NATURE

"To the solid ground
Of nature trusts the Mind that builds for aye."—Wordsworth

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NATURE

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THE FOOD AND AGRICULTURE ORGANISATION

THE work of the Food and Agriculture Organisation of the United Nations is so closely linked in some fields with that of the United Nations Relief and Rehabilitation Administration that the confidence in which this first of the functional organizations of the United Nations was shaped at Hot Springs in May 1943 has naturally been somewhat undermined by the chequered career of the Relief and Rehabilitation Administration and the tardiness of the United States, for example, in voting essential supplies. None the less, the firm pledges which Britain has given to the latter organization and the successful issue of the preparatory conference, which in November led to the constitution of a United Nations Educational, Scientific and Cultural Organisation, are among the pointers which warrant the hope that in the Food and Agriculture Organisation there is another functional association through which science may make an increasingly effective and important contribution to human welfare.

The appointment of Sir John Orr as first director-general of the new Food and Agriculture Organisation will be widely and warmly welcomed among men of science. It is in itself a conspicuous example of the fitness of a man of science for high administrative office ; and it also emphasizes the vital importance of scientific and technical factors in this field of agriculture, nutrition and food supply. The association of Sir John Orr with such a post suggests that in the policy of the Food and Agriculture Organisation the findings of nutritional science and human needs will be kept well in the forefront.

The United Nations Information Organisation has done well, therefore, to issue an account of the new organization*. The Charter of the Food and Agriculture Organisation was signed on October 16 by thirty nations at Quebec and a few days later was signed by the Soviet delegation, and the pamphlet gives a brief account of the work of the Interim Commission from the close of the Hot Springs Conference to the opening of the Quebec Conference on October 16 last. The Interim Commission consisted of representatives of the forty-four United and Associated Nations under Mr. L. B. Pearson as chairman, and was working at Washington six weeks after the close of the Hot Springs Conference. It has recruited a small international secretariat, and prominent scientific men and economists from many countries have given freely of their time and expert knowledge. For the first few months, the Commission concentrated on drafting a constitution and on preparing the First Report to Governments on the scope and functions of the Organisation. In this work it was aided by two panels of experts, one on the scientific aspects of food and agriculture and the other on the economic.

The Declaration drafted by the Interim Commission pledges the Governments adhering to the Organisation "to work separately and together to the end that

* Towards a World of Plenty. (United Nations Information Organisation.) (London : H.M. Stationery Office, 1945.) 4*d*. net.

want and the fear of want shall be progressively abolished", and for this purpose to take all measures within their power "to raise the level of nutrition and the standard of living of the peoples under their jurisdiction" ; and "to improve the efficiency of agricultural production and distribution". In essence, the Food and Agriculture Organisation will attempt to marry nutrition with agriculture. On one side it is a direct outcome of the advances in the knowledge of nutrition which has characterized the last two decades. Equally, however, the Hot Springs Conference approached the problem of agricultural production in the light of the human and universal need of freedom from want, and was therefore able to bring the whole economic problem under review and to place the individual needs of producers and consumers of particular commodities in their proper perspective.

This was probably the greatest achievement of the Hot Springs Conference, and the reports prepared for the Quebec Conference further attest the objective approach which the Organisation is making towards the whole question and the importance of the technical and scientific contribution, from the agricultural scientific worker, the economist and the expert in nutrition. A Committee on Agricultural Production, of which first Prof. J. A. Scott Watson, and later Mr. Peters of the Netherlands, was chairman, has prepared a report on the major problems facing the farmers of the world and has made recommendations for concrete programmes of work to be undertaken by the Organisation. Under Dr. F. Boudreau, a Committee on Nutrition and Food Management is reporting on the way experience gained in war can be utilized for purposes of peace, while a Committee on Forestry, under Dr. Graves, has presented a report on world forestry and primary forest problems, and is now considering the more immediate world problems. A Committee on Fisheries under Dr. Finn, the Canadian Deputy-Minister of Fisheries, as chairman, is reporting on the fisheries of the world, and a fifth technical committee, on statistics, under Dr. Becker, is considering ways in which statistical information about food, agriculture, forestry, and fisheries can be improved.

The scientific and technical contribution from these committees is one sound reason for appointing a scientific man of outstanding administrative ability to take charge of the whole organization ; and Sir John Orr's appointment may be taken as evidence of the desire of the members to see that the Organisation sets itself severely practical tasks, and that in particular it serves as an adequate and effective source of technical and scientific advice to whatever political body may be authorized by the United Nations to take executive action. It is of the first importance that the Food and Agriculture Organisation should secure a position of authority as an impartial source of information and advice in agricultural and nutritional matters, and the presence of a leading man of science as its director-general affords a guarantee that this primary objective will be steadfastly pursued.

There can be no question as to the value of the work which the new Organisation could do in spreading knowledge of the best agricultural practice and technique, and in acting as a clearing house of information, both practical and statistical. The backward condition of agriculture in so many parts of the world indicates sufficiently the wide field for improvement here, and if the new Organisation merely provided a meeting ground for the experts and a forum for the discussion of matters of common concern, it might still be invaluable in furthering the aims of the Hot Springs Conference, irrespective of the progress made in organizing economic relations internationally in more general fields. But besides this, the Organisation can undertake research and lend or negotiate the services of experts for the improvement of agriculture and nutrition. On request, it may review projects for the development of agriculture involving requests for international loans, and it may also undertake the drafting of conventions for the control of agricultural pests and diseases, the establishment of grades and standards, statistical measurements and nomenclature.

It is not belittling the contribution which the Food and Agriculture Organisation could make to point out that, where development schemes are concerned, no major results can be obtained without providing machinery and equipment on a large scale. In countries such as India and many parts of Africa, the problem becomes one of providing the means for development, in order that the efficiency of production may be raised from its primitive level. Similarly, on the side of nutrition, the elimination of malnutrition and the raising of standards of living mean the alleviation of poverty and provision of purchasing power. Here the Organisation clearly cannot take the practical steps of subsidizing food consumption or negotiating agreements with food producers.

The economic measures by which the increased production and purchasing power are to be secured in any particular country are matters for the Government of that country and not for the new Organisation. The value of its advice and the pressure it is able to exert will be all the greater if it can work out scales of priorities for meeting the world's hunger and want which take into account the relative and diverging interests of producers and consumers. If the Organisation, in fact, did no more than find out the size of the world's food requirements and the potentialities of its agricultural production, and then try to bring the two halves of the picture together, the world would still have ample reason to be grateful to President Roosevelt for his initiative in calling the Hot Springs Conference.

To achieve its aims, the director-general will need to see that the Organisation deals only in realities ; and no efforts should be spared to achieve increased efficiency, not only in the production of the world's food supplies but also in their distribution. It is understood that before returning to England to settle up his affairs, the director-general instructed the officials of the Organisation to prepare a world-food picture, indicating the world-market situation for the next twelve months of each important commodity and the nutritional situation in every country.

In this formidable task the Combined Food Board and the United States Department of Agriculture's Office of Foreign Agricultural Relations are assisting.

On the statistical side the new Organisation will develop and extend the useful work that was done before the War by the International Institute of Agriculture at Rome, and should increasingly provide in this field the factual basis for policy and practice which Lord Woolton, in his recent presidential address to the Royal Statistical Society, noted is at present lacking. But Sir John Orr has already indicated that the main problem of the next few years, as he sees it, is to harness the agricultural productive capacity of the Western world, increased during the War, to the needs of an Eastern world which is starving but cannot pay for imports of food ; and while the solution of that economic problem is a responsibility for higher levels, the Food and Agriculture Organisation, as a fact-finding body, can do much to demonstrate that its solution is possible. Here again, with a scientific man at its head, the Organisation may well be expected to lend adequate support to such methods of attack as the selection in poor countries of demonstration areas and in them to develop all resources for the improvement of nutrition by education, special food distribution schemes and increasing the production of foods, particularly protective foods, such as milk, vegetables, fruit, fish and eggs.

No efforts should be spared to achieve increased efficiency, not only in the production of world food supplies, including the development of fresh sources of supply—as, for example, the exploration of hitherto untouched fishing grounds—but also in their distribution. The importance of the latter question was well emphasized in the Sectional Report (III) of the Hot Springs Conference, and the Committee on Fisheries now at work may well be relied upon to report on the way in which fisheries could be opened up in other waters, as has in fact been done during the War, to supplement those in the North Atlantic and North Pacific Oceans which at present provide ninety per cent of the world's supply. Particularly while the shortage of livestock persists, and also in countries where livestock industries cannot easily be developed, such steps might provide a valuable addition to diets deficient in animal protein.

Important as is this process of fact-finding, research and demonstration of technique and practice, the scientific and technical aspects of the work of the Food and Agriculture Organisation cannot stand by themselves. Even from a wider point of view, the important contribution to world co-operation, order and understanding which such functional organizations can make does depend to some extent on the facilities and organization at their disposal for giving effect to their findings. For that reason it is important to consider the exact position of the Food and Agriculture Organisation in the United Nations Organisation as a whole and, in particular, its relation to the Economic and Social Council in regard to the translation of policy and findings into action.

For the same reason, while the Food and Agriculture Organisation has commenced by calling on the help of the Combined Food Board, it should not be forgotten that there is evidence of a tendency to terminate as quickly as possible the agencies for collaboration set up during the War. The work of the Food and Agriculture Organisation will undoubtedly be handicapped by the recent decision to abolish the Anglo-American Middle East Supply Centre. That decision was severely criticized in the course of a recent debate in the House of Lords, and Lord Pethick Lawrence's reply to Lord Altrincham did nothing to allay the misgivings as to the termination of the valuable technical and scientific activities of the Centre. The discontinuance of these activities will undoubtedly be a handicap to the Food and Agriculture Organisation in dealing with the problems of the Middle East. The contraction of the Anglo-American Middle East Supply Centre, from a body capable of developing into a wider international regional organization, into a national one cannot but be a setback to international social and economic collaboration. The Middle East Office set up by the British Government to maintain close personal liaison with its economic and social representatives of the Middle East Governments in Teheran and to work in harmony with the Middle East States can never provide the same effective instrument which the Food and Agriculture Organisation would have found inherent in the Middle East Supply Centre.

The Food and Agriculture Organisation is bound also to be handicapped by the increase in suspicion and distrust which has marked the international situation during the last six months. An important result of the Organisation's work should indeed be the dispelling of such distrust, and the growth of co-operation and mutual understanding on the political plane through the demonstration of the value and practicability of collaboration in the attack on common problems and building up a spirit of friendship and comradeship, the importance of which was so well emphasized in the recent House of Lords debate on the international situation. In that sense the work of the Food and Agriculture Organisation is part of that larger task of re-establishing full freedom of communication and intercourse, and building up that sense of common rather than divergent interests which must form the firm basis of the United Nations or any other world organization.

We may well look, therefore, to the Food and Agriculture Organisation as one of the means for restoring understanding and for promoting the consideration of technical and economic problems from a scientific or professional point of view, free from political bias. To the extent to which it is able to do this and to put politics in the right position of serving and not, as Mr. Bevin indicated in speaking on European relief on October 26, frustrating human welfare, the new Organisation should also promote the cause of world order and peace by dispelling suspicion and enlarging the area of understanding. Clearly, too, if it is to mean anything, the investigations and advice of the Organisation must have important bearings on the formulation of agricultural policy in every country, and the importance of an adequate understanding of the new Organisation and its potentialities is not easily overstressed.

Not all the changing factors of the last two and a half years have, however, been adverse. The Relief and Rehabilitation Administration has done more solid work, in spite of limitations and difficulties, than is often recognized, in dealing with the immediate problems of which even the long-range plans of the Food and Agriculture Organisation must take account. The United Nations Organisation has come into being, and despite changes in the whole political outlook since the San Francisco Conference, the Preparatory Commission has settled to its work with surprising smoothness in London in readiness for the first full meeting of the Organisation in January. More important still, the widespread concern with the present position in Europe, and which has sometimes issued in unreasonable criticism of the activities of the United Nations Relief and Rehabilitation Administration itself, represents a consciousness of the real issues which may well prove one of the greatest sources of strength to both that Administration and the new Food and Agriculture Organisation.

The concern at the threat of famine to Europe manifested in several debates in Parliament has only been strengthened by such statements as that issued by the Ministry of Information dealing with a combined conference in Washington on problems of food supplies and distribution last April, before the close of the War in Europe, and still more by the United States report on Relief and Rehabilitation Needs of Europe and the meeting of the Relief and Rehabilitation Administration Council in London. In a debate on November 16, Mr. Noel Baker, the Minister of State, gave details illustrating the scale of the work of the United Nations Relief and Rehabilitation Administration and the speed with which its organization has responded to varied and unexpected requests ; but his speech did nothing to allay the anxiety expressed in the debate that distress in Europe was overtaking the means designed to cope with it and was in danger of reaching an intensity beyond control.

The apprehension of a European breakdown is widespread, and references to it in that debate were the more emphatic because many of the speakers had lately returned from visits to Europe. In an earlier debate, on October 26, Miss Rathbone quoted Sir John Orr's opinion that "in the coming winter in Europe more will die from lack of food and shelter than were killed in the whole five years of the War" ; and Sir Benjamin Smith, the Minister of Food, in urging that the supply of additional food for Europe on a scale sufficient to effect any widespread improvement in the situation must be organized on an international basis, had said that the world food-supply prospects for the coming year are seriously disquieting and that in almost all the main foods we are facing deficiencies on the world balance sheet for the next twelve months. When we add to this the limitations placed on the United Nations Relief and Rehabilitation Administration's activities—its statutes forbid it from offering relief to Germany and it is not operating in the invaded countries of western and north-western Europe—and that although the Administration has now reasonable assurance that it will be able to carry on its work until the end of next year, delay in voting supply has already deprived it of the opportunity of purchasing needed supplies, there can be no room for doubt as to the desperate seriousness of the situation and that we are still threatened with the victory of destruction.

Unless, indeed, we can avert from Europe—and that must include Germany—the twin menace of epidemic disease and social chaos, the Food and Agriculture Organisation will have no opportunity of functioning effectively. It is not easy to convince public opinion that everything possible has been done to reduce the danger of disease and social upheaval to a minimum. That is one reason why Sir Arthur Salter's request for the publication of a White Paper which would help to form sound judgment by giving a clear statement both of the needs of Europe and of the resources available to meet them is thoroughly sound.

Such a statement, to the extent that it would enable opinion to be formed on the basis of incontrovertible facts, should be specially welcome to scientific workers, who in this field have no small responsibility in forming and guiding public opinion, and above all in emphasizing the physical, mental and psychological consequences of failure to pay adequate attention to the nutritional requirements and of allowing a state of starvation or semi-starvation to intensify and spread in Germany and Europe. But measures to mitigate the existing conditions will not by themselves be entirely adequate. Beyond that, as the Foreign Secretary has emphasized, we have to eliminate the paralysing effect of fear of a revival of the German menace. That fear is still the greatest obstacle to confidence and co-operation, and to dispel and eliminate it will be one of the greatest tasks of the United Nations and all its functional organizations. The immediate need is to prevent the onslaught of famine and economic chaos. While that is being done, the Food and Agriculture Organisation must be formulating its plans for long-term developments ; and with its practical objectives, it may well play an important part in stimulating that international co-operation which is essential for the well-being of mankind.

THE PLEISTOCENE PERIOD

The Pleistocene Period

Its Climate, Chronology and Faunal Successions. By Dr. Frederick E. Zeuner. (Ray Society Volume 130, for the two years 1942 and 1943.) Pp. xii+322. (London : Bernard Quaritch, Ltd., 1945.) 42*s*.

THERE must be many students who, like myself, being interested in some geophysical problem of the Pleistocene period and not satisfied with the conclusions given in the text-books, have turned to the original papers to find exactly the evidence on which those conclusions were based. Anyone who has had that experience knows how difficult it is to co-ordinate the original papers : the evidence is partly geological, partly biological, partly cultural ; the nomenclature is varied and often of local significance ; the evidence is fragmentary, no one geological section giving the whole history, so that exposures in one region have to be matched against those of another, giving play to opinions rather than facts. Moraines in one place have to be co-ordinated with river terraces elsewhere, and these again with raised beaches far away ; and one has always to consider the possibility of tectonic changes of the surface layers. It is a great example of not being able to see the wood for the trees, and one has longed for a clear map of the 'woods' with the 'trees' marked in their proper places and each kind of 'tree' labelled with a name which does not change from place to place.

The great merit of Dr. Zeuner's book is that it does just that, and I for one must admit that when I had finished reading it I had a clearer picture of the sequence of events during the Pleistocene period and of the evidence on which that picture is based than I was ever able to get from all the original papers of which I have read so many. The book is easy to read for such a complicated subject, is on the whole well arranged and what is more important it gives the evidence largely direct from the original sources. The bibliography is full (but why is it subdivided according to chapters, which makes it difficult to find an item and involves much cross-referencing ?) and the index is adequate. It is a book that every student of the Pleistocene will enjoy reading, if for no other reason than that for once both 'woods' and 'trees' can be seen in their true perspective.

Having said so much in praise of the book, it must not be assumed that I agree with all Dr. Zeuner's conclusions. Neither Dr. Zeuner nor anyone else has said the last word on the Pleistocene ; and the main problem of all, the causes of the great changes in climate, is still open to discussion. Dr. Zeuner has adopted the radiation theory, in which the changes in climate are ascribed to the inequalities and perturbations of the earth's orbit ; in fact, it may truthfully be said that the whole book is written around this theory, and it is because he has such a clear aim (whether it be true or not) that he has been so successful in giving a connected picture. It must be admitted that he brings a great deal of evidence to support his contention, and I can quite understand that it will carry conviction to many readers. This is not the place to discuss the evidence in detail, but I cannot refrain from saying that the supporters of the radiation theory would have been in a stronger position if it had been found that the first three glaciations had each two peaks and the last glaciation three peaks before Milankovitch had produced his radiation curve and not after. If the evidence for the radiation theory marshalled by Dr. Zeuner stood alone, it would be difficult not to accept it ; but it does not stand alone ; there are many questions of physics involved, and these, as I have tried to show elsewhere, give answers which are completely opposed to the theory. On one hand, we have the similarity (more or less) of two curves, and on the other certain objective facts of radiation and energy ; if the latter cannot be reconciled with the theory, then the former means nothing.

As I have already mentioned, one of the great difficulties in the study of the Pleistocene has been the want of a uniform nomenclature, and this is particularly the case in the designation of the successive glacial phases. Having reached the conclusion, by aid of the radiation diagram, that there have been four main glaciations during the Pleistocene, Dr. Zeuner naturally wished to give them suitable names. When, forty years ago, Penck and Brückner were faced with the same problem in the case of the four glaciations they had discovered in the Alps, they reached a very practical solution : they chose for each glaciation a short name taken from a region where evidence of that glaciation was present ; the initial letter of each name was alphabetically in the same order as the series of glaciations starting from the earliest : thus we have Günz, Mindel, Riss, Würm. This committed them in no way to the number of glaciations : if new ones were discovered, new names in the same alphabetical series could be chosen to designate the new glaciation ; there are plenty of vacant spaces. Thus one had a flexible system of names, each short and its initial letter giving its position in the series without excluding possible future additions. The abbreviations G. M. R. W. are concise and unmistakable. For the interglacials Penck and Brückner simply stated the two glaciations between which they fell, thus giving : Günz–Mindel Interglacial, Mindel–Riss Interglacial and Riss–Würm Interglacial, which were abbreviated to G–M, M–R, and R–W, the form of which shows at once that they refer to interglacials.

In choosing his new names, Zeuner has departed entirely from this practical system and has designated his four glaciations Last, Penultimate, Antepenultimate, and Early ; while he designates the three interglacials by the same adjectives. The glaciations are abbreviated to LGl, PGl, ApGl and EGl respectively, and the interglacials to LIgl, PIgl, and ApIgl. With regard to the names of the glaciations : they are not short (Antepenultimate Glaciation is particularly awkward) ; they are not flexible (but by aid of the radiation diagram Zeuner is convinced that no further glaciation will be discovered) and the introduction of the purely English words 'Last' and 'Early' makes the series unsuitable for international use. The abbreviations are particularly unsatisfactory : they are long and there is nothing to show at a glance which are the abbreviations of glaciations and which of interglacials ; for example, ApIgl and ApGl, the former being an interglacial and the latter a glacial ; they should be compared with G–M and M. Considering that Zeuner is satisfied that Penck's four glaciations are identical with his own, why should he not have adopted their names ? They are known to everyone who has read the most elementary text-books on the Pleistocene and they are suitable for use in every language, to say nothing about giving honour where honour is due.

These blemishes, though not small, do not rob the book of its value and I am convinced that many well-read students of the Pleistocene will enjoy, as I did, this bird's-eye view of the most interesting of all geological periods. G. C. SIMPSON.

