth examining in some detail the reasons for making it.

When a man is depleted of water, the harmful effects are due much more to the rise in the concentration of salts in the fluids of his body, which follows the withdrawal of water, than to the actual reduction in the volume of the body fluids. The concentration of body fluids approximates to a 1 per cent solution of salt, whereas sea water is about $3\frac{1}{2}$ per cent salt. Human urine never contains more than about 2 per cent of salt. Hence, if sea water is drunk by a man who is short of water, it hastens the rise in the

concentration of his body fluids. This happens whether or not the salt is excreted in the urine, for the salt can only be excreted at the cost of a volume of water which is greater than the volume of sea water drunk. This is true for any volume of sea water, large or small. (To justify these statements further, the following points may be made. Only about half the total concentration of urine can be made up by salt, owing to the presence of urea: this is the reason why the limit for salt is as low as 2 per cent. During water depletion the urine is being concentrated up to a maximum total concentration; therefore, even if the amount of salt in urine is initially small, salt cannot be added without an increase in the volume. Finally, this increase in volume will always be that corresponding to a 2 per cent solution of the added salt, since more urea is excreted as the volume increases). Consequently, drinking sea water will always make the most serious effect of water depletion worse, and the lethal level will be reached sooner.

Mixing sea water with a ration of fresh water makes no difference to its effects. A man who does this will not survive for as long as if he had only drunk the ration of fresh water. Nor is the loss of salt by sweating a justification for drinking sea water. Sweat is a much more dilute solution of salt than body fluid (about 0.1 to 0.2 per cent salt), and therefore losing sweat increases the concentration of the body fluids nearly as much as losing water alone. If the salt were to be replaced without the large volume of water which was lost with it, matters would be made worse.

A factor which complicates the issue slightly is the effect of sea water on the distribution of water between the different compartments within the body. Part of the water in the body is contained within the cells and part is outside them. Normally when water is lost these two 'compartments' bear the brunt equally, in proportion to their volumes. This keeps the change in concentration in either compartment as small as possible. Sea water, however, causes water to move out of the cells to the extracellular compartment. Most of the symptoms and outward signs of water lack depend on the state of the extracellular compartment, and this movement of water probably explains the apparent benefit which

has been reported on some occasions after experimental drinking of sea water for periods of a few days. The cells, however, suffer more severe depletion, and those of the brain are the first to fail. Death after sea water drinking in lifeboats has apparently been due to failure of the cells of the respiratory centre, and supervenes rather suddenly, often preceded by mental derangement but not accompanied by the usual signs of water lack.

Perhaps the most convincing argument against the drinking of sea water by castaways is provided by the figures collected by McCance and his collaborators. In their series 163 of the men who made depositions had been given a page to fill in on which a question was asked as to whether sea water had been drunk in their craft. In those craft where sea water was recorded as having been drunk by one or more men the mortality from all causes was 39 per cent. Where no positive statement was made, the mortality was 3 per cent. The difference is the more remarkable, because not all the individuals in the sea water group drank sea water, and some of those in the other group may have drunk sea water without the fact being recorded. The two groups were comparable in other respects. As would be expected, the death rate with sea water was highest when the supplies of fresh water were smallest. Drinking sea water was sometimes reported as the cause of delirium, madness, suicide, or death in the life-craft; in no instance was it reported to have prolonged life.

It is doubtful whether fluid obtained from fish would be of value to castaways as a source of water. Surface fish can probably only be caught in the warmer regions of the seas, and attempts to get fluid from fish in the laboratory have only been successful when a massive press has been used. The composition of the fluid so obtained varies widely. The mean is about on the borderline between what would be beneficial and what would not, and it seems that surface fish yield a fluid of less favourable composition than deep-water ones. Fish as a food contains much protein, and is to be avoided unless supplies of fresh water are plentiful. Plankton, the minute forms of animal and plant life found in the sea, is almost certainly not useful. It could not in any case be caught from a drifting – as opposed to a sailing – raft, and as

caught it contains a great deal of salt, and its nutritive value is low.

PRACTICAL CONSIDERATIONS

Although this article is concerned mainly with physiology, some remarks on the practical aspects of using the new rafts may be added.

The advantages of the raft in respect of protection have not been gained at the cost of the strength and seaworthiness of the conventional ship's lifeboat. The buoyancy tubes make a surprisingly rigid structure, and at the same time the raft is not liable to be damaged during launching or by being thrown against the ship's side by a heavy sea, nor will it injure men who jump into it from a height or become trapped between it and the ship. The flat bottom sticks to the surface of the water, and water-pockets increase the stability. As a result the raft is not liable to be swamped or to capsize when occupied (if it should be capsized during initial inflation, it is unstable in the capsized position and can be righted by one man). It is true that lifeboats are navigable, but the raft is not intended to be. The intention is that the group of rafts should drift away slowly from the scene of the sinking, making it as easy as possible for rescuers to find them.

One of the advantages of an inflatable raft is the ease of launching. In the past, damage to launching gear in wartime, heavy seas, listing of the ship and other causes have sometimes prevented the successful launching of lifeboats. The *Princess Victoria* affords a recent example where this happened. Among the naval vessels sunk during the war it has been estimated that only a small proportion of the Carley floats carried became waterborne. As an inflatable raft can be thrown over the side by two men in a matter of seconds, and can be arranged to launch itself entirely automatically if the ship sinks very quickly, it seems reasonable to hope that a high proportion of the new rafts will be launched.

The disadvantages of the raft include the cost and need for inspection and maintenance; the fact that the raft cannot move to come to the assistance of swimmers; and, in wartime, suscepti-

bility to damage by splinters or fire. The cost of the new raft is greater than that of the primitive rafts and other devices classified as 'buoyant equipment', but it is much less than that of lifeboats and their launching gear. Susceptibility to damage is also only a relative drawback. A raft when packed exposes a very small area to splinters. Since inflation is at low pressure (2 lb. per sq. in.) punctures do not lead to rapid deflation, and means of repairing them are provided. The buoyancy tubes are divided into two main compartments with non-return valves, and if one section is badly damaged the other half of the raft will still support all 20 men.

The greatest outstanding problems are getting men in the water to rafts, and providing reliable means of boarding in difficult conditions. Unlike lifeboats, rafts can probably not be manned at deck level and then lowered into the water. As matters stand, after launching the rafts the passengers and crew of a merchant ship would have to abandon the ship by ladders down the side or similar means, or possibly by jumping, and many would have to board the rafts from the water. The problem is to reduce the risk at this stage. A powered lifeboat used in conjunction with the rafts may be part of the solution, and it would also be useful for congregating rafts after boarding. The ideal ladder for boarding from the water, which must pack up with the raft, and which can be found and climbed easily by a cold, wet, exhausted man, encumbered with a life-jacket and perhaps covered in oil fuel, has yet to be developed. A strop with which men already in the raft can haul in helpless or unconscious men from the water is under trial. Intensive study of all these problems, and realistic practical trials, are well worth while, for it can fairly be claimed that once men are in the raft, their chances of survival should be extremely good.

FURTHER READING

- Beesley, L. *The Loss of the 'Titanic'*. London, Philip Allan, 1912.
 Critchley, M. *Shipwreck-survivors. A Medical Study*. London, J. & A. Churchill, 1943.
 Gamble, J. L. 'The Water Requirements of Castaways.' *Proceedings of the American Philosophical Society*, 88, 151 (1944).

- Gamble, J. L. 'Physiological Information gained from Studies on the Life-raft Ration.' *Harvey Lectures*, 42, 247 (1947).
 Hervey, G. R., and McCance, R. A. 'The Effects of Carbohydrate and Sea Water on the Metabolism of Men without Food or Sufficient Water.' *Proceedings of the Royal Society*, B 139, 527 (1952).
 Hervey, G. R. and McCance, R. A. 'Emergency Rations.' *Proceedings of the Nutrition Society*, 13, 41 (1954).
 McCance, R. A., Ungley, C. C., Crosfill, J. W. L., and Widdowson, E. M. *The Hazards and Fate of Men in Ships Lost at Sea, 1940-1944*. Medical Research Council Special Research Report Series. London, H.M.S.O., in press.
 Marriott, H. L. *Water and Salt Depletion*. Springfield, Ill., C. C. Thomas, 1950.
 Molnar, G. W. 'Survival of Hypothermia by Men immersed in the Ocean.' *Journal of the American Medical Association*, 131, 1046 (1946).
 Newburgh, L. H. *Physiology of Heat Regulation and the Science of Clothing*. Philadelphia, W. B. Saunders, 1949.

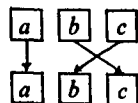
THE ALGEBRA OF PERMUTATIONS

H. A. THURSTON

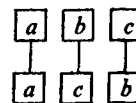
THE most familiar uses of mathematics – at any rate for the non-mathematician – are, first, for calculating with numbers, and, second, for proving things about points, lines, etc., in space. The first use includes arithmetic, elementary algebra, calculus, and statistics; the second includes geometry and topology. But these merely happen to be familiar because they are fairly simple to deal with and of obvious practical use. Many things besides points and numbers can be treated by the symbolic calculational methods which we call ‘mathematical’. The aim of this article is to show how mathematical methods can be applied to permutations.