SELF-SUFFICIENT BIOLOGY

The Directiveness of Organic Activities

By Dr. E. S. Russell. Pp. viii+196. (Cambridge : At the University Press, 1945.) 8*s*. 6*d*. net.

DR. RUSSELL'S book is concerned to establish "the conclusion that directiveness and creativeness are fundamental characteristics of life, shared by no inorganic system ; that they are not to be explained in terms of mechanism or of purpose ; that human directiveness or purposiveness in thought and action are a specialised development of the directiveness and creativeness inherent in life" (p. 178). Its purpose is primarily philosophical, to develop a mode of approach to the problems of biology. However, its method, perhaps wisely, is not dialectical; instead of presenting a close train of argument, Dr. Russell indicates, by descriptions of actual examples, what he means by "directiveness". Look, he says, at this regenerating flatworm or developing egg; naïvely beheld, they cannot but seem to strive towards a well-recognized completeness, which is their goal. Consider again the ability of rats to choose, from an array of purified substances, just those which together make up a perfect diet; or the fact that a rabbit, which reacts to a loss of blood by rapidly making more, and to the transfusion of extra blood by getting rid of the excess, nevertheless does neither of these when the loss is rapidly followed by a transfusion; is it not clear, Dr. Russell asks, that it is the need of the organism, rather than any mere physico-chemical stimulus, which determines the animal's behaviour?

The examples are not always convincing. For example, rats may, as is asserted, be able to pick out for themselves a perfect diet, though the matter is still, I believe, under discussion; but there is no doubt that many other animals cannot. Again, it is difficult to see why, on any reasonable hypothesis, a rabbit should show any marked reaction to an experimental series of bleedings and transfusion which end by restoring the *status quo ante*. But undoubtedly, Dr. Russell can adduce many remarkable biological phenomena which, as he puts it, "are directive towards ends of self-maintenance, development or reproduction". It is difficult not to admit some degree of astonishment at the performance of the minute worm *Microstoma*, which eats *Hydra*, digests all but the sting-cells (nematocysts), and from these selects the varieties with the greatest offensive power (rejecting the other sorts through the mouth), and finally arranges to shuffle these captured weapons through the thickness of its body wall until they reach the surface, where their captor can use them for its own purposes. Such things, to use a cliché, would seem to demand an explanation.

The most unsatisfactory feature of Dr. Russell's book is that he makes no attempt to provide an explanation in other than self-consciously "biological" terms, or even to discuss the explanations which most biologists nowadays advance. "As a philosophy materialism is merely absurd," he asserts, "why then base biology upon it" (p. 2). "For such understanding we require a free biology, with laws and concepts of its own, independent of those of the physical sciences, based upon an objective study of the directive activities of organic agents, unrestricted by the hampering mechanistic hypothesis which is at the back of the causal-analytical method" (p. 4). It may be granted that, whatever a "free biology" may be, specifically biological laws may be quite useful tools of thought. Even the application of straightforward teleology is a quick rule of thumb which often works. And biologists, still without reaching the domain of physics, can rely on a much subtler form of explanation, the appeal to natural selection and the gene.

Russell scarcely mentions either of these fundamentals of biological thought. His only reference to the gene is to classify studies on "the presumed action of genes on development" among the "analytical and disintegrative" physico-chemical investigations which miss the point of "the organism as a living, developing, reproducing whole" (p. 2). This is odd; not only because he allows (p. 158) that those other, one would have thought equally mechanistic, agents, evocators and growth-hormones, do contribute to "a biological account"; but even more because the modern view of an organism in terms of its genes would seem much more closely akin, philosophically, to his own ideas than was the usual biological picture of, say, thirty years ago. In his summing up, Russell applies to the living organism a saying of Spinoza's: "The effort by which each thing endeavours to persist in its own being is nothing else than the actual essence of the thing itself". As we should put it: the actual essence of the thing itself is to be found in the genes which control its synthetic activities, and which, being capable of maintaining their own specificity by self-reproduction, direct this synthesis always into the paths which are characteristic of that particular organism.

Thus an outlook which stresses that development and self-maintenance are essential characteristics of organisms is in no way in opposition to current biological thought; but that body of thought has already gone further than Dr. Russell in relating these aspects of life, through the concepts of the gene and of natural selection, to the rest of Nature.

C. H. WADDINGTON.

TREES AND FUNGI

Trees and Toadstools

By Dr. M. C. Rayner. Pp. 71+18 plates. (London: Faber and Faber, Ltd., 1945.) 6*s*. net.

IN this book the general results of investigations carried out over sixty years by a great number of workers is brought together and presented in a form that should appeal to the layman. It concerns the structure and nutritional relationships between the green and the non-green plant when the connexion is not one of host and parasite but a delicately balanced relationship in which each partner has a share.

The first chapters give an outline of the physiological processes of green and non-green plants and form a useful introduction to the more complex relationships discussed later in the book. This leads on to a consideration of soil problems and the part played by the micro-fauna and flora in the breaking down of humus.

After this introduction, the author gives a description of the toadstools of woodlands with the structural features of their mycelia and the part they play in the soil and in the higher plants.

The later chapters give an interesting account of the fungus roots of coniferous and some other trees and discuss the different types of mycorrhiza and their reciprocal relations with higher plants. The researches of the author have contributed much to our knowledge of some of these problems; but many of the complex relationships are still unknown and occur not only in plants living in soils rich in humus but also in plants living in the desert where humus is almost absent. Dr. Rayner has given an outline of the technique necessary in these investigations but has confined her descriptions mainly to the macroscopic features, and this part of the book contains some excellent photographs from her own and some other sources.

The book gives a well-balanced description of the problem as it stands to-day in regard to conifers, and has provided a new meaning for the layman of the striking display of toadstools in our woodlands during the autumn months.

SCIENCE AND RECONSTRUCTION*

By The Right Hon. Sir JOHN ANDERSON, G.C.B., G.C.S.I., G.C.I.E., F.R.S., M.P.

MY interest in science dates back to the early years of this century when, having secured a scholarship of sufficient value to enable me to devote a year or two to further study abroad, I went to Germany to engage in postgraduate research in physical chemistry. As it happens, the subject allotted to me was an investigation of the radioactive properties of uranium, a curious coincidence in view of certain activities in which I have recently been engaged.

I had previously, while still an undergraduate in Edinburgh, become somewhat concerned about the economic prospects for specialists in natural science. I had noticed, for example, that, apart from schoolmastering and the limited establishments of the science departments of the universities, there seemed to be no demand in Britain for chemists. Things indeed were still much as Lavoisier found them when, having been condemned to death and being disposed to protest, he was informed coldly that the world had no need for chemists. The War Office assessment of the value of trained chemists was expressed in 1905 in the offer of a commencing salary of £130 a year. The only industrial openings one heard of were occasional vacancies in breweries, and those usually went to well-trained specialists from Denmark. The fact is that, at that time, the staple industries of Britain still relied on the traditional methods which had served them well before international competition became acute. I well remember learning from a schoolfellow the horror with which his father, the proprietor of a famous tannery, had received the suggestion that he should introduce a few trained chemists into his tanyard.

However, youth is generally optimistic, and it was not until I had been at work for some time and found that my researches were proceeding rather slowly—as was bound to be the case from what we now know of the nature of the subject—that my mind began to dwell again on the prosaic question of earning one's living. On reflexion, it was borne in upon me that, while my research work was subject to no time factor, I was approaching the age limit for entry into the Higher Division of the Civil Service. I decided that I had better try for that before it was too late, and thus have no occasion to regret having missed an opportunity that would never recur. In the result, I was awarded a position as a "2nd Class Clerk in the Colonial Office"—such was the unimaginative and even repellent terminology then in vogue—and the whole tenor of my life was changed.

This fragment of autobiography enables me to bring out two facts that are not without interest in relation to the subject of my address. The first is that a good grounding in natural science can be a passport to the higher Civil Service, equally with the more usual training in the humanities, or in the history schools. The second, as exemplified in my own case, is that public administration does provide scope, apart from the professional or specialist grades, for men with a scientific training. For example, at the beginning of the War of 1914–18, we were confronted with a serious situation owing to the sudden cutting off of all supplies of those synthetic drugs and other fine chemicals and dyestuffs for which we had allowed ourselves to become entirely dependent on Germany. I was made responsible for organizing measures for making good the deficiency so far as the medicinal requirements of the country were concerned, and I was also in continuous consultation with those who were dealing with the explosives and dyestuffs situation. It would take too long to describe the nature of the action that was taken—and taken successfully—to overcome what might have been a fatal handicap in that conflict, but I certainly could not have played my part had it not been for my earlier training. Much more recently that training has again stood me in good stead in discharging the responsibilities that fell to me as Lord President of the Council, leading up to the work on atomic energy to which I am still giving part of my time.

After this rather lengthy introduction, I come to my main subject—science in relation to reconstruction. The restoration of the economic life of Britain, and even the maintenance of the living standards established pre-war, is dependent on building up our industries to a level of efficiency higher than ever before attained. We have, to a large extent, lost the competitive advantage which our mineral resources and sea-power gave us in the halcyon days of last century. It is a further disadvantage that the exports which we must have in order to pay for vital supplies of food and raw materials are so much greater in relation to the corresponding home demand than in the case of either the United States or the U.S.S.R. It is on the quality of our products that we must rely to overcome all disadvantages, and to this end we must build up the finest possible bodies of technicians and make the fullest use of all new developments to which science can point the way. There is no reason to doubt our ability to do this if we know where the obstacles are and show a determination to remove them.

It has long been the fashion to decry our achievements in the field of applied science in the past, though in pure science they are, of course, universally recognized to have been outstanding. This has, no doubt, been in part a pose, exemplifying our well-known habit of self-depreciation. But, in my view, we have, in fact, lagged behind, and for two main reasons—partly because of the dead hand of tradition in our older industries and partly because of prejudice on the part of scientific men themselves. The profit motive was suspect in the higher academic circles before it became an object of attack in politics. The author of one of the most fundamental, and, as it turned out, most fruitful discoveries in electrical science plumed himself on the fact that it could be of no possible use. We must get rid, and we are getting rid, of all that nonsense so far as scientific workers are concerned.

I have said that we have lagged behind in the past; but things were never so bad as is sometimes made out. It is true that, apart from certain traditional lines such as acid and alkali production, chloroform, a few alkaloids and a very narrow range of dyestuffs such as alizarin (turkey red) and azo dyes (khaki) we had no chemical industry at the outbreak of the War of 1914–18. But, as I have already indicated, much was done during that War to overcome the initial handicap, and what has been achieved since—up to the outbreak of the recent War and during its progress—is full of encouragement. That was only to be expected in the case of the great new electrical

* Address before the Manchester Joint Research Council at Manchester delivered on December 6.

and chemical industries which had their foundations in scientific discoveries; but the older industries can also show many notable instances of a fresh outlook. Let me give a few illustrations in both categories.

In the textile industries, before the War, both uncreasable cotton and unshrinkable wool had been produced as a result of prolonged researches and, during the War, cotton fabrics have been produced in large quantity in which the fibres are so twisted that, on contact with water, they immediately swell and render the cloth waterproof, while at the same time allowing the passage of air.

In the steel and non-ferrous metal industries new processes have been developed and new materials produced for the first time, including many types of alloy steel, and great quantities of magnesium metal from sea-water.

In television we undoubtedly led the world before the War and we are probably still ahead. In the various applications of radio-location, founded upon investigations originally conducted under the Radio Research Board of the Department of Scientific and Industrial Research, we were in almost every instance well ahead of all competitors. Here is a magnificent example of co-operation between science and industry, resulting in the production of power valves working on wave-lengths of a few centimetres—a development undreamt of a few years before.

Between the Wars, Britain gained all the speed records in the air, on water and on land, with British pilots and British machines made in British workshops. We now hold all these records again.

All aeroplanes are fitted with perspex—the transparent non-splintering plastic produced in Britain—and, during the War, our chemical industry produced also polythene—the finest insulator known—without which some of the most important applications of radio-location would have been impossible.

An insecticide (gammexane) probably more efficient than D.D.T., a range of selective weedkillers, linked with the discovery of the hormones produced in the growing tips of plants—which may well mean a revolution in agricultural methods—a synthetic adhesive, heat-, water- and acid-proof, giving unions stronger than those obtained by riveting or spot welding, are other achievements of our progressive and virile chemical industry.

The 'Mosquito', produced straight from the drawing board, largely by coach-building and furniture factories, and made operational in the record period of twenty-two months, could carry the same weight of bombs to Berlin as a 'Flying Fortress', with a crew of two instead of ten, and at 100 miles an hour faster. The British-made 'Lancaster' carried the biggest bomb-load and was the only aircraft capable of carrying the 10-ton bomb. Between 1939 and 1944 the Merlin engine was doubled in horse-power and performance, without any increase in weight, and we have recently read with pride what they think of it in the United States.

In the manufacture of scientific instruments, where during the First World War we found ourselves sadly behind, there is the same story of technical skill and successfully applied research. Our bomb-sights and predictors, containing incredibly delicate and complicated mechanism, were nevertheless robust enough to be carried in our bombers, and simple enough in operation to be used by technically unqualified personnel.

The measures to combat the magnetic mine—perhaps our greatest anxiety in the early months of the War—the prefabricated breakwaters and harbours known as "Gooseberry" and "Mulberry", and the submarine pipelines for carrying petrol to the Continent, are other outstanding examples of technical achievement.

The illustrations, which I could multiply many times over in respect of a wide range of activities, show the high technical efficiency of British industries and their readiness to appreciate and develop new ideas born in our research laboratories. I give them now merely in order to establish my contention that the prognosis in this matter of the application of science in industry during the period of reconstruction is extremely favourable.

If that is accepted, let us look a little farther to see what is still needed in order to make the most of our resources, actual and potential, and of our opportunities. This is the kernel of our problem, and I will deal with it under the three heads of: (1) personnel, (2) finance and (3) organization.

Personnel. Sir Ernest Simon has directed attention to the discrepancy between the numbers of scientifically trained personnel turned out by the universities and other teaching establishments in the United States and Great Britain respectively. Allowing, as of course one must, for the difference in total population, we come very badly indeed out of the comparison. No doubt we make up to some extent in quality for lack of numbers; but the fact seems indisputable that the output has, in the past, been far below what the real needs of the country demand.

Clearly the deficiency, whatever it may be—and some further investigations seem called for under that head—cannot be made good all at once. The first step must be to enlarge the capacity of our teaching institutions, to get more scientific men of high quality back from war service to the teaching staffs of the universities, and then further to expand their numbers in the appropriate branches and perhaps to extend classroom and laboratory accommodation. This will require some order of priority, and I have little doubt that Mr. Morrison was right when he said recently that the needs of industry must at first give way to some extent before those of the universities. Then the necessary steps must be taken to increase the flow of students—not just any sort of students, but students of the right type—with the best kind of general educational background. Some propaganda will be needed here. I, personally, thought it unfortunate that so little was made of the importance of scientific and technical education during the passage of the Education Bill last year. I think a great opportunity was lost; but no harm may result in the long run if we do now what is urgently needed, and that is to prepare the best possible estimate—quantitative and qualitative—of future requirements. Universities, industrial firms and all other potential employers, including the Government, should make, and make known, their plans, indicating the types and numbers of trained men and women they require, the terms they offer and the kind of organization they propose. Such information, properly collated, will be an indispensable basis for the work of the committee on scientific priorities which Mr. Morrison recently announced in Parliament, and will also provide the necessary material for an approach to headmasters and, through them, to parents. As a first step one cannot go wrong in urging that everything possible should be done to hasten the release of trained scientific workers and of partly trained and untrained students from the various forms of war service.

Finance. I come now to a matter which I would rank as high in importance as that of personnel—finance. Here I may modestly claim to have myself blazed a trail. Broadly speaking, research and development must be financed in one or more of the following ways: by industrialists, either in their own laboratories or through industrial research associations, or through grants to universities and other scientific institutions; by universities and other scientific institutions out of their own resources; by Government, through research institutions of its own, or through grants to industrial research associations, to individual workers or to universities.

While I was still Lord President, the basis of grants to research associations was reviewed by the Advisory Council of the Department of Scientific and Industrial Research, under Lord Riverdale, and now arrangements have been made which represent a very substantial improvement on the old. Whereas the Department used to work on the principle that a Government grant would be available only until the association could be made self-supporting, the principle of a permanent grant is now accepted, and the basis of calculation has been liberalized. This marks a fundamental change in outlook. Government grants to universities are made through, and on the recommendation of, the University Grants Committee, which has been reconstituted to bring it into closer touch with the universities. About a year ago, I met the Committee of Vice-Chancellors and foreshadowed a substantially increased rate of grant which has now been voted by Parliament for the current year. In addressing them, I referred, in particular, to the need for a revision of university salaries, and I made it clear that, for the first time this year and in future, capital expenditure would be taken into account in assessing grants. It is left to each university to allocate its grant as it thinks fit, subject, of course, to any general understanding between that university and the Committee. Grants earmarked for specific researches are made not by the Committee but by the Department of Scientific and Industrial Research, with the approval of the Treasury. The Department also administers a scheme of maintenance grants for postgraduate students to allow them to be trained in methods of research.

Expenditure by industrialists is assisted not by direct grant but by a system of tax relief. Up to this year, maintenance expenditure in connexion with research was allowed as a deduction from earnings, in accordance with the usual principle, but no allowance was made in respect of expenditure of a capital nature. Under my Budget proposals of last year, to which statutory effect has now been given, all research expenditure in connexion with any particular industrial undertaking is allowed as a deduction from earnings before assessment of tax. This applies to all expenditure on buildings, plant and equipment, and represents a clear departure from the hitherto sacred principle that untaxed income must not be used to produce additions to capital assets. The same principle is applied to contributions by industrial firms to research associations or universities for research. The value, and of course the cost to the taxpayer, of such relief can be gathered from the contributions of this character which have already been made. For example, I.C.I. have given eighty fellowships at universities, of an average of £600 a year each, for research in physics and chemistry, and sciences derived therefrom, including engineering and chemotherapy. There are no restrictions or conditions attached to these fellowships. The Shell Group has made a gift of £435,000 to the University of Cambridge for a Department and chair of Chemical Engineering, together with a yearly grant of £2,500 for scholarships, and Courtaulds have given £118,000 for a similar purpose to the Imperial College of Science and Technology. I do not doubt that, as time goes on, many other similar grants will be made, thus adding greatly to the funds available for fundamental research. Of the funds devoted by the Government to research in its own institutions, little more need be said except that experience goes to show that there are certain forms of fundamental research which it will probably be agreed can be only satisfactorily developed in this way. I mention, by way of illustration, the work to be carried on in the new Aeronautical Research Establishment at Luton, and the future of research into the uses of atomic energy.

There seems to be little doubt that the expenditure directly incurred by the Government on research will increase substantially as time goes on. I hope in the summary I have given I have succeeded in showing that, so far as financial aid is concerned, we now have a framework within which all that is required can be secured.

Organization. The efficient use of our resources, limited as they must be for a long time to come, demands the best possible organization. Without seeking to dogmatize in any way, I would suggest the following: (*a*) There should be recognized standards of remuneration. (*b*) Conditions of service should be such as to facilitate interchange between establishment and establishment, and between one type of service and another. A uniform pensions scheme, such as the Federated Universities Superannuation Scheme, should help to this end. (*c*) Free exchange of information and ideas should be encouraged. (*d*) Without encouraging teaching institutions to undertake agency or contract work, research in applied science in such institutions should not be discouraged. (*e*) Similarly, the advantage of allowing a certain amount of fundamental research in industrial research establishments should be recognized. It should be remembered that it is to research, so conducted, that the discovery of the gas-filled lamp, which has meant an enormous annual saving in consumption of electricity, is due. (*f*) To prevent wasteful overlapping, the various industrial associations should establish special committees to discuss long-term programmes of research with representatives of the universities and the Government research organizations. In this connexion the question as to what fundamental researches can most profitably be conducted in Government establishments, or centrally in selected institutions, should be considered.