By ‘permutation’ a mathematician does not mean what a football-pool solver means by the word, nor does he mean quite what is meant in elementary algebra. In elementary algebra about all one does to permutations is to count them: a typical question is ‘How many permutations of abc are there?’ and the answer is ‘Six: namely abc , acb , bac , bca , cab , and cba .’ A permutation is a rearrangement of the given objects a , b , c ; and we have listed the effects of all possible rearrangements. In advanced algebra we are interested not in the effect of rearrangement so much as in the process of rearrangement. We want to say, for example, that $fabced$ is the same permutation of $abcdef$ as $cdefab$ is of $defabc$.

We therefore define a permutation in such a way that it represents the process of replacing each of the objects being permuted by the appropriate object. For example,

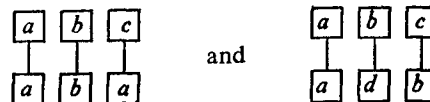


is a permutation of abc . It will be referred to later as permutation (i). For convenience of writing and printing one usually does not bother to put the heads on the arrows. Each permutation is to be regarded as a single mathematical entity, and may well be denoted by a symbol: here Greek letters will be used. Let us, in fact, denote the permutation above by θ . We denote the effect of θ on, say, abc by $(abc)\theta$. Thus $(abc)\theta = acb$. Similarly $a\theta = a$, $b\theta = c$, $(ca)\theta = ba$, and so on. It is, of course, possible to rearrange the letters in the bottom line so that the ‘arrows’ all run vertically downwards:



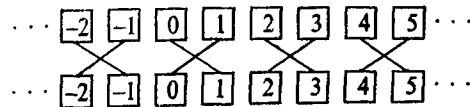
This is nothing more or less than carrying out the permutation on the abc in the bottom line.

However, if one does this, one must be careful to see that the elements in the bottom line are precisely those of the top line, each occurring once and once only. For example,

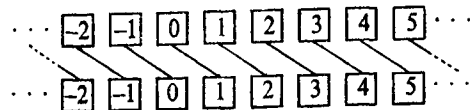


are not permutations.

There is not the slightest reason why we should not have a permutation of an infinite number of objects. For example,

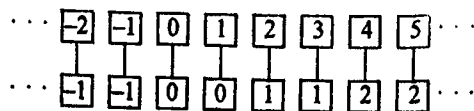


is a perfectly good permutation of the integers, and so is

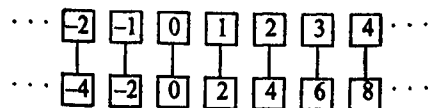


There is one point we must watch if we apply the permutation to the bottom row, to make the 'arrows' vertical. If we are permuting a finite number of objects – and if no extraneous objects occur in the bottom row, which is quite easy to check – we need only make sure that each object occurs at least once in the bottom row, and it then follows that no object can occur more than once. Alternatively, it would have been enough to make sure that no object occurs more than once, because this would imply that each object must occur at least once. But for infinite numbers of objects we must verify both points: neither is enough on its own.

For instance, in

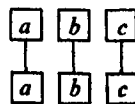


every integer appears twice in the bottom row; and in

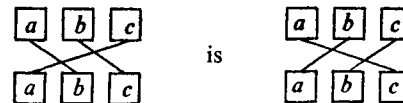


no integer appears more than once, but the odd integers do not appear at all.

The simplest permutations, though they are by no means unimportant ones, are those which cause no change in the objects to which they are applied. They are called 'identity permutations'. The identity permutation of abc is

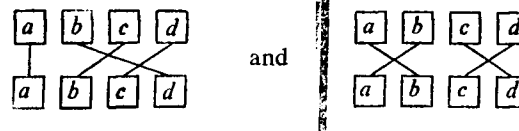


if θ is the permutation (i), which changes abc into acb , the opposite process – i.e. the permutation which changes acb into abc – is called the 'inverse' of θ , and is denoted by θ^{-1} . The inverse of a permutation can be obtained by reversing all the 'arrows'. For example, the inverse of

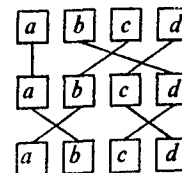


and the inverse of θ happens to be equal to θ , as is easily verified.

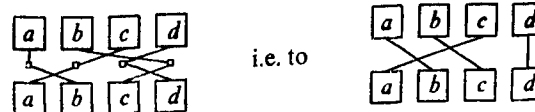
In elementary algebra we add, subtract, multiply, and divide. Let us for the moment consider just one of these operations: addition. It is a process for getting from two given numbers to a third, uniquely determined by them, which we call their sum. Similarly for the other three, the uniquely determined number being the difference, the product, and the quotient respectively. It is the manipulation of these operations which constitutes elementary algebra. If we can define a similar operation on permutations, we can apply mathematical reasoning to them. The way this is done can be best illustrated by examples. Suppose we are given two permutations, α and β , of $abcd$:



We can apply α to $abcd$, and then apply β to the result. This double process can be represented by the diagram



which can be abbreviated to



that is, it is itself a permutation. It is sometimes called the 'result'.

tant' of α and β and written $\alpha^\circ\beta$, the symbol $^\circ$ being analogous to $+$, $-$, $\times$, or $\div$; but more often it is called their 'product' and written $\alpha\beta$. In fact, the language and notation of algebra is consistently taken over: formation of the 'product' $\alpha\beta$ is called 'multiplication' of α by β ; $\alpha\alpha$ is called the 'square' of α and written α^2 ; the process of finding a ξ such that $\alpha\xi = \beta$ is referred to as 'division'; α^{-1} is called the 'reciprocal' of α ; the identity permutation is called the 'unit'; and so on.

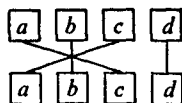
The last two terms come from the obvious facts that the product of the identity permutation with any permutation θ must simply be θ , and that the product of a permutation with its inverse must be the identity permutation — i.e. doing something and then undoing it is equivalent to leaving things alone.

There are a couple of important properties which some operations have and some have not. Consider, for example, $x + y + z$ and $x \div y \div z$. The first makes sense, but the second is ambiguous: it might mean $(x \div y) \div z$ or it might mean $x \div (y \div z)$, which is not the same thing. The reason why $x + y + z$ makes sense is that, although it might stand for either $(x + y) + z$ or for $x + (y + z)$, these two are always equal. The same is true for $(x \times y) \times z$. Admittedly, $x - y - z$ makes sense, but that is because we have made the convention that it stands for $(x - y) - z$ and not for $x - (y - z)$. The same applies to

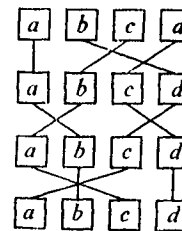
$$\begin{array}{ccc} & z & \\ y & & (y) \quad z \\ x & \text{which stands for } x & \text{and not for } (x) \end{array}$$

though any teacher of algebra will confirm that this convention causes many mistakes and that it is wiser to put the brackets in.

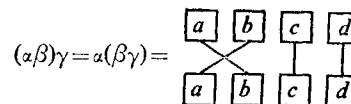
The property which addition and multiplication have, by virtue of which the brackets can be safely omitted, is called 'associativity'. We naturally want to know whether multiplication of permutations has this property. It is easy to see that it has. Suppose that γ is



and α and β are as before. Then $(\alpha\beta)\gamma$ and $\alpha(\beta\gamma)$ are both represented by the same tableau, i.e. by



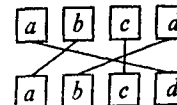
That is to say,



The same considerations clearly apply to any three permutations of any set of objects, and a general proof could easily be written out if required.

A rather similar point, which has no analogue in algebra, is that if x is any of the objects permuted by α and β then $x(\alpha\beta) = (x\alpha)\beta$ and so can be written as $x\alpha\beta$. This is easily seen from the tableau, and a general proof could be given if required.

The second property which some operations have is called 'commutativity': we say that addition and multiplication are commutative because $x + y = y + x$ and $x \times y = y \times x$. But we cannot say that $x - y = y - x$, or that $x \div y = y \div x$, or that $x^y = y^x$; and so subtraction, division, and exponentiation are not commutative. Is multiplication of permutations commutative? No. If we work out $\beta\alpha$ it comes to



which is not the same as $\alpha\beta$.

This means simply that we have to be careful about the order in which we write the factors of a product. We cannot, for instance, given $\eta\xi$ in one line of a problem, cheerfully alter it to $\xi\eta$ in the next because we like factors to be in alphabetical order.

We are now in a position to work out problems analogous to some of the simpler problems of algebra, such as the following. If we are given the values of a and b we can solve the equation

$$x + 2a = b \quad (\text{Eqn 1})$$

or the analogous equation for multiplication instead of addition which is

$$xa^2 = b \quad (\text{Eqn 2})$$

The analogous equation for our algebra of permutations is

$$\xi\alpha^2 = \beta$$

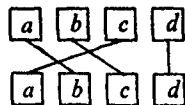
However, Equations 1 and 2 are respectively the same as $2a + x = b$ and $a^2x = b$. Owing to the non-commutativity of multiplication of permutations these are analogous to a different equation,

$$\alpha^2\xi = \beta$$

with a different solution. The solutions of the two permutation equations are easily seen to be

$$\xi = \beta(\alpha^2)^{-1} \text{ and } \xi = (\alpha^2)^{-1}\beta \text{ respectively.}$$

Given the values of α and β these are easily worked out. For example, if α and β are as above, the solution of the second equation comes out to

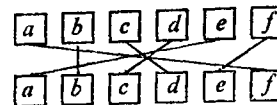


Again, there are problems which reduce immediately to equations. Consider the following, for example. 'Four boys stand in a row. The second one goes to the end and the last two move down one, the first one standing fast; this happens twice. What rearrangement is now necessary to bring the first two boys into each other's places and the last two into each other's places? If we call the boys a, b, c , and d we see easily that the final permutation is β , and the rearrangement which takes place twice

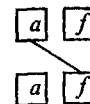
is α . Thus the answer to the problem is the solution of the equation $\alpha^2\xi = \beta$, which we have already worked out. In fact it is: the fourth boy stands fast, the third moves down two, and the other two move up one.

Country-dance enthusiasts can apply this technique to the progressive movement in a minor-set dance. (It will not, of course, help their dancing in the least.)

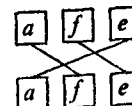
The tableaux we have been using so far are very useful for the portrayal of permutations of finite numbers of objects in a way which catches the eye; but they are clumsy compared with most mathematical notations. It is, however, quite easy to condense them; and the following example illustrates the process. Starting with, say,



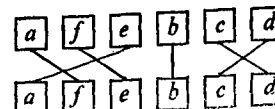
we choose any letter, a say, and rearrange the top line so that the letter into which a permutes – in this case f – comes second. So far we have



We then place the letter into which f permutes next, and so on until we get back to a :

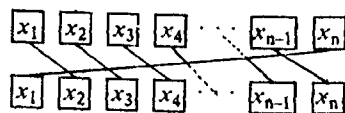


We choose another letter, b say, and do the same, and so on until we have treated all the letters. In this case we finish up with



which we may call permutation (ii). In general, we see that a

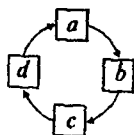
permutation will be reduced to a number of separate 'cycles' of varying lengths in which each letter permutes into the next one except for the last, which permutes into the first. A typical cycle is



Since the pattern of the 'arrows' is always the same, and since the bottom row is the same as the top, we can omit these. We use square brackets to separate the different cycles in a permutation; and so permutation (ii) for example becomes, in the cycle notation,

$$[afe] [b] [cd]$$

Of course, it does not matter in what order the cycles are written down; $[b] [afe] [cd]$ denotes the same permutation. Nor does the cyclic order in each cycle matter; $[abcd]$, $[bcda]$, $[cdab]$, and $[dabc]$ represent the same cycle. But $[cbad]$ is different. In fact, it is best to regard the notation as being in the 'bracket' form only for convenience of printing, and to consider that $[abcd]$ really stands for some such scheme as



Inverses can be written down very easily in this notation: the whole formula is simply written backwards. For example, the inverse of permutation (ii) is $[dc] [b] [efa]$.

Multiplication can be carried out quite easily, too. For example, to multiply permutation (ii) by $[ab] [cde] [f]$, which we may call permutation (iii), i.e.

$$[afe] [b] [cd] \times [ab] [cde] [f],$$

one starts with any letter, say a . This permutes into f under (ii), and f permutes into f under (iii). Therefore we start with af . Then f goes to e which goes to c by permutations (ii) and (iii)

respectively. We now have $[afc]$. And so we carry on until we reach a again. In this case we get $[afceb]$. We then carry on with the other letters, if any. We finish up with $[afceb] [d]$.

Calculation of powers of permutations is particularly easy. To write down the n th power of a permutation, one picks out every n th letter in each cycle. For example, to write down the third power (i.e. the cube) of $[abcde] [fghijk]$, which we may call permutation (iv), start with a say. The third letter on is d . The third letter from d is b (remember that the formula represents a cycle so that the order is in effect $\dots abcdeabc \dots$), and so on, until we reach a again. We have now $[adbec]$. We start with f , which takes us on to i and then back to f , so we have $[fi]$; g and h behave similarly, so the second cycle of (iv) splits into three: $[fi] [gj] [hk]$. The result is that the cube of permutation (iv) is $[adbec] [fi] [gj] [hk]$.

One interesting use of the cycle notation is in solving certain quasi-mathematical problems of the following type: 'Each of six persons a, b, c, d, e , and f took a book to a bazaar. Each sold his book to one of the others. The buyer of b 's book sold his book to a . The man who sold his book to c bought a book from the buyer of d 's. If the buyer of e 's book did not sell his book to f , who bought a 's?' To make a start on the problem, we can define a permutation θ by letting $x\theta$ be the buyer of x 's book. Then the second sentence can be rewritten as ' $b\theta$ sold his book to a ', i.e. a is $(b\theta)\theta$; that is, $b\theta^2$. Now $y\theta^{-1}$ is, of course, the man who sold his book to y . Therefore the third sentence becomes ' $c\theta^{-1}$ bought $d\theta$'s book', i.e. $c\theta^{-1} = d\theta^2$, i.e. $c = d\theta^3$. Similarly the last sentence tells us that $e\theta$ is not $f\theta^{-1}$, i.e. f is not $e\theta^2$; and we are to find $a\theta$.

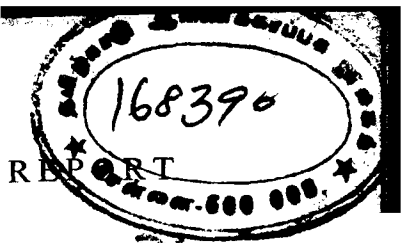
Let us reconstruct θ from these data. No one bought his own book: this means that every cycle of θ contains at least two letters. The possible patterns are therefore

- | | |
|-------|---------------------------|
| (i) | $[\dots] [\dots] [\dots]$ |
| (ii) | $[\dots] [\dots]$ |
| (iii) | $[\dots] [\dots]$ |
| (iv) | $[\dots]$ |

Now $a = b\theta^2$; this means that a is the second letter on from b in

θ , because, as we have seen, θ^2 is obtained by taking every second letter in θ . Therefore θ must contain somewhere the pattern $b \cdot a$. Similarly, because $c = d\theta^3$, θ must contain $d \cdot c$. Finally, it must not contain $e \cdot f$. Now $b \cdot a$ will not fit into (i), and $d \cdot c$ will not fit into (ii). Let us look at (iii); $b \cdot a$ will not fit into the second cycle, so we must have $[b \cdot a \cdot] [\cdot \cdot]$. Now $d \cdot c$ will not fit into the first cycle, and so we have $[b \cdot a \cdot] [dc]$. Now however we fit in e and f we get $e \cdot f$ occurring, so this pattern is impossible. We are left with pattern (iv). We have to fit $d \cdot c$ into $[b \cdot a \cdot \cdot]$, which can be done in two ways: $[bca \cdot d \cdot]$ and $[bda \cdot c \cdot]$. The only ways of fitting in e and f so that $e \cdot f$ does not occur are $[bcafde]$ and $[bda fce]$. In either case, $a\theta = f$.

This problem is simple enough to be solved also by trial-and-error methods, but the following one would be tedious without algebra: 'A, B, C, D, E, F, G, H, and J met, and each took another's umbrella. The owner of the umbrella taken by B took the umbrella belonging to the man who took the umbrella belonging to the man the owner of the umbrella taken by whom took the umbrella belonging to the man who took A's umbrella. The owner of the umbrella taken by D took the umbrella belonging to the man who took C's; and the same can be said of E and D respectively; of G and F; and of H and J. The man who took the umbrella of the man who took the umbrella of the man who took the umbrella of the man who took B's umbrella met A and B for the first time at the meeting we are describing. The owner of the umbrella taken by whom took the umbrella belonging to the man who took J's umbrella?'



RESEARCH REPORT

A. W. HASLETT

ATOMIC TIME

THE adoption of a 'physical second', based on an atomic standard of frequency, as distinct from the astronomical unit of time, has been proposed by Sir Edward Bullard, until lately Director of the National Physical Laboratory, Teddington. His proposal is based on recent research by L. Essen and J. V. L. Parry, of the electricity division of the laboratory. They have used an atomic standard - a resonant frequency of the caesium atom - to calibrate precision clocks more accurately than has been done using an astronomical standard of time. In spite of much talk of 'atomic clocks', this has not before been achieved. The 'physical second', if adopted, would be used in the most accurate laboratory measurements involving time or frequency. An immediate example would be in microwave spectroscopy—the measurement of the resonant frequencies of atoms and molecules within the centimetre range of wavelengths. Another, and practical, use would be in the specification of radio wavelengths. The 'physical second' would not replace the astronomical unit of time for everyday purposes; nor could it, since man lives by the sun.