These are only suggestions which I hope may be thought worthy of further consideration; but, before leaving this part of the subject, in which my aim has been to indicate a number of questions which I think bodies such as the Manchester Joint Research Council might usefully explore, particularly in their regional aspects, it is right that I should say a word about the question of organization, so far as the arrangements of the central Government are concerned. I would unhesitatingly reject the suggestion, much canvassed of late, that there should be a Minister of Science. If we believe, as we must, that science enters into nearly every branch of human activity, there can be scarcely a Minister or a Department that is not interested in some way in the applications of science, and to add a super-Minister with general responsibilities and executive functions in relation to science could only lead to confusion. But that does not mean that I should be content with things as they are. We now have, in the Lord President, a Minister who is directly responsible for the general, as distinct from the departmental, scientific activities of the Government—the Department of Scientific and Industrial Research, the Medical Research Council, and the Agricultural Research Council. If a similar Council were set up for the social sciences, he would no doubt take that under his wing also. He has, during the War, been the channel of communication between the Scientific Advisory Committee and the Cabinet. I should like to see three changes made. First, I would have the Lord President formally recognized as the Minister responsible for dealing, on behalf of the Cabinet, and subject, where necessary, to reference to the Cabinet, with all general scientific questions in which the Government is concerned, including problems of organization, arrangements for Commonwealth or international conferences, and so forth. Secondly, I would continue the Scientific Advisory Committee, which was a special war instrument, on a peace-time footing, with some extension of personnel, as the principal adviser of the Lord President and the Cabinet in scientific matters. Thirdly, I would create a special section of the Cabinet secretariat, under an officer of appropriate qualifications and seniority, to assist the Lord President in the discharge of his new responsibilities. I believe, under arrangements such as these, the organization of the central Government would be suitably geared into the general complex of scientific organizations throughout the country.

Now I propose to say something about the position of our investigations into the use of atomic energy, and particularly about the prospects of industrial use. We have here a case where, as the result of a quite prodigious concentration of effort, such as could only have come about under the spur of war, results have been achieved in a few years which, in ordinary circumstances, would have taken perhaps ten times as long, or even longer. The work that has been done, of which I have some detailed knowledge, points clearly enough to certain lines of development in the future. I say nothing about the military problem. So far as industrial applications are concerned, it is quite certain that nothing has been discovered to justify the expectation that the energy released by an atomic explosion could be used directly as a source of industrial power, as, for example, the explosion of gases is used in an internal combustion engine, or a gas turbine, or in certain forms of rocket. The only method which existing knowledge suggests as practicable depends upon the conversion into heat of the energy released by a controlled process of nuclear fission. There is no difficulty in realizing this experimentally. But at the very outset practical troubles begin to arise. The first problem is to get the heat in a convertible form. This means a high temperature and involves very efficient controls and special measures to prevent corrosion of the metallic components and the rapid disintegration of the whole system.

The other major problem arises in connexion with the intense and very dangerous radiation by which the nuclear reaction is attended. This means elaborate shielding devices and a complete system of remote control. It would be absurd to suppose that these and other difficulties will not be overcome in time, and there is always the possibility of some fundamental new discovery completely changing the character of the problem ; but the best opinion I can offer—and it is not based on my own conclusion but on the conclusions of those on whose judgment I can most confidently rely—is that it will be many years before nuclear energy comes on the market, for any but the most limited and specialized purposes, in competition with existing sources of power.

Nevertheless, the ultimate possibilities are such that it is clearly incumbent upon us as a nation to devote all the resources we can spare to further researches in this field. There will, I imagine, be little doubt that the Government must, for the time being at any rate, take a leading part in the organization of such researches. Nevertheless, I must stress the fact that these recent discoveries open up an enormous new field of scientific work, and should give tremendous impetus to research by physicists, chemists and engineers. Indeed, the matter goes much farther, for the devices that are employed to produce the new element plutonium provide a ready means of procuring a vast range of new radioactive substances, including radioactive forms of common elements used in medicine. For example, radio-phosphorus, radio-iron, radio-sodium and many other substances can now be administered with the normal substance in proportions too small to be harmful, and their radioactive properties can be used to determine the exact behaviour of the particular therapeutic or nutrient agent in the human or animal organism. It does not require much knowledge, or much imagination, to realize what a revolution this may bring into medical science. In plant and animal physiology it may be equally important. There are also now available many alternatives to radium which may be both much less expensive and more manageable in radio-therapy. Thus, what may prove to be an uncovenanted benefit of inestimable value accrues to humanity as a by-product of the search for further means of destruction—proving again the truth of the saying that science often bestows her boons where no one planned.

THE EDIBILITY OF BIRDS

By Dr. HUGH B. COTT

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In the autumn of 1941 I happened to be preparing some bird-skins at Beni Suef, Middle Egypt, when a casual observation led to what has proved a somewhat fruitful and little-explored line of inquiry. A palm dove (*Streptopelia senegalensis*) and a pied kingfisher (*Ceryle rudis*) had been skinned and the carcases thrown aside. Hornets were very plentiful in the garden where I was working and soon collected on the meat ; but they were seen to concentrate their visits on one carcase, leaving the other almost neglected. Closer inspection showed that it was the dove that was receiving their attentions : with the kingfisher they would have little to do.

This apparent preference for one bird of the pair suggested further investigation, and it was decided to test the edibility of a wide range of birds, using *Vespa orientalis* as an indicator. Innumerable observations and experiments have hitherto been made on the palatability of various groups of animals—notably insects—with special reference to the theories of concealing and warning coloration and mimicry[1a]; but so far as I am aware, birds have remained largely neglected in this respect. The experimental method here adopted is an inversion of that so frequently used, in which birds have played the part of taster in investigations on the palatability of insects.

During the period 1941–44 many experiments were carried out, both in Egypt and the Lebanon, as a result of which it has been possible to assess the relative edibility of some forty species of birds in terms of hornet-preference. As the work developed, it became clear that the preferences of the insects were not dissimilar from those of other and quite unrelated meat-eaters such as the domestic cat and man. Details of this research will be published elsewhere: it is only possible here to refer briefly to some points of interest.

Among the birds tested were members of several Passerine families, including larks, wagtails, bulbuls, flycatchers, warblers, finches, buntings and swallows, together with birds of other orders including wryneck, hoopoe, buff-backed heron, white stork and little owl. One broad conclusion to be drawn from this work is that in the birds used there appears to be a relationship between edibility of the flesh and visibility of the plumage.

When arranged in order of preference, it is found that the species heading the list are those with the most effective concealing coloration, wryneck (*Jynx torquilla*) and crested lark (*Galerida cristata*) taking first and second place. Reading upwards from the bottom of the series we have the following: white-rumped black chat (*Oenanthe leucopyga*), pied kingfisher, mourning chat (*O. lugens*), masked shrike (*Lanius nubicus*), hoopoe (*Upupa epops*), hooded chat (*O. monacha*), golden oriole (*Oriolus oriolus*), red-backed shrike (*L. collurio*), lesser grey shrike (*L. minor*), European swallow (*Hirundo rustica*), common kingfisher (*Alcedo atthis*), red-rumped swallow (*H. daurica*)—all birds which in the field are highly conspicuous, the chats referred to being black-and-white deserticolous species.

Two striking exceptions to this general statement were provided by the buff-backed heron (*Ardeola ibis*) and white stork (*Ciconia ciconia*), both of which are extremely conspicuous and at the same time relatively palatable. It will be noted that both are large birds, which by reason of gregarious habits in the first case and powers of defence in the second are presumably not much subject to predacious attack in Nature.

This inverse relationship between edibility and visibility appears to occur in certain cases even within the limits of a family. For example, the finches and a bunting tested are found to stand in the following order of preference: greenfinch, house sparrow, Spanish sparrow, Cretzschmar's bunting, goldfinch, the last being rated least acceptable. Similarly, the blue-headed wagtail appears to be preferred to the white wagtail.

Experiments with the preferences of the cat for birds of various species have not yet reached a stage which would justify drawing any general conclusions; but it may be mentioned that in some instances the cat seems to be in close agreement with the hornet in his notion of what is fit to eat. For example, cats rate the pied kingfisher, hoopoe and a black-and-white chat (*O. finschii*) as relatively distasteful compared with sparrows, doves and other birds.

When we turn to the preferences of man, it becomes apparent that the birds—of many groups—which are most prized for the table are those which are specialists in cryptic camouflage; it is only necessary to mention a few: Passeres (skylark[2a], meadow lark[3]); Columbiformes (various fruit pigeons such as *Osmotreron pompadora*[4] and *Vinago delalandii*[5]); Charadriiformes (American golden plover[6] and grey plover[7], woodcock[8], common snipe[2b], stone curlew[9], bronze-wing courser[10a]); Gruiformes (bustards[2c,10b,11] and floricans[12,13a]); Galliformes (notably grouse and quail); Anseriformes (teal, mallard*, gadwall and others); Caprimulgiformes (European nightjar[16]).

Conversely, there is some evidence that numbers of highly conspicuous birds belonging to many different orders are definitely unfit for the table: sheld-duck[14], crocodile bird[15], magpie[16], and swallows being examples, though there are, of course, notable exceptions, such as the rook and starling, which make tolerable eating. Furthermore, here again within certain natural groups of birds the general correlation obtains. To take one example, among the Ralliformes, landrail[13b], clapper rail[17], and Carolina crake[18] are highly palatable and cryptic in coloration and habits; coot[19] and moorhen[16], though 'eatable', are scarcely accountable as table-birds and are among the most conspicuous members of the order.

Thus there would seem to be some evidence that the correlation above referred to is fairly general among birds as a class, and these facts would appear to bring birds into line with other groups of animals such as the Anura[1b] and Insecta[1a], in which the relationship between edibility and cryptic coloration on one hand and between certain classes of conspicuousness and unpalatability on the other, has been well established.

I am at present collecting further information on the above subject and would be glad to hear from members of expeditions, travellers, sportsmen and others who have tasted birds—of whatever kind—which are *not* usually considered as game. Such information is infrequently included in ornithological literature and would be welcomed as a contribution to the present investigation.

* Where the seasonal or sexual plumages differ, that of the female in breeding plumage is taken as the standard.

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EXPERIMENTAL IMITATION OF TUMOUR CONDITIONS

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THE cancer investigator is primarily concerned with the nature and origin of uncontrolled growth in malignant tumours which depends on an aberration of normal cell division. The processes which underlie normal cell division and differentiation, however, are still far from being well understood. Cytology, so long as it remained a descriptive science, made little progress in this direction. But the change which occurred some fifteen years ago, when its methods became analytical and experimental, has by now made possible the interpretation of some growth processes in physico-chemical terms[1]—an advance which has in its turn stimulated a greater co-ordination among the different methods of approach to the cancer problem. Especially this advance has confirmed the remarkable uniformity of nuclear behaviour throughout living organisms. Experimental work on widely different organisms can therefore now be used to contribute to our understanding not only of normal but also of abnormal growth.

The condition of malignancy is accompanied by well-defined cytological changes, most of which have long been known[2,3,4]. The following are the most conspicuous: (1) 'hyperchromatism'—increase in size of chromosomes and a higher staining capacity; (2) enlarged nucleolus; (3) increased basophily of the cytoplasm; (4) increase in cell and nuclear size and a greater variability in both; (5) wide variation in chromosome number; (6) defect in the spindle at mitosis with its consequences.

Although different tumour types vary in detail, all malignant tissues and most malignant cells show one or more of these abnormalities which serve to distinguish them from the normal. Thus the difficulty of the cytologist has not been so much in distinguishing between normal and malignant cells but in deciding: (1) if there is any causal sequence among these various abnormalities; (2) if any of them can be regarded as a specific criterion of malignancy.

Failure to answer these questions has led to much confusion and differences in interpretation. The easiest way to find an answer seems to be to try to separate the particular abnormalities experimentally. We must try to induce them in non-malignant tissue in order to determine their immediate causes, from which it should be possible to establish the probable sequence of events in tumours.

A good deal of evidence is already available in support of the view that nucleic acid supply is the crucial factor in the causation of cancer. This view has developed largely from the work of Caspersson[5], Darlington[6], Claude[7] and others who have demonstrated the significance of nucleic acids and nucleoproteins in cellular activity.

We are concerned with two types of nucleic acids:

(1) *Ribose nucleic acid*, produced apparently by the heterochromatic regions of the chromosomes and found in the nucleolus and in the cytoplasm. This acid is closely associated with protein synthesis and especially with reproduction of self-perpetuating protein elements in the cytoplasm. It can be identified, with the help of a ribo-nuclease check, by staining with pyronin by Pappenheim's pyronin-methyl green technique.

(2) *Desoxyribose nucleic acid* is produced during the prophase of mitosis, becomes attached to the chromosomes and is responsible for their reproduction. At the same time the nucleolus, with its store of ribose nucleic acid, dissolves and disappears. After the chromosomes have divided and reformed as daughter nuclei they give up most of their charge of desoxyribose nucleic acid, which is again reconstituted as ribose nucleic acid in the developing nucleolus. Desoxyribose nucleic acid gives the characteristic aldehyde reaction with Schiff's reagent which is the basis of Feulgen's technique for chromosome staining.

The two nucleic acids are thus mutually (although not directly) convertible, and in normal tissue are so balanced that chromosome reproduction and cytoplasmic synthesis are in step. Caspersson considers that this balance is maintained by the regulating action of heterochromatin[5], which under normal conditions allows the synthesis of the correct amount of nucleic acid. Any increase in activity or in amount of heterochromatin will, on this view, lead to excessive nucleic acid synthesis and increase in the rate of nuclear division.

Koller[8], from a cytological study of human tumours, finds support for this view. Thus malignant cells have a much enlarged nucleolus and a highly basophilic cytoplasm because of the higher concentration of ribose nucleic acid. In the same way the 'hyperchromatism' by which cytologists have described the larger and more deeply staining chromosomes is due to a heavier charge of desoxyribose nucleic acid. Confirmation of this interpretation has recently been obtained by La Cour[9], who found that in the unbalanced differentiation of cells in pernicious anæmia, nucleic acid supply in the red corpuscle precursors is increased and that in the white diminished.

We may now consider two experiments in which at least certain aspects of the primary change to malignancy is imitated. The first was with *Sorghum purpureo-sericeum*. Here the presence of supernumerary heterochromatic chromosomes is accompanied by a striking change in nuclear behaviour in the developing pollen[10]. Normally the first pollen grain division which produces the vegetative and generative nucleus is followed after a week by the division of the generative nucleus to give two sperms. But in pollen grains with extra chromosomes the vegetative nucleus immediately divides again and continues to divide until it has produced four or five generative nuclei. These divisions would presumably continue further but for the thick wall which confines the growth within the grain and makes it an 'encapsulated tumour'. Thus a nucleus which normally would not divide is induced to divide again and again owing to the action of extra heterochromatin introduced by a chromosome change.

The second experiment consists in the induction of giant mutant forms in yeast by the action of chemical carcinogens and camphor[11,12]. For example, Thaysen and Morris[12], by subjecting cultures of the non-sexual *Torulopsis utilis* to the action of camphor for several days, obtained a mutant in which the average cell volume was about double that of the normal. The generation-time for this new *major* strain ranged between 58 and 90 minutes as compared with 100–110 minutes for the standard strain. It was also found that "the percentage of nucleic acid extractable from a given weight of cells of the new strain was considerably higher than that obtained from the standard *Torulopsis utilis* strain". In agreement with this, the phosphorus content of the mutant strain, calculated a b c

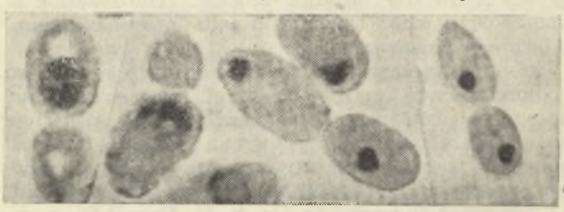
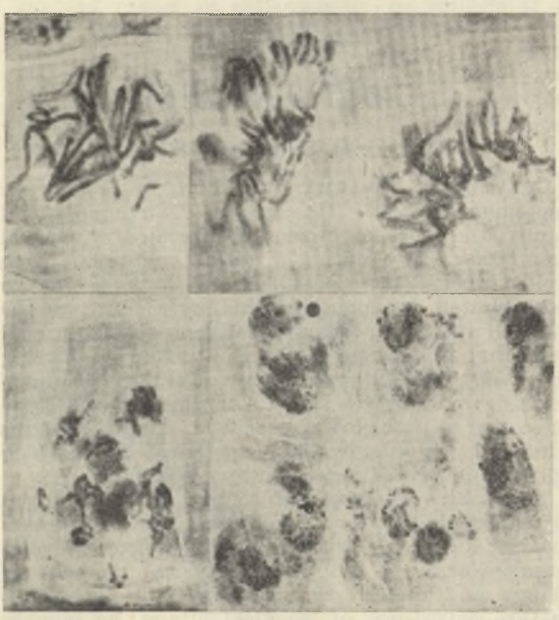


Fig. 1. *a*, *Torulopsis utilis* VAR. *major* SHOWING LARGE FEULGEN-NEGATIVE NUCLEOLUS AND CRESCENT-SHAPED FEULGEN-POSITIVE CHROMOSOMES; *b*, VAR. *major* AND, *c*, NORMAL STRAIN SHOWING DIFFERENCE IN CHROMOSOME SIZE. ALL PREPARATIONS STAINED WITH FEULGEN FOLLOWED BY ACETIC-LACMOID. (× 2000).

from the dry yeast, was 2·8 per cent as compared with 1·8 per cent for the original strain. Through the kind co-operation of Dr. Thaysen and his colleague, I was able to study the two strains cytologically. I found that the nucleolus in the *major* strain was more than four times the volume of that in the standard strain. Moreover, as can be seen in Fig. 1, the two Feulgen-positive bodies, which can be properly regarded as chromosomes[13], are also greater in size. They presumably have an increased nucleic acid charge. Further, in a culture of the normal strain which had been under the influence of camphor for two days, the mutant forms could be readily identified by their staining. They showed increased basophilic reaction with Pappenheim's pyronin methyl green solution. They had a higher concentration of ribose nucleic acid.

Thus in four respects, rapidity of division, increase in size of nucleolus, and concentration of nucleic acids of both kinds, the mutant yeast cell exactly agrees with a cancer cell. The change produced by camphor, as Thaysen remarks, is "deep-seated" or genetic since the mutant form is stable and has not reverted to the normal after continued subculturing. The action of camphor on yeast, therefore, has been to induce a specific effect on growth, by increasing nucleic acid metabolism. But here the change, although genetic, is probably not due to change in the chromosomes but rather to one in cytoplasmic determinants. The change is analogous to that of carcinogens in causing cancer in animals.

a b c



d e

Fig. 2. ACTION OF LACTIC ACID ON ROOT-TIP TISSUE IN RYE (18 HR. TREATMENT WITH 250 P.P.M. *p*H 5·66. 28° C.). MULTIPOLAR SPINDLE AT EARLY ANAPHASE, *a*; LATE ANAPHASE, *b*; METAPHASE, *c*; TELOPHASE, *d*; WITH CONSEQUENT MULTINUCLEATE CELLS AS AT *e*. (× *c*. 1000).

Perhaps the most striking cytological abnormalities in tumours are those due to defect in the spindle at mitosis. In fact Boveri[14] originally thought that the multipolar spindle was a primary cause of tumour formation; but we now know that initially cell division is by normal mitosis and these abnormalities are absent or very rare in many types of tumours. They therefore seem to be secondary.

The spindle can be upset in normal tissue by a number of physical and chemical agents, producing effects which imitate the behaviour in tumours. The more effective agents, such as colchicine, cause complete suppression of the spindle, apparently by preventing the orientation of its long-chain molecules. The less-effective agents induce a multipolar spindle by preventing the co-ordination of partially orientated spindle elements. Complete suppression of the spindle leads to polyploidy, while partial suppression results in hypo-diploid nuclei and multinucleate cells. Both these conditions occur in tumours with the expected results. The latter is the commoner and is probably the main cause of cell degeneration.

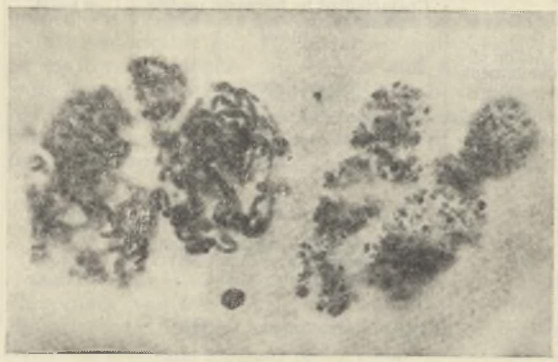
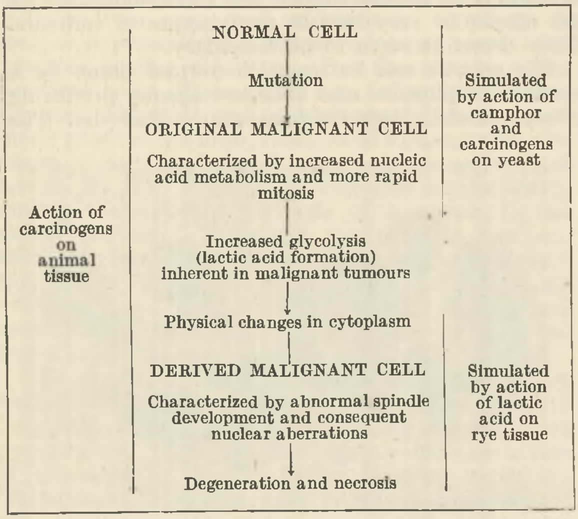


Fig. 3. AFTER 24 HR., SHOWING IRREGULARLY SHAPED POLYPLOID NUCLEI AT RESTING STAGE AND AT PROPHASE. THIS TYPE OF NUCLEUS IS OFTEN FOUND IN TUMOURS (*cf.* REF. 4, P. 257). IT RESULTS FROM A MULTIPOLAR SPINDLE. (× *c*. 2000.)