The accuracy which has been realized is one part in a thousand million (1 in 10^9). This is equivalent to one ten-thousandth part of a second a day. For comparison, the precision time signals broadcast twice daily from the Post Office station at Rugby are accurate to little more than one hundredth of a second. For the measurement of broadcast radio frequencies, there is a demand already for an accuracy 10 times as great as this. The accuracy now achieved provides the further factor of 10 which keepers of standards like always to have in hand.

This, therefore, has been one reason for the interest taken

during the past ten years in the possible development of a physical standard of time. Another has been the progressive realization by astronomers that the clocks controlled by quartz crystals which provide the short-term rendering of Greenwich Time are collectively more uniform in their time-keeping than is the rotation of the earth. But as the source of an absolute standard of time, the vibrations of quartz crystals are open to the same objections as is any physical property of any material: namely that there is variation as between one specimen and another, and also in the same specimen with time. Even in their practical use—to fill in the gaps between astronomical observations—it has therefore been necessary to use crystal-controlled clocks in batteries, and to make regular inter-comparisons between clocks.

The obvious alternative has been to use as the standard of time a frequency characteristic of a particular atom or of the atoms of a molecule. But until the research now reported, this has not been shown to be practical.

The principle—that of spectroscopy—depends on the fact that the constituent particles of any molecule, atom, or nucleus can usually exist in a variety of configurations or states, each with a definite energy. A change from a higher level of energy to a lower one, or *vice versa*, is balanced by the emission or absorption of a quantum of radiation. Hence the connexion between energy levels, which are a property of the molecule or atom, and the frequency of the radiation emitted or absorbed, which is proportional to this energy difference. The new, or comparatively new, element in the situation is the extension of accurate spectroscopy to a lower range of frequencies, that of microwave radio, than those of optical or even of infra-red spectroscopy. In pure physics, this has meant that fine differences in energy level can be measured more accurately than before, and it was about this aspect of microwave spectroscopy that J. Little wrote in *Science News* 32. But for the purpose of obtaining an absolute standard of frequency, capable of transfer from atom to practical clock, the possibilities offered by microwave spectroscopy have been wholly new. The extension is here from optical frequencies, which can neither be measured directly nor handled as such, to radio

frequencies, to which electronic techniques can be applied, and which therefore can both be measured and used in comparisons.

The U.S. National Bureau of Standards has worked longest on the problem. The first attempt at the Bureau made use of a vibration of atoms in a molecule—that of ammonia. If the three hydrogen atoms of the molecule are pictured as lying in the plane of the paper, then the nitrogen atoms to which they are linked can be in one or other of two positions, either above the paper or below it. In any one molecule there is an alternation of positions, which has been likened to the inversion of an umbrella in a wind. The energy associated with this motion is small enough to bring it within the microwave range of spectroscopy; the corresponding wavelength in free space is, in fact, about 1 centimetre. This was the basis of the first 'atomic clock'. With it, as long ago as 1949, an accuracy of 1 in 10 million (1 in 10^7) was thought to have been obtained. But the possibility of improvement with this method has never looked to be quite good enough, and no absolute measurement of frequency has yet been published.

The first attempt to obtain a time standard by using a resonant frequency of caesium atoms (an atomic, as opposed to a molecular, frequency) was made again at the National Bureau of Standards, with which Dr Essen has been in touch. The transitions used are the result of interaction between the spins of the nucleus and of the electrons surrounding it. They are least complicated in an atom which has only one electron in its outermost electron shell, that is, in one of the alkali metals—lithium, sodium, potassium, rubidium, or caesium. Caesium in particular offers the advantage that it shows a convenient resonant frequency, one with an equivalent free wavelength in the neighbourhood of 3.25 centimetres; and for this frequency the National Physical Laboratory had precision apparatus already available. Other advantages are that caesium is easily detected, and that it exists, for practical purposes, as only one isotope. There are therefore no complications from the presence of atoms of more than one kind.

The observational method used was developed by I. I. Rabi about 1939 as a way of measuring the magnetic moments of

atomic nuclei. As such, it was described by Little in the article in *Science News* 25, already cited. In outline, an atomic beam is passed down an evacuated chamber, to which the radio-frequency field is applied. The condition to be determined is that of resonance between the atomic and the radio frequencies; and the means of detection are provided by a steady magnetic field, which deflects the atomic beam in the condition of resonance, but not otherwise. Under the conditions proposed, the resonant frequency is 9,192,632,050 cycles a second. The measurements are believed to be accurate to 1 part in 10^9 , 'the highest accuracy ever achieved in the measurement of any physical quantity in terms of a definitive standard', and still higher accuracy is thought to be possible. Finally, although the radio frequency part of the apparatus has to be made with extreme accuracy, a comparative measurement with it can be made in a few minutes.

With little loss of accuracy, the result of calibrating an existing quartz-crystal standard against the atomic standard could be broadcast, and so made available over much of the earth's surface. This was established by a recent series of experimental transmissions of radio frequency made from the British Post Office Station at Rugby and measurements of the received frequency made at Harvard University in the United States.* The accuracy of inter-comparison which was then achieved between quartz crystal standards on opposite sides of the Atlantic was of the same order (1 part in 10^9) as that now realized in calibrating a quartz crystal against the atomic standard in the same laboratory. Not all countries, therefore, would need to maintain standards.

The account given by Essen and Parry is followed by a further communication from Sir Edward Bullard, as director at the time of the National Physical Laboratory. 'Experience of the working of the machinery for obtaining international agreement on such matters', he comments, 'suggests that some years are likely to elapse' before a decision can be reached on the definition of a physical second. (Note inserted in proof. An interval seems indeed

**Science News* 35, 116 (1955).

likely. A recent international decision introduces a non-varying, astronomically defined second as the absolute unit of time. On this basis, an atomic standard would provide the means by which the absolute unit was made available, but would not define it—at least for the present.)

Sir Edward calls attention, too, to the bearing of this research on the suggestion, made by various theoretical physicists, that what are usually regarded as 'the constants of physics' may change with time. 'It is not self-evident', he points out, 'that the times shown by atomic and astronomical clocks will keep in step indefinitely.' The development of the caesium standard of frequency should enable the point to be settled by observation within a reasonable number of years.

Essen, L., and Parry, J. V. L. *Nature*, 176, 280 (1955).

Bullard, E. C. *Nature*, 176, 282 (1955).

STRUCTURE OF BIOLOGICALLY ACTIVE MOLECULES

The identification of active components in the structures of complex hormones and drugs is a line of research which promises in the long run to yield new understanding of their mode of action and encourages the hope that simpler substances will be found, and in time synthesized, which will do the same job as well or better. There are many researches which, at any one time, fall under this general heading. Several of current interest have to do with combinations of amino-acids as polypeptides through linkages of the same kind as are found on a larger scale in proteins. Another is suggested by recent work on the structure of vitamin B₁₂, the anti-pernicious-anaemia factor.

An objective which has long attracted attention has been to discover what it is about the insulin molecule which makes it an effective agent in controlling the sugar content of the blood, and so in the treatment of diabetes. There is here an intriguing conjunction of research. On the one hand, there has been the recent completion by F. Sanger and his colleagues in the Department of Biochemistry at Cambridge of the full working out of the chemical structure of insulin, the protein hormone produced by

the pancreas which affords the natural means of control. This has been a chemical *tour de force*, the result of ten years' work, which a generation ago it would have been difficult to imagine completed. On the other, there has been the beginning of some research in the West Indies which suggests, unexpectedly, that two relatively simple polypeptides produced by a plant share the essential property of insulin, that of reducing the sugar content of the blood. This was reported by Professor C. H. Hassall, of the University College of the West Indies, at the recent Bristol meeting of the British Association. It may be that neither of these substances will prove of practical value; they may be less good than insulin, or there may be other objections to their use. But in relation to the mode of action of insulin, the conjunction appears at least promising.

The structure of the insulin molecule was worked out by stages. At the start even the size of the molecule was not known correctly. It was known, however, that among its component amino-acids were a number of units of a double amino-acid, cystine, which in wool and hair provide the means by which adjacent chains are linked together. Sanger began, therefore, by investigating whether insulin consisted of only a single chain of amino-acids or of a number of chains linked together through cystine. He found that there was more than one chain, and settled down to work out their separate structures. To do this, he studied systematically the smaller fragments which could be obtained from them. Two main techniques were involved. One was a method, which he had devised himself in 1945, for identifying the free end-group (that with an uncombined amino group) in each fragment. The other was paper chromatography—the method introduced by A. J. P. Martin and R. L. M. Synge, in which a mixture of unknown substances is separated as so many spots on a sheet of filter-paper, and the different components identified by comparing the positions of the spots which those given by known substances. With a colleague, H. Tuppy, Sanger published in 1951 the complete structure of one chain. It consisted of a string of 30 amino-acids, every one of which was placed unequivocally in order—the only order in which 48 over-

lapping fragments could be pieced consistently together. Up to this stage, it had been expected that there would be four chains to be worked out. But in 1952 the problem was simplified by a new determination of the molecular weight of insulin, which showed that there were only two chains in the molecule. The structure of the second chain was worked out with E. O. P. Thompson and published in 1953. It consisted of 21 amino-acid units. The remaining task was to find out how exactly the two chains were linked together. This proved more difficult than might have been expected, but was completed last year after further work with A. P. Ryle, and later with L. F. Smith and Ruth Kitai.

The very complexity of the insulin molecule suggests with unusual force the obvious pharmacologist's question: is all of insulin necessary, or is the action of the whole determined by some smaller part of the molecule? One indication is afforded by differences in the structure of insulin according to the animal from which it is obtained. Sanger's main work has been on insulin from cattle. But he has done enough on insulins from pigs and sheep to show that they differ slightly both from each other and from beef insulin.

Professor Hassall's work, already mentioned, offers the possibility of new evidence from a direction which could not before have been suspected. It was presented during a symposium at Bristol on the chemistry of colonial products, in which Professor Hassall's part was to speak on medicinal products from tropical plants. From relatively well-worn material, he passed to recent work in his department which had started from the then unsolved problem of the cause of a disease known as 'Jamaican vomiting sickness'. Many different causes had been suggested, but without supporting evidence. The solution was found in a richly coloured, segmented fruit, the ackee apple, which is much eaten in Jamaica. A specimen of the tree grew conveniently near to the laboratory, and from the seeds of its fruit two active substances have been separated. They have been named hypoglycin A and B; both are relatively simple polypeptides and work on their structure is proceeding. The connexion with insulin is that a symptom of the sickness now attributed to the ackee apple is a lowering of blood

sugar content. A similar lowering has been shown to be produced in animals by the separated polypeptides. Since this is the essential effect produced by insulin, but by no other substance, there is an interest (to which Professor Hassall referred in an overseas broadcast) in the relation of structure and effect. The work is still at any early stage, and its value may lie in a variety of directions: in the search for common factors between these plant-produced and relatively simple polypeptides and the complex animal hormone; in the synthesis of the active polypeptides; or in that of related substances, possibly with greater activity or free from possible undesirable effects. The road to a simpler, preferably synthetic, substitute for insulin may still be a long one, and the ackee apple merely one step on the way. But at least a new point of attack has been provided.

A second example, also from polypeptide chemistry, was reported by Professor C. H. Li of the University of California at the Gordon Conference on Proteins held earlier in the summer at the New Hampton School, New Hampshire. Professor Li and his colleagues have been working for some 15 years on the hormones secreted by the anterior lobe of the pituitary gland. The research reported at New Hampton was on ACTH, the adrenocorticotrophic hormone, which stimulates the adrenal cortex to produce cortisone and related substances. It has been done in conjunction with I. I. Geschwind, J. S. Dixon, R. D. Cole, and I. D. Raacke. Together, they have worked out the complete structure of ACTH, an achievement only slightly less spectacular than that of Dr. Sanger and his colleagues in the case of insulin. ACTH consists of a sequence of 39 amino-acid units, linked together in a straight chain, with a molecular weight of 4,500. In addition, they have broken off a sequence of 11 amino-acid units from one end of the chain, and obtained a 28-unit chain which retains the biological activity of the hormone. The question, what is the smallest chain which retains activity?, is regarded as still open, although activity from an 8-unit chain has been claimed.

A more general question is about the inter-relation between the various hormones produced by the anterior pituitary, and

whether the different activities attributed to each may prove to be separable. Including ACTH, there are six of these hormones, and each of them was credited at first with a distinct and limited function. The approach is shown in the names which they were given, and by which they are still known: for example, the growth hormone, the follicle stimulating hormone (FSH), and the lactogenic hormone. Their actions are not really so simple. Thus in research published last year, Professor Li and Professor W. R. Lyons found that five out of the six hormones known to be produced by the anterior pituitary contributed to the mechanisms of control by which the female breasts are stimulated to develop and to secrete milk. Alternatively, if attention is concentrated on the effects produced by a single hormone, the situation is again complicated. For example, growth hormone has been found to act powerfully in ways not associated with growth; and ACTH is credited with a 'general activity', distinct from that produced at second hand through stimulation of the adrenal cortex. The question is therefore raised whether different portions of the complete molecules may not be responsible for different activities, so that by the synthesis of a number of simpler polypeptides and their use in combination more exact control of treatment could be secured. An incidental point is that, as with insulin, and also two of the hormones secreted by the posterior lobe of the pituitary gland,* there are differences according to the animal from which the hormone is obtained. Professor Li has worked on sheep ACTH; the structure of pig ACTH has been only partly determined, but it is known to be chemically different. Since both the pig and the sheep hormone act effectively in human beings, it follows again that less than the whole structure is uniquely necessary.

A last example is the determination by X-ray analysis of the arrangement in space of the atoms forming a substantial fragment of the molecule of Vitamin B₁₂. There has also been sufficient progress towards the determination of the complete molecule to suggest that the structure of this, too, is effectively known.

**Science News* 31, 114-15 (1954).

The most recent part of the story began with the separation of the fragment in question by J. R. Cannon, A. W. Johnson and Sir Alexander Todd in the University Chemical Laboratory, Cambridge. As now known, it contains a total of 127 atoms, of which 67 are atoms other than hydrogen. Like the complete vitamin, it is red in colour, and includes the atom of cobalt which is the most obviously unusual feature of the vitamin. At the time when this chemical separation was effected, Dr. Dorothy Crowfoot Hodgkin and her colleagues, working in the Laboratory of Chemical Crystallography at Oxford University, and J. G. White, working in the Frick Laboratory, Princeton University, were already collaborating in the preliminary study of the complete vitamin by X-ray analysis. From the first stage of this work, it was supposed that the central cobalt atom was surrounded by a planar group of the same form as that which surrounds the iron atom in the haem component of haemoglobin. But there were indications, mainly from the work at Oxford, of a modified arrangement of the surrounding planar group which at the time was unexpected. At this stage, a single selected crystal from the Cambridge fragment became available for analysis. The unexpected central arrangement was confirmed, and a preliminary account published in December of last year. The speed of subsequent progress affords a good illustration of what can be done by X-ray analysis, when supported by full computing facilities. The collaboration between Oxford and Princeton has been continued; the National Bureau of Standards western automatic computer in Los Angeles has been made available with help from the Office of Naval Research and the Office of Ordnance Research; and Professor K. N. Trueblood and R. J. Prosen, of the University of California, have come also into the research with the advantage that they could work directly with the computing staff. The result has been the full determination of all atoms but those of hydrogen in the Cambridge fragment, and the arrival at a chemical structure based essentially on X-ray evidence. 'To be able to write down a chemical structure very largely from purely crystallographic evidence on the arrangement of atoms in space—and the chemical structure of a quite formid-

ably large molecule at that—is for any crystallographer', they comment in their joint publication, 'something of a dream-like situation.'

X-ray analysis of the complete vitamin is less far advanced. But there is provisional agreement between the X-ray collaborators, the Cambridge chemists, and Dr. E. Lester Smith, one of the joint discoverers of vitamin B₁₂, on its probable chemical structure.

The interval between the preliminary publications and those just summarized has been less than eight months. Still more recently, there has been direct confirmation of further features in the structure of the vitamin. This has been obtained by a preliminary analysis of a modified form of the vitamin, containing two chlorine atoms in place of methyl groups. The modified form was prepared at the Glaxo Laboratories by making use of the ability of certain micro-organisms to synthesize chemically-substituted variants of the vitamin when provided with suitable raw materials. The inclusion of extra heavy atoms (here chlorine) is a help in X-ray analysis, and is common practice for that reason. But they are usually put there by chemists. The use of a micro-organism to insert them was thought of, and acted on, when work on the structure of penicillin was proceeding. In that case, analysis of the structure had been completed by the time that the bacteria-modified material was available. The present is therefore the first occasion when the method has been used in analysis.

General accounts of research on the structure of insulin have been given by A. W. Johnson (with references) in *Science Progress*, 43, 81 (1955), and by E. O. P. Thompson (more readably, but without references) in the *Scientific American* (May 1955, p. 36). The work by Professor Hassall is reported from a preliminary review given orally at the British Association. The most recent work on ACTH was released by the University of California. Work on the structure of vitamin B₁₂ is summarized in the following original publications:

Hodgkin, D. Crowfoot *et al.* *Nature*, 176, 325 (1955).

Bonnett, R., *et al.* *Nature*, 176, 328 (1955).