One of the less effective agents is lactic acid. Its action can be shown readily on root-tip tissue in rye. The abnormalities produced closely resemble those in malignant tumours (Figs. 2 and 3). The precise mode of action of lactic acid on the spindle will be discussed elsewhere. The action seems to be the same as that which Heilbrunn[15] terms "reversible coagulation of the cytoplasm" observed by him when studying the action of fat solvents and heat. The term "thixotropic setting" which Seifriz[16] uses for the action of anæsthetics in producing a reversible gelation of the cytoplasm probably refers also to the same physical change.

Now the accumulation of lactic acid, due mainly to aerobic glycolysis, is regarded as "the outstanding metabolic characteristic" of tumour tissue[17]. Hochwald[18] and Reiss[19] had already concluded that lactic acid was the cause of the central necrosis in tumours. More recently Guyer and Claus[20], from viscosity measurements, suggested that this acid might be the agent which induces the mitotic abnormalities by causing partial coagulation of the cytoplasmic proteins. We may therefore suppose from these different kinds of evidence that lactic acid is the immediate cause of spindle defects in tumours. The question as to whether the formation of lactic acid in the tumours is itself secondary and a consequence of excessive growth activity, or whether it is bound up with the primary change in nucleic acid metabolism, does not now need to concern us.

To summarize, we may arrange, as in the accompanying table, the probable sequence of events in tumour development.



The change from a normal to a malignant cell is of the nature of a mutation, but until recently we had few ideas as to how it might be brought about. We have seen that a change in the amount of heterochromatin may be one way and we also know that mutation in a single nuclear gene may induce polymitosis[21]. We now have the further possibility of an effect through mutation in cytoplasmic elements. In this there is a remarkable similarity of views between two workers engaged in entirely different fields of research. Darlington[22], basing his arguments primarily on cytogenetic grounds, considers that there are genetically important elements in the cytoplasm which are self-perpetuating. These he calls plasmagenes. They differ from viruses in their adaptation to the 'host' and in their interaction with the cell nucleus. Both plasmagenes and viruses depend on chemical equilibrium for their continuance, so that a mutation in them can alter not only their equilibrium but also their interaction with the cell nucleus. Potter[23], as an enzymologist, considers that cancer is caused by the introduction of "an abnormal protein" which he calls a "cancer virus" and which he assumes "to arise spontaneously in certain cases, to be formed by the action of carcinogenic agents in other cases and to be introduced into the cell in the case of tumours known to be of virus origin". This 'cancer virus' is assumed to be almost identical with an enzyme *X* which he believes is a complex of respiratory enzymes of the nature of a ribonucleoprotein. A change in equilibrium is brought about by competition between these two for the same 'building blocks', and when the ratio $\frac{\text{cancer virus}}{\text{enzyme } X}$ reaches a certain value the cell becomes malignant. The change is thought to be irreversible because the 'cancer virus' competes more successfully than enzyme *X* for the 'building blocks'. The reaction is analogous to what Darlington calls 'suppressiveness' as between plasmagenes.

These experiments and theories seem to show that a common basis has been reached for the study of the cancer problem. They seem to show also that the elements of the problem can be pulled apart, robbed of their complexity and subjected to profitable experimental tests that will now have a meaning.

I am much indebted to Mr. S. H. Revell for technical assistance with some of the experiments discussed in this article.

Note added in proof. Since the above article was written, Potter has also directed attention to this remarkable coincidence of views in *Science*, **101**, 609 (1945).

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OBITUARIES

Prof. C. E. Spearman, F.R.S.

Charles E. Spearman was born in London on September 10, 1863. He came of a Northumberland family, the more prominent members of which had been either Army officers or mining engineers. With one or other of these possible professions in view, he gave himself assiduously in his early youth to the study of applied mathematics. Eventually he entered the Army, and served in the Burmese War. During the Boer War he was deputy assistant adjutant-general in Guernsey; and in this way became interested in problems of personnel. He resigned his commission in order to study experimental psychology at Leipzig, at Göttingen, and at Würzburg.

Spearman's interests first centred on the problem of determining sensory thresholds. Here his mathematical knowledge enabled him to suggest several improvements in the psycho-physical methods of Wundt and Muller. But, coming as he did with a mature experience of practical affairs, he quickly decided that the experimental psychology of those days was engrossed too exclusively with questions far removed from those of ordinary human life. His early aim, therefore, was, in his own phrase, "to connect the psychics of the laboratory with those of real life".

The idea of using sensory thresholds to measure general ability or intelligence he took from Galton. In the United States, one of Galton's own pupils, J. McKeen Cattell, had applied laboratory tests to students, and, on assessing the results by Galton's method of correlation, had been surprised to find that the agreement of the various modes of assessment was extremely low. Cattell, Thorndike and others were consequently led to the conclusion that there was no general faculty of intelligence, as Galton had assumed, and that the mind was a "host of particular abilities". This negative result Spearman believed to be erroneous. Adapting a more elaborate statistical procedure from Karl Pearson and Yule, he endeavoured to eliminate the irrelevant factors which appeared to be obscuring the underlying correlations, and then found an almost perfect agreement between the various tests and assessments. These results he published in one of his earliest and most important articles in 1904—"On General Intelligence Objectively Determined and Measured". In this research he believed he had demonstrated "the profoundly important conclusion that there exists something which we may provisionally term general sensory discrimination"; and this he identified with "general ability" in Galton's sense. Few papers in psychology have given rise to such productive controversies and to so long a list of suggestive investigations.

In 1907 Spearman returned to England. At that time McDougall was interested in Galton's scheme for an anthropometric survey which should include intelligence tests. In France, Binet had produced his scale of tests; and at University College, London, Karl Pearson had already been working with correlational methods applied to teachers' assessments of their pupils' mental characteristics. On McDougall's recommendation, Spearman was appointed reader in experimental psychology at the same College, and four years later became Grote professor of mind and logic. During the next twenty years, he and his research students were systematically engaged on demonstrating the supreme importance of a general intellectual factor, which he himself preferred to call, not intelligence, but *g*.

At first Spearman relied mainly on sensory tests of a laboratory type—the æsthesiometer, Galton's cartridge weights, and a dichord of his own invention for testing discrimination of pitch. Other workers in Great Britain, however, showed that better results might be secured by using tests of more complex abilities, particularly those involving the perception or use of logical relations, such as could be given without apparatus to large groups of children in school. These suggestions Spearman readily incorporated. His main interest, however, lay rather in the theoretical study of the mind in general than in educational or other practical applications.

His later work with tests of this new type led him to formulate his "fundamental noegenetic principles". According to this doctrine all intellectual processes were to be conceived as depending essentially upon the "education of relations and correlates". His first book, on "The Nature of Intelligence and the Principles of Cognition", gave a systematic picture of the workings of the human mind based on this hypothesis. It was quickly followed by a second volume on "The Abilities of Man", which gave the final statement of his 'two-factor theory'—the theory that all mental processes are pervaded by a single central function, combined with a second highly specific factor peculiar to each test or trait.

Later, in a smaller work on "The Creative Mind", Spearman endeavoured to show how his fundamental noegenetic laws could be applied to other intellectual fields. His last work, "Psychology Down the Ages", was a historical study in two volumes of the evolution of the chief doctrines of psychology, with the intention of showing how nearly all the more acceptable formulations were dim foreshadowings of his noegenetic laws.

In 1931, on his retirement, Spearman was made emeritus professor in the University of London. He had already been elected a fellow of the Royal Society, and been honoured by numerous scientific bodies in Germany, France, the United States and Czechoslovakia. All who worked with him, or discussed with him their common problems, will testify to his remarkable gift for inspiring enthusiasm both in his own colleagues and pupils and in those who were drawn to criticize his views. Few have possessed his power of co-ordinating and concentrating the interests of numerous research workers on a single fertile scheme. C. BURT.

Dr. Frank Chapman

THE death has been announced of Dr. Frank Chapman, who for many years was head of the bird departments of the American Museum of Natural History, until his retirement in 1942.

Dr. Chapman was born at Englewood, New Jersey, on June 12, 1864, and on leaving school entered a New York bank. So keen on birds was young Chapman that he used to get up at daybreak, hunt birds on the way to the station, where he would arrive at 7.30, change into his city clothes and catch the 7.39 to town. All his spare time was devoted to collecting and studying birds, and after six years in the bank he decided that was not the life he could live. In 1886 he left the bank and two years later was appointed assistant to Dr. J. A. Allen, curator of mammals and birds in the American Museum of Natural History. This was the beginning of his long connexion with the Museum and the great bird collection which was so wonderfully developed under his charge. When Chapman joined the Museum the study collection consisted of only 300 skins! Under the joint care of Allen and Chapman the collection grew apace, and the latter made many expeditions to different parts of the States and the West Indies to secure further material.

Early in his career Chapman became interested in the life zones of the Andes, and thereafter most of his work was devoted to studying the birds of the Andes in Venezuela, Colombia, Chile and Peru. He published several monographs dealing with the subject and was the recognized authority on the birds of these regions. As Chapman himself wrote, he was more interested in watching live birds in the field than in collecting or studying skins, and after the First World War he settled on an ideal spot for the purpose. This was Barro Colorado Island in the Panama Canal zone, and there he built a house, called Fuertes House, after his friend the American bird artist. Here he could study and photograph wild life to his heart's content. Among the trees toucans, guans, parrots and woodhewers abounded, and down below were ocelots, peccaries and other interesting mammals. The joys of the place and what can be seen are described in "My Tropic Air Castle".

The love of birds brought Theodore Roosevelt and Chapman together, and through Roosevelt he was invited by Lord Grey to the British Embassy to talk birds. Later, during visits to Great Britain, he was taken by Lord Grey to see birds in the New Forest and along the banks of the Itchin, and a few years after he paid a visit to Fallodon which is vividly described in the "Autobiography of a Bird Lover".

Chapman was an ardent protectionist and for many years edited *Bird Lore*, the magazine of the Audubon Societies. In 1893 he published a useful "Handbook of Birds of Eastern North America", a revised edition of which appeared in 1912.

To Chapman the American Museum owes the development of the habitat groups for which that Museum is so famous.

In 1898 Chapman married Miss Embury, and the marriage was an ideally happy one, his wife developing as keen an interest in birds as her husband and often accompanying him in the field. N. B. Kinnear.

Dr. H. E. Durham

By the death at Cambridge, on October 5, of Dr. Herbert Edward Durham both bacteriology and botany lose a distinguished discoverer. Durham was born in London in 1866. He was the son of A. E. Durham, senior surgeon to Guy's Hospital and a grandson of William Ellis, the economist. Educated at University College School and King's College, Cambridge, Durham took first class honours in Parts I and II of the Natural Sciences Tripos at Cambridge in 1890, his medical qualification in 1892 and his F.R.C.S. in 1894. In 1894 he was appointed to a Gull Research Studentship and went to Vienna to work in Gruber's hygiene laboratory. There he and Gruber first directed attention in January 1896 to the diagnostic value of the agglutination of pathogenic organisms by the sera of animals immunized against them. Later in 1896 this test was applied by Widal for the diagnosis of typhoid fever. The reaction was known for a time as the Gruber-Durham reaction and later as the Widal reaction.

In 1896 Durham was appointed a member of a Royal Society committee upon diseases transmitted by tsetse flies. In 1897, while holding a Grocers' Scholarship and working at Cambridge, Durham described the Durham tube, a small inverted test-tube placed in bacteriological media for the collection of the gas produced by fermentative organisms. In 1900 he headed the expedition sent by the Liverpool School of Tropical Medicine to Brazil to study yellow fever. Durham contracted the disease, but lived to lead, during 1901–3, the expedition sent by the London School of Tropical Medicine to Christmas Island and Malaya for the study of beriberi. From Malaya, Durham brought home samples of *Derris elliptica* and appears to have been the first to recognize the insecticidal properties of this plant.

In 1905 Durham was appointed supervisor of the laboratories of Messrs. H. P. Bulmer and Co., of Hereford, where he worked for thirty years upon problems of fermentation and apple culture in relation to cider production. In 1909 he took his Sc.D., Cambridge, and in 1935 retired to Cambridge, where he cultivated, in his garden there, many culinary plants and herbs which are rare in Great Britain. Many of these he had himself introduced, and several Cambridge gardens still cherish those which he gave to their owners.

A retiring man who could be a charming and humorous companion, Durham acquired a wide culture and was always ready to give of his rich store of experience to his many correspondents and friends. He sent many valuable contributions to the "Dictionary of Gastronomy". Responsible for much original work, and also for much stimulation of others, he scattered his knowledge in private communications and did not always obtain full credit for his discoveries. Not least among his later activities were his hobbies of photography and lathe-work. While his bacteriological work is well known, his contributions to botany, and especially perhaps to apple culture and problems of fermentation, merit wider recognition. The sympathy of everyone will go out to Mrs. Durham, who survives him, and to his brother, Colonel F. R. Durham, secretary of the Royal Horticultural Society. G. Lapage.

Prof. C. E. Wright

The death of Prof. C. E. Wright, professor of gunnery and mathematics at the Military College of Science, Woolwich, occurred on October 30, 1945.

Charles Edward Wright was born on January 31, 1886. He studied mathematics at King's College, London, between 1903 and 1906, where he was awarded the Clothworkers' and Sambrooke Exhibitions and a Drew Gold Medal. After graduating he was appointed tutor at University Correspondence College, Cambridge. During the War of 1914–18 he served with the Royal Engineers and was, for a short time, at the Research Department, Woolwich. He obtained the degree of M.Sc. by examination at Queen Mary College, London, in 1921.

In 1919, Wright took up the appointment of senior lecturer at the Military College of Science, Woolwich, where he spent the greater part of his career and where he was appointed professor shortly after the retirement of Prof. H. C. Plummer in 1940. Wright was wholeheartedly devoted to the instruction of army officers to fit them for technical staff appointments. The many students who have worked under him have proved their value during the recent War, where their services have proved indispensable in the development and production of weapons to meet the needs of the soldier in the field. Apart from his professional duties, Wright was actively engaged in voluntary war work, but alas, his zeal was greater than his strength.

He was an able mathematician and, in addition to his instructional duties and the preparation of Service text-books on ballistics, he found time to write several papers and notes published in the *Philosophical Magazine* and educational journals. The elegance exhibited in some of his mathematical work is most attractive. He was of a cheerful disposition and was popular with students and staff alike. C. J. Tranter.

We regret to announce the following deaths:

Dr. E. F. Armstrong, F.R.S., on December 14, aged sixty-seven.

Mr. A. Lucas, O.B.E., sometime chemist to the Egyptian Department of Antiquities, on December 9, aged seventy-six.

Mr. F. Milsom, for many years the official referee for Hepatics for the British Bryological Society, on December 5.

NEWS and VIEWS

Geological Survey (Scottish Office)

Dr. Murray Macgregor

The retirement of Dr. Murray Macgregor during the past autumn brings to a close a long and eventful term of office in charge of the Geological Survey in Scotland and north-east England. On joining the staff in 1909, Dr. Macgregor was stationed at Edinburgh, and in 1921 succeeded the late Mr. L. W. Hinxman as district geologist for the Central Coalfield and Northern Highlands; and in 1925 he was appointed assistant director for Scotland. Dr. Macgregor has rendered signal service both in organizing and personally conducting geological investigations for industrial developments in Scotland. He will, however, be remembered especially for the close contact which he maintained with those engaged in the coal-mining industry. In addition to his many contributions to the economic geology of the coalfields in Survey memoirs he has written a comprehensive account of Scottish Carboniferous stratigraphy, which was published by the Geological Society of Glasgow in 1930. During the past twenty years a large programme of normal survey work was carried out under Dr. Macgregor's guidance. The first revision of the Scottish and Northumberland Coalfields was completed, as well as a second revision of a large part of the Central and Lothians Coalfields and the oil-shale field of the Lothians. Up to the outbreak of war in 1939, striking progress had also been made with the primary survey in several areas in the Highlands and Islands. During the war years the activities of Dr. Macgregor and his staff were concentrated upon economic aspects of geology, related to the War itself and to reconstruction. A number of special investigations were carried out at the request of several ministries and to meet various industrial needs. Among these may be mentioned a detailed examination of Scottish limestones, mainly for agricultural purposes, a search for feldspar, silica-rock, mica and iron ore, surveys of underground water resources and of areas for open-cast coal production, and an investigation of building materials, including slates, brick-clays and granite.

In addition to his Survey duties, Dr. Macgregor has contributed largely to scientific journals and to the work of scientific societies. He has served as president of the Edinburgh Geological Society and the Geological Society of Glasgow, as editor of the *Transactions* of the latter Society over a long period of years, and as member of council of the Royal Society of Edinburgh. He is a vice-president of the Geological Society of London. In 1941 he was awarded the Murchison Medal by the Geological Society of London, and in 1944 the Clough Medal by the Edinburgh Geological Society.

Mr. T. H. Whitehead

Dr. Murray Macgregor is succeeded at Edinburgh by Mr. T. H. Whitehead, who joined the Geological Survey in 1914. During the First World War he served in the Suffolk Regiment, reaching the rank of captain, was severely wounded and afterwards was appointed to the Intelligence Staff. Among his important published work is the Geological Survey memoir on the South Staffordshire Coalfield, in which he collaborated with T. Eastwood, now assistant director in England; other memoirs of which he is a principal author are those on Coventry, Birmingham, Stafford, Wolverhampton and Shrewsbury. In 1933 he was transferred to the Manchester branch office, and from 1935 until it was closed in 1938 he was in charge of that at York. During the War he has been the district geologist responsible for urgent revision work on the coal and other resources of the Midlands, and for the six-inch survey, now completed, of all the Jurassic and Cretaceous iron-ores south of the Humber. Whitehead's judicial mind and terse clarity in argument are acknowledged and admired by his colleagues of the Survey and Museum, whom he has served well both as counsellor and advocate, and by his fellow-members of the Council of the Geological Society. These qualities, coupled with his extensive and intimate knowledge of British geology and its economic applications, make him admirably suited for his new post.

University of London, Institute of Education:

Prof. Karl Mannheim

Dr. Karl Mannheim has been appointed to the chair of education in the University of London, in succession to Sir Fred Clarke, who retired at the end of the last academic year. The appointment dates from October 1, 1945, and is tenable at the University Institute of Education. Mannheim's appointment may be regarded as both daringly original and a sign of the times. A Hungarian by birth, he gained his doctorate in philosophy at the University of Budapest, and was becoming known as a sociologist of repute while teaching in Germany before 1933. The events of that year brought him to Britain, and to a lectureship in sociology in the London School of Economics. In 1940 his impressive study of "Man and Society in an Age of Reconstruction" established him in the front rank of sociologists, and the collection of essays he published in 1943 under the title of "Diagnosis of our Time" enhanced an already considerable reputation.

Mannheim has always shown himself peculiarly sensitive in his sociological studies to the importance of education, and since 1941 has been attached to the staff of the London University Institute of Education, but can scarcely be regarded as possessing the normal professional qualifications for a professorship in the subject. Paradoxical as it may seem, this is probably why he has been appointed. The Institute, under the leadership of Sir Fred Clarke, has in recent years been laying increasing emphasis on the social aspect of public education, and insisting that the philosophy of education must be worked out on a sociological basis. No one is better fitted to undertake this task than Mannheim.

Benjamin Rush (1745-1813)

Born on Christmas Eve two hundred years ago, Benjamin Rush remains one of the most versatile and colourful personalities in American history, and the greatest physician America has produced. One of the signatories of the Declaration of Independence, treasurer of the U.S. Mint, founder of the American Anti-Slavery Society, advocate of prison reforms, it is upon his reputation as a physician that his immortality rests. His account of the yellow fever epidemic which swept Philadelphia in 1793 won him international fame, while the singular courage and the untiring devotion to duty which he displayed in attending to its victims raised him in popular imagination to the stature of a hero. His descriptions of cholera infantum and of focal sepsis—he was one of the first to recognize the relationship between infected teeth and arthritis—have become medical classics. Though many of his theories are to-day of historic interest and many of his therapeutic measures have been discarded, his title 'father of American psychiatry' is justified, for his "Medical Inquiries and Observations upon the Diseases of the Mind", published the year before his death, was the first American text-book on psychiatry, and he was a pioneer in the humanitarian treatment of the insane and an early advocate of occupational therapy in asylums. A magnetic teacher, an elegant speaker and a felicitous writer, Benjamin Rush was a pronounced individualist, who made enemies as well as friends. He was one of the greatest 'bleeders' in the annals of medicine.