Kamper, M.J., and Hodgkin, D. Crowfoot. *Nature*, 176, 551 (1955).

COMPARATIVE GENETICS AND PATHOLOGY

The use of inherited diseases of animals as a tool in medical research has lately been recognized by the Medical Research Council through the establishment of a research group with this object. The possibilities of work on these lines were illustrated at the British Association meeting by H. Grüneberg, Reader in Genetics at University College, London, with examples from different laboratories. Many close counterparts of human anomalies, he stated, such as incomplete fusion of the vertebrae, hare lip, and cleft palate, had been encountered in laboratory animals, especially the mouse, and other anomalies, not before recognized, had been found quickly when looked for. It was then usually possible to produce a stock in which the anomaly was present in a predictable proportion of the animals, and the development of the anomaly from embryological stages onwards could be studied more systematically than could be done in human populations. In the case of hare lip and cleft palate, the obvious interpretation was that there had been incomplete fusion in the processes which form the upper jaw and nose. When a similar condition was studied in the mouse, it became clear that in some embryos such a connexion was formed, but was later broken again. This had been suspected in some cases of human hare lip, but had not been proved. A second example was a disease, hydrocephalus, in which an obstruction in the pathway of the cerebrospinal fluid in the brain led to an accumulation of fluid. In mice embryos, the abnormality was seen as part of a general skeletal anomaly, of which the first effect was a reduction in the size of all membranous skeletal structures. The pathology of development—of which Grüneberg gave other examples—was, he stated, a substantially new branch of medicine.

As well as early-appearing anomalies, which could be studied in embryos, the comparative method could be applied also to inherited disabilities which tended to appear in adult life. During the last year several distinct kinds of deafness had been found in laboratory mice, although nothing before had been known of them. They had been found at once when looked for systema-

tically, and the anomalies shown later as deafness could be studied before deafness itself developed. Another line of research was on resistance or susceptibility to various infectious diseases which, from very limited data, were generally assumed to be in part under genetic control.

KEEPING WATCH ON VOLCANOES

The use of repeated magnetic surveys for keeping an eye on volcanoes has been discussed by T. Rikitake and I. Yokoyama of the Earthquake Research Institute of Tokyo University, mainly on the strength of observations made on and around the volcano Mihara. The suggestion is that a period of developing activity is accompanied by a rise in the internal temperature of a volcano; that, in volcanoes composed of basaltic rocks, whatever magnetite they contain will then be heated above the transition temperature, the Curie point, above which ferromagnetism disappears; and that such changes may be sufficiently extensive to be detected readily at the surface.

The volcano Mihara rises from the centre of Ooshima Island, about 100 kilometres south of Tokyo, and its rocks have a high content of magnetic material. It had a serious eruption in 1950. During the early and again during the most violent stage of the eruption surveys of the angle of magnetic dip were carried out 'over the volcano'. (One would have liked to know more about conditions of work, but no further description is given, except that the published charts include the results of observations made at two points close to the crater rim.) There was a two months' interval between the surveys. During this time the dip angle had become less at all points. The decrease was largest (30 minutes of arc) in the central part of the island, close to one side of the crater rim, and smaller at greater distance from the centre. Unrelated changes, such as magnetic storms and daily changes, were eliminated by comparison with the records kept at Kakioka Magnetic Observatory, 150 kilometres from the volcano. On the basis that the magnetic changes were caused by heating, the volume of rock heated above the Curie point during the two months between surveys is roughly estimated as a sphere of

2 kilometres radius, with its centre 5 kilometres below the surface.

Between the eruption of 1951 and October 1953, two surveys of dip-angle were made, and also two complete magnetic surveys. In addition, a temporary station for continuous recording was set up at the foot of the volcano. The continuous recordings were of the direction of the horizontal component of the magnetic field (compass direction), and showed a continuous drift away from the volcano. A few observations were made also at a station on the opposite side of the island, where a drift in the reverse direction was found. At the main station there were also 'undulatory oscillations' of a few months which 'seemed to correspond to the heating and cooling associated with the intermittent activities'. In October 1953 there was a further but smaller eruption, and during the following two to three days an additional survey of dip angle was completed. There was again a consistent pattern of change, but on a smaller scale (maximum change 4 minutes of arc) than those which had accompanied the bigger eruption. Finally, there is supporting but less reliable evidence of magnetic changes from an earlier eruption of another volcano, Miyakeshima, this also being composed largely of basaltic rocks.

The extent of the thermal changes thought to have been observed is admitted frankly as a difficulty. Heating during developing activity might be explained, it is suggested, by the injection of high-temperature gas. But no explanation has been offered which would account for cooling at the required rate while activity is diminishing.

From a practical point of view, Rikitake and Yokoyama sum up cautiously that they 'believe that they can presume the internal state of certain active volcanoes to some extent by the magnetic method. If we accumulate observations over a long period we might be able to predict occurrences of eruptions by applying this method. It will be of interest and importance to make similar studies on other volcanoes composed of rocks with high magnetization.'

Rikitake, T., and Yokoyama, I. *Journal of Geophysical Research*, 60, 165 (1955).

INTERNATIONAL GEOPHYSICAL YEAR

A statement on British contributions to the International Geophysical Year, from July 1957 to December 1958, has been issued by the Royal Society. Over forty scientific research stations in the United Kingdom, over thirty stations in the Colonies, and seven research vessels at sea will take part. Warnings of 'special alerts', for example for solar flares and hence for geomagnetic and associated disturbances, will be distributed by the Radio Research Station at Slough to British stations, having been received at Slough from the Central Radio Propagation Laboratory in the United States. An outline of international arrangements for the year was given in *Science News* 35 (pp. 112-14). There have been other announcements of American plans for the launching of small 'not-quite' satellites, from whose descending course through the upper atmosphere information about the density of the air will be obtained; and of British plans for the launching of research rockets, not as part of the international geophysical year, but in time for it. According to an account given by R. L. F. Boyd at the British Association meeting, these British rockets will use a solid fuel as propellant. A new experiment which it is hoped to carry out is the use of mass spectrometers to obtain direct information on the numbers of different ions which are present in different regions in the ionosphere. The following further notes are abstracted from the Royal Society's statement.

Geomagnetic observations, especially of sudden disturbances, will be made at a station on the Antarctic continental land mass, as well as in the Falkland Islands Dependencies and at four stations in Great Britain. Auroral effects, often synchronized with magnetic disturbances, will be co-ordinated from observations in a large section of Europe by J. Paton of Edinburgh University. Aircraft and ships will co-operate in this part of the British programme, and in the Falkland Islands Dependencies records of *aurora australis* will be kept. Radio observations of the ionosphere, also related to magnetic disturbances, will be made at six stations in the United Kingdom and at five stations overseas. Continuous solar observations will be made by the three Royal

Observatories (Greenwich, Edinburgh, and Cape of Good Hope), and by radio methods by the Cavendish Laboratory, Cambridge, and the Jodrell Bank Station of the University of Manchester.

A study will be made of short and long period changes in sea level and of the general circulation of water in the oceans. The Royal Research Ship *Discovery II* and the Fishery Research Vessels *Scotia* and *Ernest Holt*, as well as land stations at Lerwick (Shetland Islands), Freetown (West Africa), Takoradi (Gold Coast), and at South Georgia and Argentine Islands (Falkland Islands Dependencies) will take part in the oceanic section of the programme.

Objectives in the scientific study of the south polar regions will include an investigation of atmospheric flow over this, the largest and coldest land mass of the world. There will also be studies of adjacent ocean currents, and of the geomagnetic and ionospheric characteristics of the region, determinations of gravity, and recordings of earth tremors. The stations of the Falkland Islands Dependencies will participate in research on these lines, and, with the help of the Governor of the Falkland Islands, the Royal Society is to establish a special station in the neighbourhood of Vahsel Bay at which a full programme of geophysical observations will be put in hand. The trans-Antarctic journey under the leadership of V. Fuchs to the Vahsel Bay area will be separately organized but closely associated with the work of the Vahsel Bay Station. An attempt to estimate the total ice-content of Antarctica will be made by the trans-Antarctic expedition. In relation to climatic fluctuation, observations will be made of glaciers in British southern polar territories, and also (which is no less necessary) on high-altitude equatorial glaciers in Africa.

CORRESPONDENCE

EXISTENCE OF THEORETICAL ENTITIES

THE article on 'The Existence of Theoretical Entities' by Bernard Mayo in *Science News* 32 is an expression of views that can only in part be acceptable to many scientists. The criticism raised against it by W. H. Bett in *Science News* 33 does not appear to deal adequately with these; and so, though I had expected that others more competent than I would take this up, I feel it desirable to try to deal with them.