The Good and the Clever

It was a happy inspiration which caused the Master of Balliol to base his Memorial Lecture at Girton College, Cambridge, upon the verses in which Miss Wordsworth referred to the possible antithesis between goodness and cleverness, when "the good are so harsh to the clever", and "the clever so rude to the good". He set out to show that nothing will make sense of the distinction, except to acknowledge that goodness is in its own way as rational as cleverness. "Let us begin with the Greeks," said he to his hearers, and then, as became his learning and his office, he took them through a fascinating journey from the Greeks on to the Renaissance, and thence to Rousseau, Kant and Bentham. Naturally, those of his hearers who had studied philosophy to some purpose must have got most out of this part of his discourse; but none of them could have missed the heart of his theme, which was that "the really good man may not be learned or intellectual, but though he need not be clever he is not stupid. He has the imaginative power of putting himself in other people's places, and of going right past differences of rank and wealth and ability and all else, and getting to the essential human being". One remark will be of special interest to readers of this journal, devoted to science in its broadest interpretation. "Psychological classifications, case papers, and all the elaboration of card indices does help one to deal more effectively with people, but the most elaborate and scientific analysis misses out the authentic individual, just exactly what goodness should have the power of apprehending. If we only deal with people as specimens of this or that, sorted out in the most elaborate of ways . . . can we retain that reverence for the human personality as we meet it in ordinary men and women which is essential to any true service. . . . Good and noble work has been done in that way, but not the best." It must have been a salutary experience for members of the particular audience addressed to be so effectively reminded that good men and women are sometimes also exceedingly wise, and that clever men and women can sometimes also be exceedingly unwise.

"Faraday's Encyclopedia of Hydrogen Compounds"

The scope of this important new aid to using chemical literature has already been explained in *Nature* (April 21, p. 467). Dr. Faraday and his publishers are to be congratulated on the fulfilment of their promise to keep it up to date by producing the first instalment of supplementary pages. This is no mean achievement in relation to the well-known difficulties of the moment with which printers and publishers are faced. The single sheets of the first replacement addition issue (C_1—C_5) cover the year 1944 and bring the book up to January 1, 1945 (Chemindex, Ltd., Manchester 2. Pp. 64. 15*s*.). We know of no other book of reference in science which is in this happy position. On trial, it was found that the necessary work of changing the sheets and adding new ones took less than half an hour; the instructions given are so precise that any intelligent person can follow them. The new matter fits easily into the system and only three correction sheets were required, which is a testimony to the care taken in the original preparation. A leaflet enclosed with the sheets promises a second volume dealing with C_6 and C_7 shortly, while others are ready for the printer: we believe the practising organic chemist will be eager for these. It is of interest to note the degree to which the various abstract journals cover the published literature. A spot test of a random sample has shown that *British Chemical Abstracts* covered 27 per cent, *American Chemical Abstracts* 57 per cent and *Chemisches Zentralblatt* 70 per cent. On the basis of these figures, there seems room for improvement in Great Britain.

Research Corporation Grants for Research

Some 2,500,000 dollars is being offered in grants to educational institutions by the Research Corporation of New York, a non-profit organization devoted to advancing research and technology by the use of revenues from inventions assigned to it by public-spirited inventors. Preference in making these grants will be given, other factors being equal, to smaller institutions and those of more limited financial resources for research. The five-year programme announced by Dr. Joseph W. Barker, acting president, who is dean of engineering at Columbia University and until recently was a special assistant to the Secretary of the U.S. Navy, provides for one to two hundred grants of 2,500–5,000 dollars a year to enable young men of science, engaged for the most part in war research in uniform or as civilians, to undertake at universities and colleges research of peacetime importance in pure science, especially chemistry, physics, mathematics and engineering. The first grants will be made shortly by a special committee of eminent men of science from industrial and university laboratories. Grants will be made to the institutions at which successful candidates will work and teach. The funds allotted will be available for the purchase of needed equipment and for the employment of assistants either as fellows or otherwise. Awards will be based primarily upon the demonstrated ability of the men who will conduct the researches and contribute to the teaching programme of the school. The grants are made possible by the fact that during the war years research programmes that would be normally supported by Research Corporation grants have been laid aside in order to free men and facilities for war research. The Research Corporation was begun in 1912 with the gift, through Dr. F. G. Cottrell, of patent rights on electrical precipitation, which is used for removing dust, fume and mists from industrial gases and from the atmosphere. From revenues derived from these and other patents it has made grants of 1,279,637 dollars in past years to fifty-two institutions. In recent years the Corporation has served universities by administering inventions that may

[None of the vacancies in these columns relates to a Man between the ages of 18 and 50 inclusive or a Woman between the ages of 18 and 40 inclusive, unless he or she is excepted from the provisions of the Control of Engagement Order, 1945, or the vacancy is for employment excepted from the provisions of that Order.]

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arise in their laboratories. Correspondence in connexion with the grants should be sent to Dr. Robert R. Williams, Research Corporation, 405 Lexington Avenue, New York 17, N.Y.

Sterilization of Syringes

The difficulty of sterilizing syringes satisfactorily has been the subject of much recent research. A leading article in the *Lancet* (111, July 28, 1945) discusses this problem, the importance of which is emphasized by the publication, in the same issue (p. 106), by E. M. Darmady and C. Hardwick, of an account of hepatitis in ten subjects which followed the administration, by means of syringes, of pentothal and penicillin. These authors discuss the difficulties of proving the possibility of the transmission by syringes of the small amounts of blood containing an icterogenic principle which are required to produce hepatitis. The icterogenic principle involved is, these authors say, so heat-resistant that ordinary methods of sterilization of syringes do not destroy it. Prolonged heat is necessary for this. The use of needles alone for the withdrawal of blood for laboratory purposes would also prevent the contamination of whole syringes. The discussion of the same problem in a memorandum issued by medical officers of the Ministry of Health, recorded in the same issue of the *Lancet* (p. 116), comes to the conclusion that revision of existing injection techniques is required, and that hepatitis following the injection of arsphenamine, gold and other substances is an expression of homologous serum jaundice communicated by traces of blood transferred from subject to subject on syringes and needles. It is, of course, easy to overestimate the dangers which may result from existing methods of sterilization of syringes. Millions of injections must be done every year without untoward results. But the evidence recorded here emphasizes the importance of the report of the committee appointed by the Medical Research Council (M.R.C. War Mem. No. 15, H.M. Stationery Office, 1945, 4*d.*) to consider the sterilization of syringes. This report should interest the experimental worker as well as the medical man, for, in certain kinds of experimental work, the results may be seriously affected by inadequate attention to the cleaning of syringes and needles.

Toxicity of D.D.T. to the Housefly

During the War the importance of the compound usually referred to as D.D.T. as an insecticide has been demonstrated. In the *Bulletin of Entomological Research* (**36**, Pt. 2; Sept. 1945), E. A. Parkin and A. A. Green, of the Pest Infestation Laboratory at Slough, describe experiments in testing the efficacy of controlling the housefly (*Musca domestica*) by means of D.D.T. They found that when applied as a spray containing 0·1 per cent w./v. or more of D.D.T. in kerosene it is very toxic to the insect in question. At 1 per cent strength D.D.T. gives an effective spray for practical use but at lower concentrations its action is too slow. Its advantages as a fly spray are that it can be prepared synthetically; it is almost odourless; it will not stain fabrics, etc.; it appears to have no marked irritant effect upon the operators up to 2 per cent strength; it is extremely lethal to flies both in solution and in the solid form after deposition on walls, etc., from volatile solvents. Its main disadvantage is its slow rate of action on flies unless used at a concentration of at least 1 per cent w./v.; but, even at this strength, affected flies produce an unwelcome buzzing, for at least an hour prior to the wing muscles becoming paralysed. The slow action of this compound can be overcome by the admixture with D.D.T. of a small amount of pyrethrins which has a very rapid paralytic effect. It would appear that the two constituents act independently, the rapid effect of the pyrethrins keeping the flies immobile until the slow lethal action of the D.D.T. has had time to come into effect.

British Rheologists' Club

The fifth annual general meeting of the British Rheologists' Club was held at the Engineers' Club, Manchester, on October 5. Dr. G. W. Scott Blair, the retiring honorary secretary, reported on the activity of the Club during the year 1944–45, and stated that membership had reached the three hundred mark. The Club had held four general meetings, and two bulletins of information, abstracts, etc., had been issued. Close contact had been maintained with the Society of Rheology (United States), and contacts had begun with rheologists in the U.S.S.R., France and other countries. The Proceedings of the Oxford Conference were nearly ready and would be published in book form. Prof. E. N. da C. Andrade was re-elected president for the ensuing year. Dr. E. W. J. Mardles of the Royal Aircraft Establishment, Farnborough, Hants, was elected secretary; a vote of thanks was moved from the chair to Dr. Scott Blair, who had been secretary of the Club since its inception in 1940. The meeting was followed by a joint discussion with the Manchester Section of the Oil and Colour Chemists' Association on the general rheological properties of suspensions.

Announcements

Dr. H. R. Hulme, sometime a chief assistant at the Royal Observatory and recently director of naval operational research at the Admiralty, has been appointed scientific adviser to the Air Ministry.

At a meeting of the Council of the University of Sheffield held on December 14, the following appointments were announced: Mr. P. C. Sylvester-Bradley to be assistant lecturer in geology, and Dr. H. McIlwaine to be lecturer in biochemistry. Dr. A. Elliott, lecturer in physics, Dr. J. C. Speakman, lecturer in chemistry, and Mr. F. G. Hannell, lecturer in geography, have resigned.

The All-Union Scientific Research Institute of Plant Industry of the U.S.S.R., made famous under the directorship of N. I. Vavilov, has returned to Leningrad from evacuation, and has begun to set its collections in order and to re-establish its exchanges of seeds and literature. None of the priceless collection of seeds has been lost. During the siege the Institute was guarded by five members of the staff who remained in Leningrad. This corrects the statement in the obituary article of Vavilov (*Nature*, **156**, 622; 1945) that "the residue of his collections was eaten by the famished people". Altogether about thirty members of the staff were killed or died of starvation during the siege. The director, Academician Eichwald, and the librarian, Mrs. Heintz, are anxious to resume exchanges. The address of the Institute is Ulitsa Gertsena 44, Leningrad.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Freedom for Scientific Research

In these days of 'planning' there seems a general desire by many, little qualified, to put science in its place.

The horrible threat that science was no longer to be international has been protested against, very rightly, by all that is best in the United States, Britain and elsewhere.

The idea that there should be two classes of men of science, one knowing government secrets and research work, and those outside the sacred fold, was, and is, a very shocking proposal. Now, I notice a school of thought that wants to plan fundamental research, forgetting that once you instruct anyone as to what they are to study, you are *ipso facto* imposing *ad hoc* research upon them.

I think it was the great J. J. Thomson who once said after the Boer War that if you had planned and organized science to explore the best way of finding bullets located in the human body, certainly probing might have become an art and the human body a pin-cushion, but not that way would there have been brought about X-rays. Mr. Herbert Morrison, I was delighted to see, speaking at the British Association, appreciated the point, and said you could not dragoon men of science relative to fundamental research. Well spoken.

Much remains to be done in planned *ad hoc* research by capable men to exploit (unpleasant word) for the good of all the discoveries of the laboratory. Here much can and should be done, for it is only by producing the most up-to-date and advanced design and technique that we can hope to export our goods in a world where every country is seeking to become industrialized.

We must appreciate that the pure scientist is a creative artist. No one must boss him. No one drive him. We are lucky in the production of these great men. Certainly arrange that their lives be free of monetary worries, but let us remember that their life interest and pleasure is research, and leave them to carry on undisturbed by the stormy blasts and distractions of a world licking its wounds and quarrelsome after a protracted fight. Do not of all things plan their lives. Some will produce future miracles no doubt, but most perhaps may not be so lucky. That should not worry us; our duty is to have given them the chance, and although many of their names may not be handed down to the future praised by all, yet they will have done good and noble work, for which we should all be no less grateful.

BRABAZON OF TARA.

70 Pall Mall,
London, S.W.1.
Dec. 10.

Lognormal Distributions

In pointing the way towards easier manipulation of non-normal distributions, Prof. J. H. Gaddum's recent article[1] should act as a heartening stimulus to geologists. Sedimentary petrologists in particular will welcome it, for the efficacy of Prof. Gaddum's main theme is already established in certain parts of their domain.

We are indebted chiefly to the American school. Up to the present, most attention has been paid to the normalization of size distributions. The 'phi' and 'zeta' scales of Krumbein[2,3,4,5] are illustrative in this connexion, representing logarithmic transformations of the Wentworth and Atterberg geometric grade-scales. The transformations are carried out by substituting logarithms of the grade-scale limits for the appropriate diameter-values (ξ mm.) through

$$\varphi = -\log_2 \xi, \text{ and } \zeta = 0{\cdot}30\dot{1} - \log_{10} \xi.$$

They yield, of course, scales having arithmetic intervals; hence the 'normal phi curve' (ref. 5, p. 252), and the direct application of conventional statistics to problems of sedimentary distribution. But one needs to recall that moment-analysis shows that sediments yield many local size-distributions which are not rendered significantly (or even approximately) normal through the simpler logarithmic transformations. The indiscriminate use of a particular function is consequently to be deprecated, and a preliminary attempt should always be made to discover that one which will either give the 'perfect' measure directly, or at least ensure the greatest chance of obtaining it. Should such treatment prove vain, then recourse must be had to 'post-mortem' search (often the simpler of the two) succeeded by individual transformation of the variates. It must not be forgotten, too, that the skewness and kurtosis of certain sedimentary variables (such as particle size measured in terms of diameter) probably have considerable geological significance, and must not be lost to sight.

Distributions of sedimentary composition may be approached in the same way. Within geologically 'synchronous' horizons, the areal variation may be considerable (for example, up to 500 per cent per 700 sq. miles in certain Lower Cretaceous rocks). Its underlying variates (specific and varietal proportions), however, are seldom lognormal. A contributory cause may be the complex relation that seems to exist between the compositional standard deviation (σ_h) and the compositional mean (ρ_m). For actively sorting, eroding or aggrading currents (providing the mean patchiness they impose is not widely different from ρ_m) this may be of the form

$$\sigma_h^2 = \rho_m^2 \left(J + \frac{1 - 2\rho_m}{L\rho_m^2} S\right) + \sigma_r^2,$$

where S is variable and related to the numbers of particles removed and added locally, σ_r is the random deviation and J and L are horizonal constants for all allogenic minerals[6]. If experiment should confirm this, then a general transformative panacea appropriate to each horizon may be proved to exist. On the other hand, a few mineral species and varieties do occasionally yield to transformations of the type $X = \log (x_0 - x)$. (This is inevitably the premier alternative to $X = \log x$ for those minerals which are liable to absent themselves locally.)

Neither can Prof. Gaddum's pertinent remarks concerning variance be overstressed to the geologist. No assumption of homogeneity of the variance within sedimentary universes should be lightly made, but be based where possible on rigorous test[7,8]. This is particularly urgent during detailed studies of 'patchiness' by the analysis of variance when differences of locality are used as differences of 'treatment'. The sedimentary petrologist, confronted in his quarry with the solitary pebble embedded in fine silt, or the rare particle locally so abundant, will most heartily concur in Prof. Gaddum's advocacy of the merits of the geometric mean.

PERCIVAL ALLEN.

Department of Geology,
University of Reading.
Nov. 5.

[1] *Nature*, **156**, 463 (1945).
[2] *J. Sed. Petrol.*, **4**, 65 (1934).
[3] *J. Sed. Petrol.*, **6**, 35 (1936).
[4] *Neues Jahrb. f. Min.*, etc., **73**, 137 (1937).
[5] "Manual of Sedimentary Petrography" (New York, 1938).
[6] *J. Sed. Petrol.*, in the press.
[7] Bartlett, M. S., *Proc. Roy. Soc.*, A, **160**, 268 (1937).
[8] Bartlett, M. S., *J. Roy. Statist. Soc.*, Suppl., **4**, 158 (1937).

PROF. J. H. GADDUM has dealt[1] comprehensively with the subject of logarithmic transformations, including $X = \log (x + 1)$. The use of this transformation was first advocated by Williams[2] on the grounds that it approximated to the logarithmic transformation for large values of x, and avoided the difficulty of a non-finite value of X when x was zero; but this argument is rather arbitrary, since the same might be said of any function of the form, $\log (kx+1)$. Also, the existence of zeros implies usually that many of the values of x will be small and lie in the range where $\log (x + 1)$ behaves like a square-root transformation. Consequently, some further investigation of the properties of this function seems to be called for.

Since there are several instances[3,4] where a good correction for discontinuity has been found by adding $\frac{1}{2}$ to the variate, it appears possible that the transformation, $X = \log (2x + 1)$, would have the advantages of $X = \log (x + 1)$ and would also correct the ordinary logarithmic transformation for discontinuity.

Two trials have recently been carried out to investigate empirically the properties of these two transformations. Each was concerned with the numbers of fruits on apple trees. In the first there were four randomized blocks of seven treatments with seven trees to a plot. It was thus possible to compute a variance within plots with 24 degrees of freedom for each treatment, and to compare the seven variances thus obtained by evaluating the ratio of their geometric and arithmetic means. This quantity, known as L_1, has been tabulated by Nayer[5]. Using the $\sqrt{x + \frac{1}{2}}$ transformation, L_1 was $0 \cdot 863$; for $\log (x + 1)$ it was $0 \cdot 980$; and for $\log (2x + 1)$ it was $0 \cdot 982$. The first of these values indicates a significant departure ($P < 0 \cdot 01$) from uniformity in the variances; but it will be seen that both the logarithmic transformations were successful in equalizing them. The mean numbers of fruits per tree for the different treatments varied from 1·3 to 10·0, so the values of the variate largely lay in the range where discontinuity is serious and where $\log (x + 1)$ most closely approximates to $\frac{1}{3}\sqrt{x}$. In the second trial there were 64 pairs of trees of which 32 had been starved of potash while the rest had received adequate fertilizer, the treatment means being respectively 8·3 and 12·5 fruits per tree and the ranges within which the data lay being respectively 0–35 and 0–67 fruits. Variances with 32 degrees of freedom were worked out between trees within pairs within treatments. Using the transformations $\sqrt{x}$, $\sqrt{x + \frac{1}{2}}$, $\log (x + 1)$ and $\log (2x + 1)$, the values of L_1 were 0·968, 0·952, 0·9987 and 0·9998 respectively. Of these the second approaches the 5 per cent significance point, while the last two represent unusually good agreement between the variances. Indeed, such good agreement must be considered as partly fortuitous.

It would be rash to base much on the results of two small trials. However, there is reason to think that the transformation, $X = \log (x + 1)$, may be useful in dealing with small integral values despite the arbitrary element in its original selection, and despite its approximating sometimes to a square root and sometimes to a logarithmic transformation. It seems, also, that $X = \log (2x + 1)$ has at least an equal claim to be considered when such data are dealt with. It is to be hoped that this matter will engage the attention of some mathematical statistician, for it has obvious importance in the interpretation of a large class of data.

S. C. PEARCE.

East Malling Research Station,
East Malling,
Maidstone,
Kent.

[1] *Nature*, **156**, 463 (1945).
[2] Williams, C. B., *Ann. Appl. Biol.*, **24**, 404 (1937).
[3] Yates, F., *J. Roy. Statist. Soc.*, Suppl., 1, 217 (1934).
[4] Bartlett, M. S., *J. Roy. Statist. Soc.*, Suppl., 3, 68 (1936).
[5] Nayer, P. P. N., *Statist. Res. Mem.*, 1, 38 (1936).

DR. ALLEN's letter is welcome because it emphasizes the fact that no simple transformation can act as a panacea. The choice of the appropriate technique for any given problem must always be based, if possible, on direct evidence of its suitability. Another type of distribution has been discovered by Bagnold[1], who studied the distributions of the sizes of particles of sand deposited by wind. They were not lognormal, but when the logarithm of the diameter was plotted against the logarithm of the frequency, the observations were closely fitted by two straight lines. This fact presumably depends on the physical factors governing the deposition of particles from moving air, and may have many applications in geology. Such distributions can be normalized, but no simple general formula can be given since the slopes of the two lines may vary independently, and it is doubtful whether normalization would serve any good purpose. The logarithmic transformation is a useful tool; but, as Dr. Allen has emphasized, it must be used with care.

I should be glad to hear of more examples of lognormal distributions.

J. H. GADDUM.

[1] Bagnold, R. A., "The Physics of Blown Sand and Desert Dunes" (London: Methuen, 1941).