The first point concerns the method of science, which is considered to differ qualitatively from ordinary thought: 'It is grossly misleading to describe the function of theoretical concepts in scientific reasoning in the same terms as the function of common-sense concepts in practical reasoning. The suggestion is either that practical common sense is a sort of elementary or unself-conscious science, or else that science is sophisticated common sense, involving no essential difference of method.' It is not made clear in what follows in what way the use of theoretical entities in science differs from the use of common-sense concepts; nor indeed what common-sense concepts are. In so far as the ideas quoted from Jeffreys and Born are concerned merely with passive recognition of objects this statement may be true; but everyday affairs involve much more than passive recognition; they involve active operation on objects to change them: for example, chairs are made, dogs are trained, and potatoes are grown; and such active operation involves some theoretical understanding, expressed in terms of theoretical concepts. The question as to whether we really theorize about potatoes or chairs in the kitchen obscures or ignores the incorporation of theory in routine, which means that theoretical concepts, though used, are no longer used *consciously*. The mental approach to finding out what is wrong with a motor car or a wireless set is much the same as that to solving the structure of an organic molecule; and the result of the

reasoning in either case is tested practically, by successful working of the car or set, or by the synthesis of the compound. The main difference lies in the material and ideas handled; visible, mechanical, or electrical components on the one hand, or invisible atoms and molecules on the other. There is therefore nothing inconsistent between the quoted statement of John Stuart Mill (which refers to 'proceeding' and not to material handled) and the existence of the Atomic Theory. The formation of theoretical concepts, arising from practical action, precedes by many centuries scientific thought, and even civilization itself: tribal societies have always had concepts of spirits controlling their surroundings, and these were believed to have an objective existence, which we now deny to them. Practical life has not only changed such concepts, but has given rise to many more; and especially since science developed, brought these concepts more into correspondence with objective reality. Science, in fact, is the theoretical expression of practical life developed as an independent activity. As such, its methods, while arising out of those of practice, have been improved considerably, as Mill states: but Mayo has not shown us why we should consider them as totally distinct.

The second point concerns the objective existence of theoretical entities. Mayo has discussed at some length the words 'reality' and 'existence'. If these words have several meanings in popular use, it is no doubt unfortunate for the scientist, but I shall take the statement 'the electron is real' to mean that the concept of the electron, or of electrons as a class, reflects some objective reality in the external world, quite independent of whether I or anyone else has the concept. To say 'the electron exists' is an alternative way of expressing the same thing, with perhaps the additional implication of present time. Mayo considers the concept of 'reality' hopelessly obscure: I must disagree with him, and say that to dispense with the concept will involve us in much greater obscurity. He also considers that the question of whether the electron exists is badly put; that the electron is nothing more than a concept occurring in an appropriate place in an established theory. Now since a single electron as such is invisible, and since

no scientific theory can ever be regarded as incapable of improvement in the future, it is true to say that we cannot speak as confidently of an electron as we can of a chair: nevertheless we can see when an electron strikes a zinc sulphide screen with sufficient energy; we can state confidently that modern theory *demand*s that electrons must form part of a chair, or other object; and we can use electrons to work our wireless or television sets. Can we say that a chair is real, but that it consists of parts that are less real? Or that the visible image on the television screen is caused by theoretical entities which do not exist, or have less reality than the image itself? To deny the reality of electrons seems logically to require the denial of both chair and image, which are not only visible, but can be made by the work of men's hands; and that would be to deny all objective reality, including the existence of scientists, publishers and readers. We may admit that there is much about the electron we do not know; that we may be mistaken about some aspects of it; that we do not know its properties apart from its behaviour (indeed the same applies to everything, and we should not consider the two ideas in opposition). But it seems inescapable to conclude that the concept does correspond to something in the objective world: in other words electrons exist.

R. C. TRAILL

Kew, E.4,
Victoria, Australia

Mr Mayo writes:

Dr Traill complains that it was not 'made clear' in my article how theoretical entities differ from common-sense concepts. To do so was the whole purpose of the article. It presupposed, however, a rudimentary acquaintance with the nature of physical theory, without which one cannot begin to understand the special use of the term 'theoretical' in such expressions as 'theoretical concept' and 'theoretical entity'. Of course we *can* talk about detectives and mechanics 'theorizing'; but if we are to use 'theoretical' in the special sense required for physical theory, it

is an unprofitable and indeed baffling procedure to deny that there is any such special sense, and to talk, as Dr Traill does, about 'theoretical understanding, expressed in terms of theoretical concepts' with reference to chairs, dogs, and potatoes, or to speak of animistic beliefs as 'theoretical'. The only way to make my point more clearly would be to go back behind the assumptions of my article, and to give an explicit account of the relevant parts of the logic of science. It might be possible to do something on these lines in the space of a short article, though hardly in the space given customarily to comments on a letter.

The Editor writes:

I hope that Mr Mayo will proceed with his short article.

HOW WE FORM CONCEPTS

IN his interesting account of Piaget's investigations regarding concept-formation, Dr Mays suggests that what has been discovered about the development of our concepts of space and time constitutes a refutation of what Kant has to say on these matters.

Kant, however, does not appear to be concerned with the age at which children arrive at a clear concept of space or time, or with the psychological processes leading up to this achievement. Nor is there any reason to suppose that he would be disturbed to discover that 'the abstract concept of time would seem to arise at a relatively late stage in the child's thought'. The problem with which Kant is concerned is a quite different one; it is to explain the possibility of there being knowledge, at any stage in the individual's development, of absolutely general truths about the structure of the universe. That there is such knowledge Kant believed to be beyond question, the propositions of arithmetic and geometry being the most familiar examples. Kant's proposed solution of this problem consists, very roughly, in saying that the human mind has a certain inherent way of arranging the non-spatial and non-temporal data which it receives through the senses; that the ways in which such data are spatially and tem-

porally organized are consistent with the principles of geometry and arithmetic; and that since all the data we shall ever receive will be arranged in the same general ways the propositions of geometry and arithmetic will hold good for everything which we shall ever experience. When Kant speaks of the pure intuition of space and time as being prior to experience he must, from what he says elsewhere, be interpreted as meaning 'logically prior'.

Kant's theory, although it commits him to the view that prior to experience the mind has a tendency or potentiality to deal with its data in certain ways, does not commit him to the view that the child begins life with a fully developed concept of time or space, or that it develops either concept at any particular time. And just as his theory involves no psychological hypotheses it need not commit its author to any particular set of mathematical principles. For example, the fact that Kant and most of his contemporaries never thought to question the truth of Euclidean geometry as the geometry of physical space cannot be used against Kant's doctrine. It is no part of this doctrine that we have an immediate knowledge of mathematics; mathematical knowledge must be abstracted from those products of our mental synthesis of which we become conscious, and we may make many mistakes in this process of abstraction. But when the process has been correctly carried out it is Kant's contention that we have knowledge which applies to all that we shall ever experience.

PETER REMNANT

University of British Columbia.

Dr Mays writes:

I should point out that my article on Piaget was not primarily concerned with examining Kant's views or refuting them. I only mentioned Kant and the Kantians in order to contrast their approach to such problems with the experimental approach of Piaget. Kant's views are notoriously obscure and have been subject to varied interpretations. Commentators disagree among themselves as to whether, e.g., space is for him temporally prior

to experience or logically prior (cf. Garnett, *The Kantian Philosophy of Space*, Chap. VII. Columbia Univ. Press, 1939). In view of this I cannot share Mr. Remnant's certainty as to the exact nature of Kant's views on space and time. Whether Kant was not committed to the view that the child begins life with the fully developed concepts of space and time is a moot point. Kant like most of his contemporaries probably never contemplated the possibility that things might be otherwise. As far as his conceptual equipment was concerned the child was virtually regarded as a little adult. If there were any differences in the nature of his thoughts and perceptions, they were rather of degree than quality.

The essential point is that some philosophers have believed that there are certain absolutely general truths grasped by some special faculty of intellectual intuition, and to which all empirical facts have to conform. Only when this assumption is made can it be said that Kant's account of space and time involves no psychological hypotheses. If we reject such a faculty, we have no *a priori* guarantee that there are such general truths which impose their order on all our thoughts and perceptions—that everything we will ever perceive or think must be perceived or thought about in this way. Further, the truths of logic and mathematics occupy nowadays a less exalted position than they did in Kant's time. The formalist view, e.g., regards the truths of logic and mathematics as conventions set up by definition; and necessary and certain only because they are tautologies.

The attempt to show that Kant's work is consistent with non-Euclidean geometries seems to be a case of being wiser after the event. A feature of some philosophical systems is that when suitably amended they can be made to accommodate the most inconvenient of facts.

MICRONS

I TRUST it is not too late to enter a mild protest (which some may think pedantic) about a matter of definition. J. E. Hotchin in his article 'Recent Work on Bacteriophage' (*Science News* 33, p. 92)

makes the statement 'a micron (μ) is 1/1000 of one millimetre'. This is, of course, factually correct, but nevertheless makes me squirm as much as when I first heard it during one of my early medical student lectures.