Kinematical Relativity

MY attention has been directed to Prof. H. Dingle's recent letter in *Nature*[1]. His supposed refutation of kinematical relativity is on a par with Dr. Samuel Johnson's refutation of metaphysics; my failure to reply to him directly is due to my reluctance to engage in such trivialities. I have already given him all the answer he needs by referring him to my mathematical papers, where the case of the collision of equivalent particle-observers is fully dealt with. You cannot wantonly re-graduate any casual clock and expect to secure a consistent physics. Kinematical relativity does not do so. In kinematical relativity it is shown that for consistent time-keeping to be possible, the various particle-observers whose temporal experiences constitute clocks must be members of *equivalences*. Regraduation is applied only to such equivalences; and it was shown by Whitrow and myself[2] that if two members of an equivalence ever coincide, then *all members coincide at the same event*. This technical point is well known to all who have studied time-keeping in relation to equivalences; it is fundamental, for example, in some recent unpublished work by A. G. Walker. In its application, it means that in a contracting universe of time-keepers, there would be a singularity which would be the counterpart of 'creation' in an expanding universe. Prof. Dingle is concerned with the timing of 'subsequent events'; there would be no subsequent events—Prof. Dingle would not survive the catastrophe—just as in an expanding universe there are no events anterior to 'creation'. There are other objections to a contracting universe, but the 'absurdity' which preoccupies Prof. Dingle is a monster of his own construction which simply adds point to the absurdity of a contracting universe. As the universe is observed to be expanding, Prof. Dingle's difficulties never arise.

Prof. Dingle appears to resent my referring him to my published papers. I did so because I resent being continually required to re-traverse in the looseness of verbal statement ground carefully covered, with all due reservations, in the technical papers. Prof. Dingle has recently suggested that kinematic relativity cannot account for the red-shift in the spectra of the galaxies, whereas kinematical relativity is essentially the exploration of the properties of a cloud of mutually receding particles. If Prof. Dingle does not like clock-regraduation, let him content himself with the t-scale, which describes a consistent physics as it stands; only, he will have difficulty with reconciling its results with Newtonian mechanics unless he faces the fact that Newtonian mechanics employs the τ-scale, not the t-scale.

E. A. Milne.

Wadham College,
Oxford. Oct. 15.

[1] *Nature*, **156**, 389 (1945).
[2] *Z. Astrophys.*, **15**, 277 (1938).

Prof. Milne states that I have misapplied his principle of clock-regraduation. If so, Prof. Haldane has made the same mistake. The lamp-post example was his, and his letter[1] expressed his belief that Milne's transformation was applicable to it. The only difference between us was that he trusted that Milne could make his transformation "lead to the same predictions of observable events"—he stated that this could be done—and I did not. Now Prof. Milne fails him by denying the obligation.

Nevertheless, Haldane and I are right. In Milne's accounts of his theory (in "Relativity, Gravitation and World-Structure", for example) the right to regraduate clocks is first claimed on *a priori* grounds independent of applications. Only afterwards are equivalences defined as consisting of observers who impose a particular voluntary restriction on this general right. Now Prof. Milne wants to deny the right to all but these observers. On what grounds? Indeed, for that matter, are not the nose and lamp-post members of an equivalence? They can fulfil the conditions, and no minimum number of *actual* observers is prescribed. Prof. Milne can, of course, claim that he can make what arbitrary distinctions he likes in an ideal hypothesis: in that case I challenge his right to call a romance a scientific theory. If, on the other hand, he says that the system of nebulæ provides an application for the theory, then I ask why the student of nebulæ may regraduate his clock, but not the student of lamp-posts or meteorites.

Herbert Dingle.

Imperial College, S.W.7.

[1] *Nature*, **156**, 266 (1945).

Clock-Regraduations and Relativistic Ccsmology

Prof. E. A. Milne has severely criticized relativistic cosmology for several reasons, one being that this theory is restricted to a definite time-scale, whereas his own theory admits of an infinite number of time-scales. Milne's construction of an 'equivalence' is invariant over a clock-regraduation, that is, a change from one time-scale to another.

In 1943[1], I suggested that the true analogue of clock-regraduations in relativity theory was to be found in Weyl's transformation of gauge. For the metrics of relativistic cosmology, this reduces to a change of the function γ in the line-element,

$$ds^2 = \gamma(r,t) \,.\, (dt^2 - dx^2 - dy^2 - dz^2).$$

It follows as a corollary to Infeld's[2] remarks that, with this interpretation, Maxwell's equations and Dirac's equations are invariant over clock-regraduations, while the relations of spherical symmetry and homogeneity are also preserved.

These remarks seem to dispose of one, at least, of Milne's main criticisms of relativistic cosmology.

T. J. Willmore.

Balloon Development Establishment,
Cardington, Bedford.
Aug. 18.

[1] Willmore, T. J., Ph.D. Thesis (London, 1943).
[2] Infeld, L., *Nature*, **156**, 112 (1945).

Adsorption Isotherms from Chromatographic Measurements

De Vault[1] has directed attention to the possibility of obtaining the adsorption isotherm of single solutes from chromatographic results of the 'diffuse' boundary, suggesting a differential equation for evaluating the measurements. A more useful equation can be obtained by integration of the equation

$$v/x - \alpha = dq/dc, \quad . \quad . \quad . \quad (1)$$

where α and q are the pore space and the amount adsorbed per gram of adsorbent. After developing with a given amount of pure solvent v, the amount of solute μ remaining in the column between the tail end of the chromatogram and the point $\bar{x}$ where the concentration $\bar{c}$ exists is

$$[\mu]_{\bar{c}} = \int_{c=0}^{c=\bar{c}} (q + \alpha c) dx \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

$$= \Big[(q + \alpha c)x\Big]_{c=0}^{c=\bar{c}} - \int_0^{\bar{c}} \left(\frac{dq}{dc} + \alpha\right) x dc \quad . \quad (2b)$$

$$= \overline{qx} + \alpha\overline{cx} - v\bar{c} \quad . \quad . \quad . \quad . \quad . \quad (2c)$$

Thus we obtain for the amount adsorbed

$$\bar{q} = f_{(\bar{c})} = ([\mu]_{\bar{c}} + v\bar{c})/\bar{x} - \alpha\bar{c} \quad . \quad . \quad . \quad (3)$$

Since μ is obtained experimentally as the total subsequent eluate in grams when $\bar{x}$ represents the length of the column, the complete adsorption isotherm can be obtained from a single chromatographic experiment simply by measuring the concentrations of successive eluted fractions.

The same integration can be carried out for two (and more) solutes. The fundamental equations for the mixed band of varying concentration

$$v/x - \alpha = dq_1/dc_1 = dq_2/dc_2 \quad . \quad . \quad . \quad (4)$$

require that co-existing values of c_1 and c_2 are functions of each other only; so that

$$q_1 = f_{(c_1c_2)} = \Phi_{1(c_1)} \quad . \quad . \quad . \quad . \quad (5a)$$

$$q_2 = g_{(c_1c_2)} = \Phi_{2(c_2)} \quad . \quad . \quad . \quad . \quad (5b)$$

This means that within the diffuse mixed band the two solutes behave individually like single solutes, having an adsorption isotherm which is modified by the presence of the other solute. They follow the equations:

$$v/x - \alpha = d\Phi_{1(c_1)}/dc_1 = d\Phi_{2(c_2)}/dc_2, \quad . \quad . \quad (6)$$

where the functions Φ_1 and Φ_2 depend on f and g, and also on the initial concentrations c_1^0 and c_2^0. This makes it possible to integrate equation 6 in the same way as equation 1, resulting in:

$$\bar{q}_1 = f_{(\bar{c}_1\bar{c}_2)} = ([\mu_1]_{\bar{c}_1\bar{c}_2} + v.\bar{c}_1)/\bar{x} - \alpha\bar{c}_1 \quad . \quad (7a)$$

$$\bar{q}_2 = g_{(\bar{c}_1\bar{c}_2)} = ([\mu_2]_{\bar{c}_1\bar{c}_2} + v.\bar{c}_2)/\bar{x} - \alpha\bar{c}_2 \quad . \quad (7b)$$

The presence of the tail band of pure component I seems to complicate the procedure, but this is actually not so; it can be shown that the total amount contained in the pure tail band is identical with what it would contain if equation 6 were continued beyond the point x_0, where solute II becomes zero. Thus μ_1 actually represents the total amount of solute I—both pure and mixed—which remains in the column when the eluate reaches the concentration $\bar{c}_1$.

This method of determining the adsorption isotherms of binary mixtures was found to give accurate and reproducible results, provided that the chromatographic separation was carried out with negligible 'channelling' and sufficiently slowly, so as to reach local equilibrium between solution and adsorbent everywhere. Several elution experiments using widely different ratios of c_1^0/c_2^0 are required to cover the whole range of a binary adsorption isotherm. Details and experiments will be published in due course.

Incidentally, if equation 7*a* is applied to the state of complete chromatographic separation of two solutes (by making $\mu_1 = m_1$, that is, the total solute I, $c_1 = c_1' = [c_1]_{c_2=0}$ and $x = x_0 = v.[\alpha + dg_{(c_10)}/dc_2]^{-1}$), this leads to the volume of pure solvent V required for complete separation:

$$V = m_1 \cdot \frac{\alpha + g'_{(c_1'0)}}{f_{(c_1'0)} - c_1' \cdot g'_{(c_1'0)}}, \quad . \quad . \quad (8)$$

which is in agreement with equation 6 of an earlier communication[2] where α had been neglected.

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[1] de Vault, D., *J. Amer. Chem. Soc.*, **65**, 532 (1943).
[2] Glückauf, E., *Nature*, **156**, 205 (1945).

Efficacy of D.D.T. in Soap

ONE of the remarkable features of the successful use of D.D.T.-impregnated clothing against the louse is the prolonged insecticidal effect of the clothing after several launderings, which would appear to indicate a strong absorption of the compound on the textile fibres. It occurred to us that animal hair might behave in the same way, and that useful results might be obtained in veterinary practice by the simple expedient of incorporating D.D.T. in soap[1].

Rather more than a year ago, we began experiments using varying proportions of the insecticide milled with household soap, and through the kindness of several dog owners and veterinary surgeons we were enabled to arrange practical trials. Twelve dogs were treated, including spaniels, sheepdogs and other long-haired breeds, having infestations either of the dog flea, *Ctenocephalus canis*, or the dog louse, *Trichodectes canis*, varying from slight to heavy. The animals were washed with the soap and warm water for 10–15 minutes, then rinsed down and rubbed dry in the usual manner. All fully grown parasites on the dogs were seen to be killed by this treatment. Seven of the dogs were kept under observation for a further three months, during which period they were freely exposed to re-infestation. The first sign of this appeared nine weeks later, and was limited to one animal, the others remaining free from re-infestation for the duration of the trials. In two instances eggs of the insects were known to be present, but these failed to hatch during the normal period. It therefore seems that a considerable degree of immunity from such parasites can be conferred upon animals by washing with D.D.T. soap, and this should be of importance not only to dog owners, but also to farmers and animal breeders in general.

With the view of estimating the amount of insecticide left on the hair, we analysed samples from treated and untreated animals by solvent extraction, and found that the percentage of hydrolysable chlorine in untreated hair was 0·002 per cent, whereas treated samples ranged from 0·005 to 0·007 per cent, equivalent to 0·05–0·07 per cent of D.D.T. It is surprising that these minute amounts of D.D.T. should give such a lasting effect. In the case of clothing impregnated with D.D.T., a concentration of only 0·01 per cent of the compound in the impregnation solution gave surfaces displaying noticeable insecticidal properties against the louse[2]. It would appear, therefore, that there is a similar absorption on the surface of the hair which prevents the D.D.T. from being washed off with the soap medium. Experiments to examine the mechanism further are in hand.

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Aug. 29.

[1] Geigy Company, Ltd., B.P. Application 1944.
[2] Domenjoz, R., *Schweiz. med. Wchschr.*, **74**, 954 (1944).

Differentiation of Nuclear Substances

ONE of the most used reactions of the clinical-chemical laboratory is the sedimentation reaction of the erythrocytes. It has been shown that sedimentation is hastened if, in consequence of physiopathological occurrences in the organism, the protein fractions of high molecular weight (fibrinogen, globulins) have increased[1].

We were able to demonstrate by means of some typical reactions[2] that the sedimentation reaction is chiefly of an unspecific nature and occurs with polysaccharides as well as with plasma proteins. We have succeeded, by fractional precipitation of the spherical colloid glycogen with methanol, in separating polymers of high molecular weight with diameters of approximately 100 glucose molecules from polymers with diameters in cross-section about ten times smaller. These different fractions of polymers produce accelerations of sedimentation which vary in accordance with their average particle size. This interdependence was shown with still greater accuracy with the rod-shaped molecules of apple pectin[3]. These long, partly methylated chain molecules of galacturonic acid are easily disaggregated by neutral hydrolysis. In proportion to this hydrolysis it is possible to measure the diminution of viscosity, the scattered light (Rayleigh), as well as the effect on acceleration of sedimentation.

After these preliminary experiments the following tests were made and compared: adenylic acid from yeast (mononucleotid, mol. weight 347), yeast nucleic acid (tetranucleotid, $C_{38}H_{49}O_{29}N_{15}P_4$, mol. weight 1303) and thymonucleic acid kindly provided by Prof. Rud. Signer (mol. weight 600,000–1,000,000)[4]. The last-named substance accelerated the sedimentation of erythrocytes even in a concentration of 6 mgm. per cent in measurable extent. On the other hand, the yeast nucleic acids produced no effect even in a concentration of 400 mgm. per cent. A provisional review of these effects follows.

From fresh human blood the erythrocytes are washed three times with saline on the centrifuge, in order to attain a suspension of 100 per cent in the end. 5 mgm. of the nucleic acids are first dissolved in 0·5 c.c. of homologous serum and then diluted with 0·5 c.c. saline. Then 5 mgm. of glycogen, gelatine and pectin are first dissolved in 0·5 c.c. saline and then diluted with 0·5 c.c. serum. Now we mix carefully 0·3 c.c. of these solutions with 1·2 c.c. of the 100 per cent erythrocyte suspension. With this the concentration of the nucleic acids and the polysaccharides is brought to 100 mgm. per cent. Five minutes later the suspensions are drawn up in original Westergren pipettes, and the sedimentation read off in mm. in corresponding periods:

Additions to erythrocytes	Sedimentation (min.) 10	20	40	60	90	120	20 hours
Serum and saline only	—	—	—	0·5	1	1·5	16
Adenylic acid from yeast	—	—	—	0·5	1	1·5	16
Nucleic acid from yeast	—	—	—	0·5	1	1·5	18
Thymonucleic acid	1·5	9	23	61	83	96	115
Glycogen	—	—	—	0·5	1	1·5	18
Gelatine	—	—	1	2	4	6	35
Pectin	2	26	89	116	119	120	123

The superior effect of the high molecular thread-like molecules of the sodium salt of thymonucleic acid (desoxyribonucleic) compared with the yeast nucleic acids (ribonucleic) is evident. Since both types of nucleic acids are polymers of mononucleotids, it follows that the new way of differentiating between them depends on their particle-size rather than on the different constitution of their ribose component. The exact interdependence of the molecular weight and the acceleration of the sedimentation of the erythrocytes could be proved with a sequence of homologous polymers like polystyrol for example. If such synthetic polymers were to be tried, one would have to ascertain that they do not possess hæmolytic properties. The substances mentioned above have none, and moreover the serum makes an excellent buffer.

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[1] Gordon and Wardley, *Biochem. J.*, 37, 393 (1943).
[2] Wunderly and Wuhrmann, *Schweiz. med. Wchschr.*, 74, 185 (1944).
[3] Wunderly, *Viert. Jahrsschr. Naturf. Ges. Zürich*, 89, 170 (1944). *Helv. chim. Acta*, 26, 755 (1943).
[4] Signer, Caspersson and Hammarsten, *Nature*, 141, 122 (1938).

Influence of Blood Pressure and Blood Flow on the Activity of the Respiratory, Vasomotor and Cardio-Regulatory Centres

THE classical opinion is that changes in the central blood flow and blood pressure affect and regulate in a direct central manner the activities of the respiratory and cardio-vascular centres: decrease of central blood pressure or blood flow was considered to stimulate directly the respiratory and vasomotor centres and to inhibit directly the tone of the vagal cardio-inhibitory centre; increase of central pressure or flow was considered to induce directly the opposite central reactions. Although numerous experiments performed by several different methods had shown[1] that blood pressure and blood flow changes occurring in physiological and physio-pathological conditions affect the activities of the respiratory and cardio-vascular centres not through a *direct* central action but only *reflexly* by way of the presso-receptor nerves of the aortic and carotid sinus areas, the classical theory of direct central regulation is still supported, particularly as regards the physiological control of the respiratory centre[2]. As this point is of fundamental theoretical importance, I have investigated the problem.

Experiments were performed on dogs anæsthetized with chloralosane; systemic arterial blood pressure, heart frequency and respiratory rate, amplitude and volume were registered. The experimental results may be summarized as follows.

(*a*) Occlusion or desocclusion of the efferent arteries (internal and external carotid and occipital arteries) of the common carotid arteries do not affect the activity of the respiratory and cardio-vascular centres.

(*b*) Occlusion or desocclusion of both vertebral arteries, alone or simultaneously with the efferent arteries of the common carotid arteries, also do not affect the activity of the respiratory and cardio-vascular centres, although the occlusion and release of these very important and main cerebral arteries induce very marked changes in the central blood pressure and blood supply.

(*c*) Decrease up to about 75 per cent of the normal blood supply or blood pressure to the isolated and perfused cerebral circulation does not stimulate directly the respiratory, vasomotor and cardio-regulatory centres.

(*d*) The isolated cephalic circulation being perfused by means of the two common carotid arteries only, occlusion of the common carotid with normal innervated carotid sinus induces a stimulation of the respiratory centre, while the occlusion of the common carotid with denervated carotid sinus does not stimulate the respiratory centre.

(*e*) Decrease of blood pressure in the circulatory isolated but normally innervated aortic or carotid sinus presso-sensitive areas induces, as shown previously[1], a reflex stimulation of the activity of the respiratory and vasomotor centres and a reflex inhibition of the vagal cardio-inhibitory centre; increase of blood pressure in the same presso-sensitive arterial areas induces reflexly a respiratory inhibition, decrease of general blood pressure and slowing of the heart.

These experimental results thus show conclusively that: (1) Changes of the central blood supply or blood pressure within physiological, but not to extreme physio-pathological, limits do not affect directly but reflexly the activity of the respiratory and cardio-vascular centres; (2) physiological or physio-pathological variations of the arterial blood pressure act reflexly, by means of the aortic and carotid sinus presso-receptors, on the activity of the respiratory and cardio-vascular centres.

These experimental facts thus do not support the theory of the physiological regulation of the activity of the respiratory and cardio-vascular centres by means of their own internal metabolic rate. Nevertheless, as shown by previous experiments of our laboratory[1–3], extreme pathological modifications of the central blood supply may affect directly the respiratory and cardio-vascular centres; but these influences have no practical importance for the *physiological* control of respiration and the circulation. These experimental facts also show once more that hypoxæmia does not stimulate the respiratory centre directly, but reflexly by means of the chemoreceptors, and are in favour of the probability of a specific function of the carbon dioxide in respiratory control.

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University of Ghent. July 16.

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[2] Schmidt, C. F., *Amer. J. Physiol.*, **102**, 94 and 119 (1932). MacLeod's "Physiology in Modern Medicine" (C. V. Mosby and Co., 1938). "Ann. Rev. Physiol"., **7**, 231 (1945). Gesell, Robert, "Ann. Rev. Physiol"., **1**, 197 (1939).

[3] Nowak, S. J. G., and Samaan, A., *Arch. Intern. pharmacodyn.*, **51**, 206 and 463 (1935). Heymans, C., Bouckaert, J. J., Jourdan, F., Nowak, S. J. G., and Farber, S., *Arch. Neurol. and Psych.*, **37**, 304 (1937). Marri, R., and Hauss, W., *Arch. intern. pharmacodyn.*, **63**, 469 (1939).

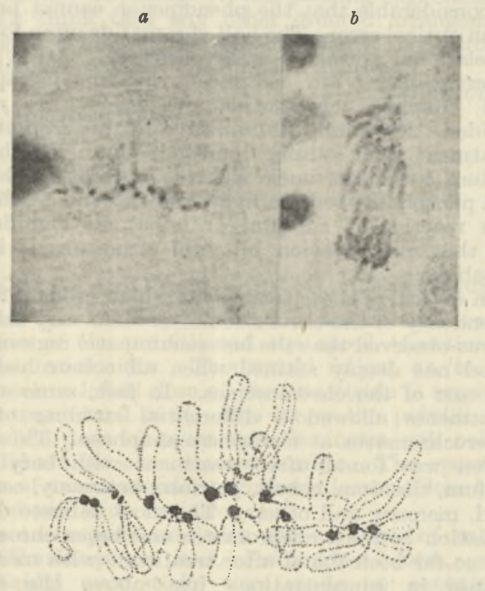
Cytological Reactions Induced by Inorganic Salt Solutions

AN investigation has been carried on for some time in this Laboratory into the immediate cytological effect of salt solutions on root meristems of *Allium Cepa*. Salts, mostly nitrates, of some forty metals were tested. A dilution series of 10–16 concentrations were studied for each substance, covering the whole range from total lethality down to such weak concentrations that no cytological effect could be detected. Fixations of root tips were made after 4 and 24 hours respectively. All slides were stained with crystal-violet.

A rather surprising result of these treatments was the very common occurrence of colchicine mitosis. Full and typical colchicine mitosis was met with in different concentrations of salt solutions of the following metals: lithium, beryllium, sodium, potassium, chromium, iron, cobalt, nickel, copper, arsenic, rubidium, yttrium, palladium, cadmium, barium, lanthanum, cerium, neodymium, erbium, gold, mercury, thallium, lead, bismuth and thorium. Almost all the substances tested showed at least some tendency to spindle disturbances such as are typical of incipient colchicine mitosis.

In many cases the activity threshold of colchicine mitosis was considerably lower than it is in colchicine, for example, in cupric nitrate, yttrium sulphate, lanthanum nitrate, auric chloride and lead nitrate, where it was situated at 0·000005–0·00005 mol., that is, at the degree of activity of acenaphthene. On surveying the whole material, a tendency of the threshold values to fall with increasing molecular weight was observed. In the fifth, sixth and seventh periods of the periodic system, all threshold values lie below 0·0005 mol., while in the second and third periods several thresholds lie as high as 0·05–0·2 mol. Especially such salts as are among the nutrients of plants have high threshold values (that is, low colchicine mitotic activity).

Several of the tested substances induced colchicine mitosis which in morphological details as well as in completeness was indistinguishable from the typical cochicine-induced mitosis. Such was the case, for example, with lead nitrate and potassium cyanide. Otherwise, the colchicine mitosis induced by salt solutions was found to show a greater variation in type than is found after treatment with organic substances. Thus, it was often incomplete: at several steps of the dilution scale it might occur intermingled



Allium Cepa, MITOSES OF THE ROOT MERISTEM, ROOTS TREATED FOR FOUR HOURS WITH 0·005 MOL. MERCURIC NITRATE; *a*, METAPHASE, × 1050; *b*, ANAPHASE, × 1050; *c*, CONTOUR DRAWING OF *a*, × 2100.

with normal mitoses. Other deviations from the normal course of colchicine mitosis, some of which have been described earlier from material treated with colchicine (Barber and Callan[1]), were: 'star-metaphase' (induced by, among other substances, lithium nitrate), 'ball-metaphase' (arsenious oxide), and 'bow-metaphase' with long arched chromosomes scattered over the cell (lanthanum and cerium nitrates). A peculiar type of mitosis, the mechanism of which may be quite unrelated to colchicine mitosis, was induced by nickel nitrate: the chromosomes behave as in colchicine mitosis, although a clearly visible spindle is present.

The colchicine-tumour reaction, which on treatment with organic substances usually accompanies the colchicine mitosis, is rarely present when inorganic salts are used for the induction of this form of mitosis. Nevertheless, there often occur characteristic growth disturbances: the roots become bent into hooks or even wound into spirals. This reaction may represent incomplete colchicine-tumour growth.

A reaction which is found only in exceptional cases after treatment with organic substances, but which was of rather general occurrence in the present experiments, is the induction of sticky chromosomes, manifested mainly by the formation of anaphase bridges. Pronounced stickiness was observed in the following treatments: lithium, beryllium, aluminium, titanium, chromium, iron, cobalt, nickel, copper, arsenic, yttrium, zirconium, molybdenum, palladium, cæsium, lanthanum, cerium, neodymium, erbium, tungsten, gold, mercury, thallium, bismuth, thorium and uranium.

Many of the salts tested caused deviations in the staining qualities of the chromosomes. Thus, during the contraction stages, the ability to take the stain was often decreased. At the same time the internal structure of the chromosomes was revealed more clearly than normally. At metaphase the chromatids were widely apart, although coiled around each other. Each chromatid was clearly divided, and the space between the two half-chromatids was sometimes so considerable that the phenomenon cannot be due to an optical error. The half-chromatids often formed a relational spiral of great clearness. After these observations, I am convinced that the metaphase and anaphase chromatids of *Allium* are really divided. Especially illuminative in this respect was treatment with sodium fluoride, sodium molybdate, sodium tungstate, auric chloride, mercuric chloride, and phosphate – sodium hydroxide buffers. Although this reaction is subvital or lethal, its significance for the interpretation of vital structures is indisputable.

In several of those treatments which brought about a tendency of the chromosomes to stain very faintly, it was observed that the heterochromatic regions still stood out deeply stained when all colour had left the rest of the chromosomes. In fact, some of the treatments allowed a differential staining of the heterochromatin at metaphase-anaphase. This condition was found after treatment with beryllium, sodium, titanium, cobalt, yttrium, antimony, cerium, gold, mercury and others. The most delicate differentiation between euchromatin and heterochromatin has so far been found after treatment with mercuric nitrate in concentrations just above the fixing threshold. It can be seen from the accompanying illustration that the 'body' of the chromosomes is very weakly stained, while small darkly stained portions are present, one on each side of the centromere of each chromosome. The greater part of the heterochromatin of this variety of *Allium Cepa* is localized to the vicinity of the centromeres.

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[1] *Proc. Roy. Soc.*, B, 131 (1943).

Carboxylase and Carbonic Acid

IT is generally understood that, in the action of carboxylase on pyruvic acid or other allied keto acids, carbon dioxide is directly split off. The following experiments show that this is not so, but that carbonic acid is first formed, the process contrasting with urease action in which carbon dioxide is directly produced in the splitting of urea. (This latter has been investigated by Roughton and Krebs[1] with respect to the action of carbonic anhydrase, and the present study of carboxylase is suggested by their procedure.)

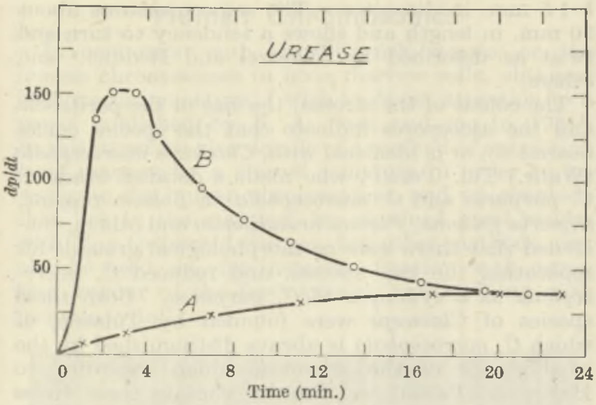
Sodium pyruvate was acted upon by a carboxylase solution in the presence of carbonic anhydrase provided by some laked rabbit blood, the whole at a *p*H of 5·5. The course of the carbon dioxide evolution was followed manometrically at 37° C. The experiment was repeated with cyanide-treated blood (to inhibit the carbonic anhydrase), the *p*H of the mixture being also 5·5. The conditions were as follows: 1 c.c. of a yeast carboxylase preparation; 0·2 c.c. laked rabbit blood (1 in 12·5, with total carbon dioxide removed); 0·5 c.c. 20 per cent sodium pyruvate, *p*H 5·5, added from side tube after equilibration. Similar experiments were carried out in which a urease preparation (buffered with phosphate) was substituted for carboxylase and 5 per cent urea for sodium pyruvate, the *p*H of the whole being 6·8.

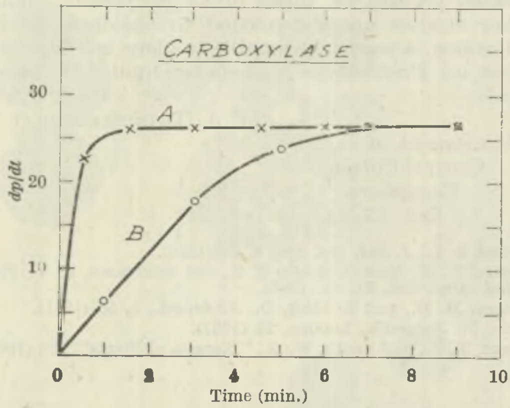
In following the course of the evolution of carbon dioxide in the Warburg manometer, when the height of the fluid in the free limb of the manometer (the other being adjusted to the usual 150 mm. mark) reached 300 mm., the vessel was immediately connected momentarily with the atmosphere, and the levels rapidly adjusted to 150 mm., the experiment being then continued as before, and the pressure developed (in mm.) added to the previous sum. This procedure no doubt introduces some error, but of a kind that has no significance for the question examined. From the data obtained the derivative dp/dt was plotted against the time, with the results shown in the accompanying graphs.

The urease curves show the same features as given by Roughton[1], namely, an initial rapid development of pressure contrasted with a smooth movement towards a steady state in the presence of active carbonic anhydrase. The picture with carboxylase shows essentially the opposite features, a rapid pressure development with active carbonic anhydrase, and a comparatively slow rise of the curve in its absence.

Theoretically, the picture with carboxylase (producing carbonic acid) should not be the exact opposite of that with urease (producing carbon dioxide). With the former an over-fling of the upper curve would not be anticipated, and does not in fact occur.

The results show that carboxylase forms carbonic acid directly, and not free carbon dioxide. This conclusion is supported by the fact that the total carbon dioxide in the suspending fluid of a fermenting yeast mixture (with high proportion of yeast) is far higher





A, WITH *active* CARBONIC ANHYDRASE ; *B*, WITH *inactive* CARBONIC ANHYDRASE.

than could be accounted for by free carbon dioxide or bicarbonate ions.

With the formation of carbonic acid by yeast carboxylase acting on pyruvic acid, it is very probable that it is also formed by similar ferments acting in animal tissues. This would affect rather much the detailed picture of carbon dioxide passage from tissues to blood as hitherto given.

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[1] Roughton, F. J. W., *Physiol. Rev.*, 15, 263 (1935).

Packing of Regular (Spherical) and Irregular Particles

MESSRS. BROWN AND HAWKSLEY have stated[1] that the figures given by Mr. Ackermann[2] are apparently incorrect. As these figures were obtained by Mr. Ackermann and myself working in conjunction, I should like to state their exact meaning.

It is stated by Messrs. Brown and Hawksley that the voidage is independent of the size of the container, provided its shape and size are such that it will contain an integral number of unit cells of the particular ordered arrangement. The figures given by Mr. Ackermann deal precisely with cases where the container does *not* contain an integral number of units, but nevertheless the spheres are packed in an ordered fashion, and this is rhombohedral (each touching twelve), but not necessarily entirely so. He deals with cases where the diameter of the sphere is an aliquot part of the side of the cube, say $1/n$th. The spheres are then packed in alternate layers of n^2 and $(n-1)^2$ spheres, which in the case of a cube will normally leave a space at the top which cannot be utilized. In some cases, however, the space left is sufficient to convert an $(n-1)^2$ layer into an n^2 layer, and in this case the rhombohedral packing is not adhered to for this layer. The question that we were dealing with was the maximum number of spheres (the diameter being an aliquot part of the side) which could be packed into a cubical box by an ordered arrangement. I append a more detailed list of the figures obtained by Mr. Ackermann and myself.

Max. number of spheres in a row $= n$	Total number of spheres in box	Voidage
5	132	0·448
6	244	0·411
7	402	0·386
8	580	0·407
9	870	0·378
10	1,205	0·369
20	10,332	0·321
30	36,561	0·292
40	87,388	0·285
50	171,535	0·282
100	1,396,070	0·269

In the case where $n = 5, 7, 8, 10, 20$, it is found possible to change an $(n-1)^2$ layer into an n^2 layer.

The limiting voidage when the particles become infinitely small is $1 - \frac{\pi\sqrt{2}}{6} = 0{\cdot}2595$.

Since Messrs. Brown and Hawksley state that spheres when poured into a box take up a disordered arrangement with a voidage between 0·45 and 0·37, it will be seen from the above table that there is considerable advantage in the packing suggested. I do not think that any other ordered arrangement will give a greater number of spheres in a cubical box than is shown by the above figures.

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[1] *Nature*, 156, 421 (1945).
[2] *Nature*, 155, 82 (1945).

Mode of Entry of Contact Insecticides

Potts and Vanderplank[1] deduce a relation between the size of the pulvilli and the speed of action of contact insecticides. This would follow, as, to kill an insect, a lethal dose of the insecticide is required to be transferred to it. The larger the pulvilli, the larger the dose of insecticide the insect will collect when it alights on a treated surface. The speed of action of various modern synthetic insecticides, however, shows wide variation.

In some experiments with various types of animal lice (*Hœmatopinus suis*, *Trichodectes latus*, etc.), insecticidal wax sticks were made up containing 10 per cent of insecticide in a mixture of beeswax, paraffin wax and soft white paraffin. By wiping pieces of khaki serge very lightly with the sticks, a thin film of the material was transferred to the fibres of the serge in the form of fine sheaths of the waxy compound around the individual fibres. The pieces of serge were then fastened over the top of glass jars and belled inwards. The insects were introduced and prevented from straying by pieces of transparent cellulose fastened over the mouth of the jar. Lice came into contact with the insecticidal wax through the tarsal claws hooking around the sheathed fibres. When *n*-carbitol thiocyanate was used, the lice were dead in six minutes, whereas with D.D.T. the lice were not dead until the lapse of 120 minutes.

In these experiments, contact between the insects and the insecticides was almost entirely through the medium of the tarsi; and as, in these insects, the pulvillus is but little developed, it is unlikely that the relative size of the pulvillus has any special significance except as mentioned above.

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Aug. 20.

[1] *Nature*, **156**, 112 (1945).

Ergot on *Pennisetum Hohenackeri* Hochst.

Pennisetum Hohenackeri Hochst. (*P. alopecuros* Nees) is a densely tufted perennial grass thriving in the clayey moist situations. The occurrence of sphacelial and sclerotial stages of a *Claviceps* on this host was first recorded by Ajrekar[1] in the course of his studies on the sugary disease of jowar in India caused by *Sphacelia Sorghi* McRae. In their studies on the ergot in South India, Thomas *et al.*[2] have given measurements of the spores of the *Sphacelia* and of the sclerotia. The honey dew forms a brownish viscid crust on the infected spikelets, embedding numerous spores. The spores are hyaline, thin-walled, 17–24 × 3–7 μ.

The ergot and sphacelial stages on *Pennisetum Hohenackeri* were collected round about Bangalore during the months of February and March. The collection of the sclerotial stages is rendered difficult at later periods on account of their dispersal by wind in the same manner as normal seeds. The persistent bristly glumes help in the process of dispersal. The identity of the *Claviceps* species has so far remained obscure on account of the lack of germination stages.

For germination, well-developed mature sclerotia were taken after dissecting out the enveloping glumes. These were wrapped up in a wire gauze and buried in dry soil placed in a pot in the greenhouse. Care was taken to prevent any extraneous moisture from soaking into the pot. The sclerotia were taken out after a month, treated with 0·5 per cent solution of potassium permanganate, washed in water and placed buried in sterilized moist sand in petri dishes. The germination of sclerotia was noticed after 20–30 days, the first indication being the rupture of the cortex of the sclerotium and the extrusion of a white globose head. This ascigerous sphæridium enlarges in diameter and is pushed upwards by the developing stipe or stem. In mature stages the ascigerous head portion is maroon red with a pinkish tinge, the stipe being pure white in colour. The surface of the sphæridium is papillate on account of the protrusion of the apices of the ostioles, and measures about 1–1·5 mm. in diameter. The stipe measures about 20 mm. in length and shows a tendency to turn and twist as described by Whetzel and Reddick[3] and others.

The colour of the stroma, the size of the perithecia and the ascospores indicate that the species comes nearest to, or is identical with, *Claviceps microcephala* (Wallr.) Tul. Petch[4], who made a detailed study of *C. purpurea* and *C. microcephala* on *Lolium perenne*, *Glyceria fluitans*, *Festuca arundinacea* and others, concluded that there were no morphological grounds for separating the two species, and reduced *C. microcephala* as a synonym of *C. purpurea*. Both these species of *Claviceps* were founded by Tulasne, of which *C. microcephala* is always distinguished by the smaller size of the sclerotia, which according to Hartwich (Tubeuf and Smith[5]) contain three times as much ergotin as those of *C. purpurea*. While further studies are yet needed to validate Petch's conclusions, it seems reasonable to place the *Claviceps* species on *Pennisetum Hohenackeri* under *C. microcephala*.

M. J. Thirumalachar.

Department of Botany,
Central College,
Bangalore.

Oct. 17.

[1] Ajrekar, S. L., *J. Ind. Bot. Soc.*, 5, 55 (1926).
[2] Thomas, K. M., Ramakrishnan, T. S., and Srinivasan, K. V., *Proc. Ind. Acad. Sci.*, 21, 93 (1945).
[3] Whetzel, H. H., and Reddick, D., *Phytopath.*, 1, 50 (1911).
[4] Petch, T., *Naturalist*, London, 25 (1937).
[5] Tubeuf, K. F., and Smith, W. G., "Diseases of Plants", 194 (1897).

Classification and Nomenclature of Animal Behaviour

It was suggested in a recent communication[1] that kineses be designated as positive and negative, as is done with taxes. The arguments advanced against the use of 'high' and 'low' for kineses still stand, but after discussions with other workers and some experience of applying positive and negative to specific cases of kineses, these terms do not appear satisfactory. For the sake of clarity it seems desirable to apply to kineses terms different from those applied to taxes.

In what has been called a 'high' or 'positive' kinesis the situation is summed up by saying there is a direct relation between temperature and activity; in a 'low' or 'negative' kinesis there is an inverse relation between them. The straightforward course is, therefore, to call the former a '*direct* kinesis', and the latter an '*inverse* kinesis'. Thus, when a high stimulation intensity is associated with a high level of activity, or low stimulation with low activity, we are dealing with a direct kinesis; when a high stimulation intensity is associated with a low level of activity, or low stimulation with high activity, we are dealing with an inverse kinesis. Dr. D. L. Gunn, who was among those not entirely satisfied with the proposal to apply positive and negative to kineses[2], informs me that he considers the above new suggestions do meet the case.

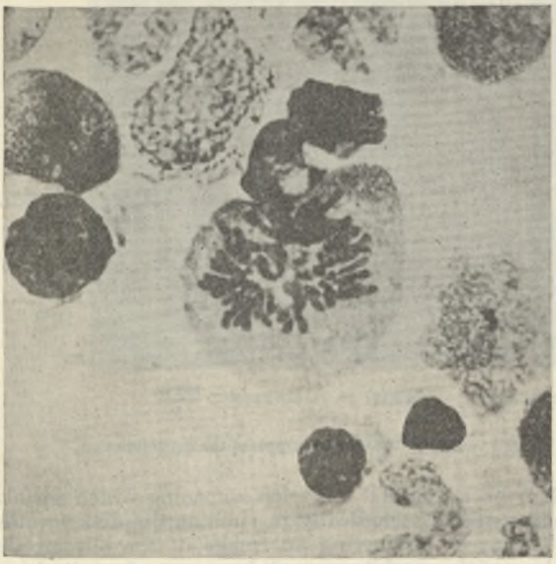
J. S. Kennedy.

Porton,
Salisbury, Wilts.

[1] Kennedy, J. S., *Nature*, 155, 178 (1945).
[2] Gunn, D. L., *Nature*, 155, 178 (1945).

Human Chromosomes

IN connexion with Dr. Śliżyński's note[1] on the human chromosomes in bone marrow cells, obtained by a sternal puncture, I wish to direct attention to a paper published by F. A. Saez and me in 1934[2]. In this paper we give details of a method of extracting bone marrow by a sternal puncture in order to investigate the human chromosomes, and we conclude that "as to the staining, we obtained good results with May-Grünwald-Giemsa, but other methods may also be used, as—for instance—fixation with osmic acid vapour of the fresh smears, and staining with hæmatoxylin".



Reproduced herewith is a copy of a photomicrograph of our preparation, made in 1934.

M. E. VARELA.

Faculty of Medicine,
Institute of General Anatomy
and Embryology,
University of Buenos Aires.
June 21.

[1] Śliżyński, B. M., *Nature*, 155, 427 (1945).
[2] Varela, M. E., and Saez, F. A., *Rev. Soc. Argent. Biol.*, 10, 421 (1934).