The symbol μ is widely used to indicate, properly, 1/1,000,000, as in the composite symbols μF and μH , and I feel that *Science News* ought not to perpetuate the improper definition; i.e. that it, μ , indicates the 1/1000 part of anything.

F. B. SINGLETON

Trevor,
Wrexham.

The Editor writes:

Lecturers in medical schools may note. But for the purpose of explanation (as opposed to definition) one thousandth part of a millimetre conveys a more ready impression of order of length than does one millionth part of a metre.

SOME BOOKS RECEIVED

THE FORESEEABLE FUTURE by Sir George Thomson, F.R.S.
(Cambridge University Press, 1955), pp. 166, 10s 6d.

A scientific prophet may be expected to provide good reading, enough of new ideas to titivate, and enough of logic to satisfy. For good measure, he should have done enough himself to earn freedom, and should not before have cavorted. Sir George Thomson passes on all counts, the first as easily as the rest. Among his more serious points, he thinks that the theoretical limit to the strength of materials may be more closely approached, forecasts a catching up of biological knowledge on physical, and emphasizes the scope offered by computing machines for handling problems not before amenable to solution. For titivation, he wants monkeys bred and trained for the duller jobs which yet are not suited to machines. As applied logic, he presents an apparently sound case for the large nuclear-powered submarine for fast cargo transport. And he has enough of well-turned phrases to encourage watchfulness for the next. Altogether a book to be enjoyed, acclaimed or criticized according to taste, and proved right or wrong by the future.

RADIO ASTRONOMY by J. L. Pawsey and R. N. Bracewell
(Oxford University Press, Geoffrey Cumberlege, 1955), pp. 361, 55s.

Useful as it should be, this may well be the last, as well as the first, book of its kind—a full-length review of radio astronomy. Like Dr Lovell and Dr Clegg in a short introductory volume published three years ago, the authors cover all sides of the radio astronomer's work, with merely such differences in emphasis as come from personal or laboratory interest. Apart from equipment and methods, together with explanations from general astronomy and physics, this involves them in solar and cosmic radio waves, emission by interstellar hydrogen, and the study of the moon and meteors by radio methods. The authors are well qualified by experience and contacts with others, and it is good that their book should have been written at this time. But the final conclusion is their own: with recent developments in equipment, 'radio astronomers will become involved in the analysis of much larger amounts of data and will be brought in close contact with the results

Some Books Received

125

and methods of optical astronomy'. In short, radio astronomy is a technique, not a subject.

THE SUN AND ITS INFLUENCE by M. A. Ellison (London: Routledge and Kegan Paul Ltd, 1955), pp. 235, 21s.

Dr Ellison has done original work on solar flares, and is also a gifted expositor. His book is probably the best account for the general reader (preferably with some knowledge of physics) of research on the sun and its geophysical effects on the earth—which for this purpose are not taken to include climatic cycles.

SOLAR ENERGY RESEARCH edited by Farrington Daniels and John A. Duffie (London: Thames and Hudson, 1955), pp. 290, 30s.

This book is based on a symposium on the utilization of solar energy held in Madison, Wisconsin, in 1953. Except for Russia and Israel, it is reasonably representative of current effort from the physical side. But individual contributions vary greatly in scale and manner, and range more widely in subject (e.g. photosynthesis, and the conversion of agricultural products to power) than is satisfactory in book form. Useful rather as a starting point for reading (one of its stated objects) than as itself a coherent introduction.

THE ORIGIN OF THE VERTEBRATES by N. J. Berrill, F.R.S.
(Oxford University Press, Geoffrey Cumberlege, 1955), pp. 257, 25s.

A discussion of the origin of the vertebrates can be made as dull as its author pleases. Professor Berrill, to whom sea squirts began to talk back and 'try to tell their story', has written imaginatively in the best sense. Without claiming that proof has been obtained, or can be, he suggests the following outline: the evolution in small sessile ascidians of non-feeding tadpole larvae; with an increase in their size, the colonizing through shallow inland seas of slow-flowing rivers as feeding territories; and, after perhaps 150 million years, the uplifting of the land, an increase in rate of stream flow, and the moment when it became necessary to go on or go back—the vertebrate ancestors to go on; Amphioxus (often tentatively near-ancestral) to go back. His interpretation deserves to be read in his own way; he claims for it that it 'makes use of the greatest number of apparently relevant facts and at the same time leaves the smallest number unaccounted for'.

THE MECHANISM OF EVOLUTION by W. H. Dowdeswell
(London: Heinemann, 1955), pp. 99, 6s.

Mr Dowdeswell's short monograph, published in the 'Scholarship Series in Biology', consists mainly of two chapters on variation and natural selection as field naturalists and geneticists are now learning to study them. To these is added some advice on the planning of experiments, which could increase the value of work done by schools.

THEORY OF GAMES AS A TOOL FOR THE MORAL PHILOSOPHER by Professor R. B. Braithwaite (Cambridge University Press), 1955, pp. 76, 6s.

An inaugural lecture by Mr Braithwaite, whether as Knightsbridge Professor of Moral Philosophy at Cambridge or otherwise, might be looked for to provide the unusual—and does. It deals with the difficulty of two neighbours, Luke who likes to play classical music for an hour at a time, and Matthew to trumpet improvised jazz, necessarily at the same time of day (if at all); and applies to their problem the lecturer's own recent discovery of a relation between the geometry of the parabola and the logic of the 'general collaboration situation'. He arrives at a definite answer.

NEW METHODS IN ANALYTICAL CHEMISTRY by Dr Ronald Belcher and Dr Cecil L. Wilson (London: Chapman and Hall, 1955), pp. 287, 30s.

This is not the book that some might expect from its title: it is a collection of 'new' methods employing the classical techniques of chemical analysis; not one about new techniques. Selection has been partly personal. Perhaps for this reason, the impression from sampling is that—apart from its obvious use for reference—most with a present or past interest in the subject should find something in the book to appeal to them.

AN ILLUSTRATED HISTORY OF SCIENCE by Dr F. Sherwood Taylor, illustrated by A. R. Thomson, R.A. (London: Heinemann, 1955), pp. 178, 120 figs., 25s.

The 1952 Christmas lectures of the Royal Institution were on the historic experiments of science; in the author's words, they amounted to a short history, written round the experiments demonstrated.

Mr. Thomson's pictures substitute one set of visual effects for another. Most are good, and some close to brilliant. But the pictures are as much for an adult as for the traditionally 'juvenile auditory' of the lectures; and the question remains, can an artist's impressions be history, even when backed by an appendix of sources used?

BRISTOL AND ITS ADJOINING COUNTIES edited by C. M. MacInnes and W. F. Whittard (Bristol: published for the British Association by the Local Executive Committee, 1955), pp. 335, 30s.

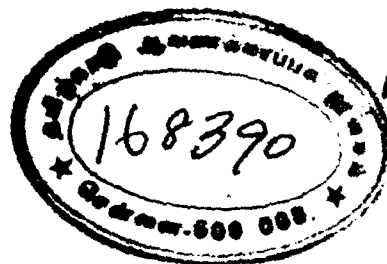
As to two thirds, this could have carried fairly the sub-title 'scientific survey', given traditionally to the annual volumes published at British Association meetings; the rest consists of historical and general-interest material. Like other volumes in the series, it should be in geographical and reference libraries.

SURGERY OF THE HEART AND THORACIC BLOOD VESSELS
edited by N. R. Barrett (*British Medical Bulletin*, Vol. II, No. 3: London, British Council, 1955), pp. 242, 15s.

A symposium on the recently developed methods of heart surgery is introduced by Sir Russell Brock with the reassurance that 'far from being a delicate structure . . . the heart is a tough organ'. Contributions are of the high standard now expected in this series.

SCIENTIFIC RESEARCH IN BRITISH UNIVERSITIES 1954-55
(London: H.M.S.O., 1955), pp. 572, 12s. 6d.

This useful reference book is published (with paper back) for the Department of Scientific and Industrial Research. Arranged by universities and departments, it includes also name and subject indices. Within its obvious limitations, it is being made progressively more complete.



ABOUT OUR CONTRIBUTORS

JOAN BANUS went up to Cambridge in 1941, where she read chemistry and did research under Professor H. J. Emeléus. She is now in the Chemistry Department at University College, London, and still working on fluorocarbon derivatives. In between she has spent 18 months in the University of Southern California, working on fluoroboron compounds, and 6 months at the Ohio State University, Columbus, Ohio, in the field of organic fluorine chemistry.

G. H. DURY is Lecturer in Geography at Birkbeck College (University of London). He graduated in 1937, receiving the degrees of M.A. and Ph.D. in 1944 and 1951. During the war, he served in R.A.F. Intelligence, and on his release resumed research on geomorphological problems, including meandering valleys on which he has previously written in *Science News* (SN33, 65).

G. R. HERVEY graduated at Cambridge and King's College Hospital, London. He spent two years in the Navy, mainly working on problems connected with survival at sea, and is now at the Medical Research Council Department of Experimental Medicine, at Cambridge.

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2

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