Duration of Life of Woodlice

SOME little time ago I published a note on the duration of life of certain species of woodlice[1] which had been studied by Major Stanley S. Flower and me, and it was there recorded that Major Flower had kept a specimen of *Oniscus asellus* Linn. (born in captivity) for approximately 4 years 8 months and 28 days or possibly 4 years 9 months 20 days. My own record for this same species was 4 years 3 months 2 days.

Major Flower has now very kindly sent me a female specimen of this species, 18·5 mm. in length, which was born in captivity between June 13, 1940, and August 31, 1940, and died on August 16, 1945. This specimen was thus certainly 4 years 11 months 16 days old and possibly 5 years 2 months 3 days. Apart from the record for *Platyarthrus hoffmanuseggi* Braudt[1] which I kept alive, in captivity, for 5 years 2 months, these constitute the only records I know of where these isopods lived for more than five years.

A further interesting point in Major Flower's letter is the following. Speaking of the above-mentioned specimen he writes, "I have looked at it every day and on August 15, 1945, it appeared to be quite healthy . . . and it was very tame and appeared to enjoy my stroking it with a finger".

WALTER E. COLLINGE.

The Hollies,
Fulford Road, York.

[1] *N. West. Nat.*, 19, 113 (1944).

Steam Tables and Steam Power

IN a recent review of "The 1939 Callendar Steam Tables"[1], Dr. H. Heywood concludes by saying that "these tables . . . should meet the requirements for many years of engineers concerned with the design and testing of steam power plant or of heating and ventilating systems". In the light of recent advances in technology and metallurgy this may be thought to be an optimistic forecast. Boiler plant design during fifty years has been consistently ahead of the accurate thermodynamic data upon which it should be based, and the 1939 steam tables can only be said to have caught up with modern practice, leaving little margin for further development in the direction of improved power-cycles.

In our view there are important theoretical considerations which point to an immediate need for extending the data into the unexplored area of the temperature-entropy plane bounded by the temperature range 600°–1,600° F. and the entropy range 1–1·5 B.Th.U. per lb. deg. Fahr. Thus, although a direct extrapolation of the enthalpy data gives no indication of any change in the trend of the pressure-enthalpy relation in this region, the corresponding data calculated from a reduced equation of state indicate the occurrence of a pronounced minimum. The van der Waals' equation, for example, leads to a minimum value of enthalpy when

$$\frac{8t_r}{(3v_r-1)^2} - \frac{6}{v_r^2} = 0;$$

which for steam gives the following values of volume and pressure at three selected temperatures:

Temperature (° F.)	Volume (cub. ft./lb.)	Pressure (lb./sq. in.)
980	0·029	14,000
1340	0·032	19,500
1700	0·035	29,500

The accuracy to be expected from any simple equation of the form $p_r = f(t_r, v_r)$ in the region delineated is, however, not great, and from the point of view of defining conditions for maximum theoretical efficiency direct observations are required.

There should be no insuperable experimental difficulties in exploring this region, since although high pressures are involved the corresponding specific volumes are small. Moreover, the advantages to be derived from the possibility of passing, without phase separation, from supercooled liquid to superheated vapour are considerable, particularly in respect of heat transfer.

D. M. NEWITT.
N. R. KULOOR.

Imperial College of Science and Technology,
London, S.W.7.

[1] *Nature*, 156, 462 (1945).

A PHOTO-ELECTRIC FOURIER TRANSFORMER

By Prof. MAX BORN, F.R.S., Dr. R. FÜRTH, and R. W. PRINGLE

Department of Mathematical Physics, University of Edinburgh

THE 'Fourier transform' $g(y)$ of a function $f(x)$, usually defined by the integral

$$g(y) = \frac{1}{\sqrt{2\pi}} \int_a^b f(x) e^{-iyx}\, dx, \quad . \quad . \quad . \quad (1)$$

plays an important part in many problems of pure and applied physics. It represents, for example, the connexion between the intensity distribution of a wave scattered by matter of a certain density distribution, which has to be calculated in a number of acoustical and optical problems and, above all, in X-ray crystal analysis work. It further allows the resolution of a complicated oscillation into a continuous frequency spectrum of harmonic oscillations, which is required in many problems of mechanical and electrical engineering. It therefore seems of some importance to have an instrument by which the Fourier transform of a given function can be automatically and quickly produced. We have now succeeded in building up an instrument which produces the graph of the function

$$g'(y) = \int_a^b f(x) \cos (yx + \delta) dx \quad . \quad . \quad . \quad (2)$$

on the screen of a cathode ray oscillograph, from a mask cut out of black paper in the shape of the graph of the function $f(x)$, or from a record of this function on a plate or film in density variation. Obviously two of the functions $g'(y)$ for two values of δ, say, $\delta = 0$ and $\delta = \pi/2$, are equivalent to the complex function $g(y)$ when $f(x) = 0$ for $x < a$, $x > b$.

The method is a photo-electric one, based on a similar principle to that used by Montgomery for discrete Fourier analysis, but extended for continuous Fourier analysis, that is, Fourier integration. A pattern of parallel fringes, with the light intensity varying according to a harmonic function in a direction normal to the fringes, is projected on the mask (or record) mentioned above representing the given function $f(x)$, and the transmitted light is concentrated on the cathode of an electron-multiplier vacuum photocell. The anode current through the cell will then obviously be proportional to the right-hand side of the expression (2) for a given spacing $2\pi/y$ and setting δ of the fringe system. y is now made to vary periodically in time, that is, the fringe system is made to extend and shrink periodically in the following way.

A glass disk, carrying a photographically produced fringe pattern in which the transparency varies sinoidally, is rotated about an axis perpendicular to its own plane. It can be set so as to let the axis intersect the pattern either at a point of minimum density ($\delta = 0$), or maximum density ($\delta = \pi$), or half-way between two such points ($\delta = \pi/2$), or for any other phase required. The disk is evenly illuminated with light from a projector lamp, and a real image of the fringes is produced on an opaque screen with a narrow adjustable slit in it, the axis of rotation of the image passing precisely through the centre of the slit. The light intensity along the slit will then be a sinoidal one with a spacing which shrinks and extends periodically in time as the disk rotates. Finally, by projecting an image of the slit on the plane of the mask (or record) by means of a cylindrical lens, with its cylinder axis at right angles to the slit, the optical arrangement is completed.

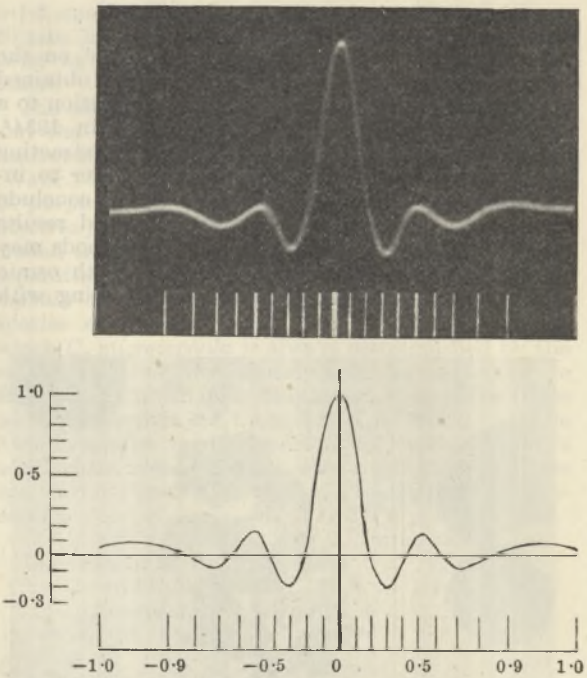


Fig. 1. Above: Record of the Fourier transform

$$g'(y) = \int_0^a C \cos yx dx \simeq \frac{\sin ay}{y}$$

Below: Calculated graph of the same function.

In order to produce the graph of the Fourier transform, the voltage across a resistance in the anode circuit of the photocell is transferred to a cathode ray oscillograph with which is incorporated a two-stage linear amplifier, and a harmonic time base is supplied, synchronized to the rotation of the disk. As the replacement of one mask (or record) by another one, the setting of the phase, and the photographing of the trace take only a couple of minutes, the new method is apparently capable of producing the required Fourier transforms in a minimum of time.

Figs. 1 and 2 show some examples of the performance of the instrument. The records were produced by using the incorporated linear time base of the oscillograph, instead of a harmonic time base, which has the effect of making the horizontal scale of the graphs non-linear. The correct scale has been inserted at the bottom of each graph. Fig. 1 shows (above) the actual record of the Fourier transform $g'(y) = \int_0^a C \cos yx dx \simeq \frac{\sin ay}{y}$ and (below) the calculated graph of this function, and gives an idea of the accuracy of the instrument. Fig. 2 shows the records of four other Fourier transforms. We have satisfied ourselves also that in these and many other cases the curves obtained coincide with the calculated ones within the limits of accuracy of the present instrument.

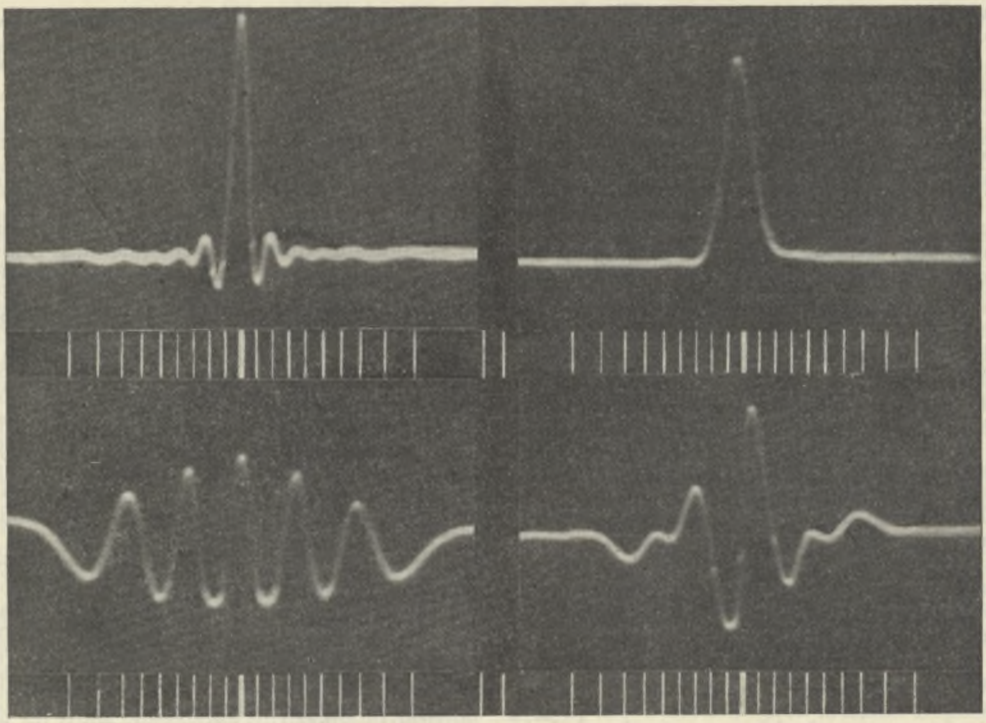


Fig. 2. Records of the Fourier transforms:

(a) $g'(y) = \int_{-A}^{+A} C \cos yx dx \simeq \frac{\sin Ay}{y}$; $(A = 2 \cdot 3a)$;

(b) $g'(y) = \int_{-A}^{+A} e^{-bx^2/2} \cos yx dx \sim e^{-y^2/2b}$;

(c) $g'(y) = \int_{a}^{a+\varepsilon} C \cos yx dx \sim \frac{\cos ay \,.\, \sin \varepsilon y/2}{y}$ $(\varepsilon = a/b)$;

(d) $g'(y) = \int_{a/2}^{3a/2} C \sin yx dx \simeq \frac{\cos ay/2 - \cos 3ay/2.}{y}$

There is little doubt that further technical development of the instrument will make possible the production of Fourier transforms with any reasonably desired accuracy. A detailed description of the new method will shortly be given elsewhere.

THE NEWCOMEN SOCIETY

THE jubilee dinner of the Newcomen Society held at Kettner's Restaurant, Romilly Street, W.1, on November 14, when a presentation was made to Dr. H. W. Dickinson, the chief guest of the evening, for his twenty-five years work for the Society, will long be remembered by those present. It was on November 5, 1920, in the Court Room of H.M. Patent Office, that the Society held its first general meeting, and Dr. Dickinson has ever since been an honorary secretary, except for the two years of his presidency. During this period he has contributed many papers to the Proceedings, arranged all the summer meetings and edited the whole of the *Transactions*, while the contacts he made during his visits to the United States in 1923 and 1938 had much to do with the rapid growth of the Society there, a growth which has led to the formation of a North American Branch as an autonomous affiliated society with a membership exceeding five thousand.

The Society came into existence through a chance conversation between Mr. A. Titley and the late Mr. J. W. Hall on September 16, 1919, at the garden party in the grounds of Heathfield Hall, held during the Birmingham commemoration of the centenary of the death of James Watt. This gathering had attracted many engineers with a love of history, and the suggestion of Mr. Titley to the effect that a society should be formed for furthering the study of the history of engineering and technology at once gained support. By May 1920 a provisional committee had been formed at Birmingham, but the Society may be said to have made its debut at a meeting a month later, on June 4, in the offices of Mr. L. St. L. Pendred, editor of the *Engineer*, at 33 Norfolk Street, London, and at a dinner held the same evening at the Savage Club, then in the Adelphi. At those gatherings Mr. Titley was chosen as president, Dr. (then Mr.) Dickinson, honorary secretary, while Messrs. Pendred, Rhys Jenkins and A. W. Hulme were to act as the London committee to collaborate with the Birmingham committee. After much discussion the question of a name for the Society was happily solved by the adoption of the surname of 'the father of the steam engine', Thomas Newcomen, of Dartmouth. Though its membership was small, the Society soon got into its stride, and at the meeting on November 5, 1920, Mr. Hulme read his paper entitled "Introduction to the Literature of Historical Engineering to the Year 1640", which forms a good starting point for any student eager to take up technological history. It appears as the first paper in vol. 1 of the *Transactions*, which contains also two of Mr. Rhys Jenkins' many papers to the Society, Mr. Pendred's "The Mystery of

Trevithick's Locomotives", a paper on "The Invention of Roller Drawing in Cotton Spinning" by the late Mr. A Seymour-Jones, and the presidential address of Mr. Titley, who dealt with the aims of the Society and the ways to achieve them.

From the first, membership of the Society has been open to all irrespective of nationality or sex, and the earliest published list contains names of members in the United States, Germany, Switzerland and China. In its second year the Society adopted an emblem, a griffin, looking backward yet going forward, and also a motto due to the late Colonel Kitson Clark, "*Actorum memores simul affectamus agenda*"; freely translated as "Mindful of what has been done, we strive at the same time after the things yet to be done". It was this idea that ran through some of the speeches at the dinner recently held, Dr. Dickinson urging the need for an influx of young and enthusiastic members to continue a work which is all the more necessary because its significance is not always appreciated.

The dinner had been preceded by the annual general meeting held at the Science Museum, when the twenty-fourth annual report of the Council and two papers were read. The report referred to the renewal of contacts with members abroad who had suffered under the Nazi tyranny, and to the formation of a Midland branch with its headquarters in Birmingham. During the War, a few meetings had to be abandoned; but all the contributions as set down in the programmes had been received and their publication in the *Transactions* only awaits the release of printers and paper. After the passing of the accounts and the re-election of Mr. S. B. Hamilton as president for a second year, a paper followed by Mr. Greville Bathe, of the United States, on "The Onandaga Salt Works of the New York State, 1646–1846". This in turn was succeeded by a review of the work of the Society during the last quarter of a century by the president. The Society, he said, had striven to achieve its objects in many ways: by enlisting the help of individuals and institutions in preserving objects of historical value, by arousing interest in local industrial history by its summer meetings, by the researches of its own members and by the publication of memoirs, by collaboration with societies having somewhat similar aims, by the compilation of bibliographies and by other means. He had classified the 358 papers contributed under the several headings of biography, ancient engineering, local industries, mills, mining, iron and steel, transport, tools, engines and the like, and he remarked that even if the Society were dissolved now, the twenty-one volumes of *Transactions* published, and the four held in suspense by inevitable delays, "are a unique contribution to historical literature". Many of the papers, he said, "are authoritative compilations by outstanding authorities who have made their respective fields peculiarly their own." As the Society has no house of its own, and meeting grounds were often chosen at places appropriate to the paper to be given, Mr. Hamilton concluded his review with an expression of thanks to the director of the Science Museum and to all those who had generously extended to the Society the use of their rooms for gatherings. In conclusion, it may be noted that besides Mr. Hamilton, the officers for the current year are Dr. H. W. Dickinson and Mr. A. Stowers, honorary joint secretaries, Mr. J. Foster Petree, honorary treasurer, with Miss G. Bingham, O.B.E., 43 King's Road, Chelsea, S.W.3, as assistant secretary.

SCIENTIFIC RESEARCH IN INDIA AND THE BRITISH COLONIES

THE first of a series of discussions on the organization of scientific research in the British Commonwealth, arranged by the Society for Visiting Scientists, was held at the Society's House at 5 Old Burlington Street, London, on December 5, with Prof. A. V. Hill in the chair. The speakers were: Dr. S. Siddiqui, director of the Chemical Laboratories C.S.I.R., Delhi; Dr. J. L. Simonsen, director of the Colonial Products Research Council; Dr. E. B. Worthington, secretary of the Freshwater Biological Association, and author of the section on "Science in Africa" in Lord Hailey's "African Survey"; Major-General Sir John Taylor, late director of the Central Research Institute, Punjab. Those who took part in the subsequent discussion included: Dr. W. R. Aykroyd, director of the Nutrition Laboratories, Coonoor, Central India; Sir Lewis Fermor, late director of the Geological Survey, India; Mr. C. Y. Carstairs, assistant secretary at the Colonial Office; Dr. C. H. Waddington, and Dr. C. Gordon.

Among the chief points made were the following: In India, government expenditure on research now amounts to the modest total of about £200,000 per annum, but the Council of Scientific and Industrial Research, which was founded in 1942, has drawn up a five-year plan for research, to be supervised by a proposed new National Research Council. The total expenditure under the plan would be £4,500,000, of which a considerable fraction would be found by industry. It would include grants for the training of a large number of urgently needed additional scientific personnel.

The outstanding results of scientific research in India to date have been in the field of tropical medicine. Here the Central Research Institute has similar functions to the Medical Research Council in Britain; through the Nutritional Advisory Committee, special consideration is given to nutritional problems. The Indian Geological Survey, established about a century ago, is expanding its activities, notably in relation to the discovery of new mineral resources and to the development of hydro-electricity.

Both in India and the Colonies, nutrition is of outstanding importance. However, it has been found that it cannot be tackled as an isolated research problem, but requires the study of many subjects, including water supply, soil erosion, malaria, agricultural science, land tenure, supply organization, demography and education.

In the course of the discussion, other examples emerged of problems which could only be properly tackled by this method of co-operating teams, often including students of both the natural and social sciences: for example, public health and water resources, the latter involving geology, soil-science, fisheries, meteorology, etc. The success of this method in operational research during the War was emphasized. Broad ecological research is increasingly necessary in tropical countries, and this too demands the co-operation of specialists from a number of fields.

In addition to the classical methods of research by subjects, and to this development of research and problems by wide teams, the method of research by 'sample areas' has recently been proved to be very effective. This was first tried in three different villages of Cyprus, one in the mountains, one in the foothills, one in the plains, where experts in medicine, forestry, agriculture and water supply are all co-operating. This method has also been used in the Colonies, and its general employment was discussed at the Quebec Conference on Food and Agriculture.

One point brought out during the discussion was the disparity now existing between wealthy and poor territories in regard to expenditure on science and its applications. Thus in Malaya, the expenditure on medical and health services is about 5*s*. a head of population, while in India, it varies from about 2*d*. to 1*s*. according to district.

The need for co-operation between scientific workers in the Dominions and the Colonies was stressed. Here the Empire Scientific Conference, which is to be held in 1946, holds out great promise.

Another need is for the raising of the status of the scientific worker in Colonial government service, so that he can occupy the same sort of position as a research worker in a good university at home. One way in which this could be achieved might be by having in each territory or regional group of territories a director of research, who would also be scientific adviser to the Government concerned, and to whom scientific workers in all fields should have direct access.

One of the most urgent problems in India and the Colonies is the study of industrial processes which can be utilized locally. Thus the conversion of molasses into industrial alcohol could be undertaken in sugar-growing countries, and the setting up of plants for extracting oil from oilseeds in Africa would provide large amounts of cattle food urgently required there. In this connexion, the decision to found a Microbiological Research Station in the West Indies is of great importance.

The discussion proved of great interest in focusing informed opinion on the urgent problems of research in India and the Colonies.

SWEDEN'S WATER-POWER RESOURCES

DURING the War, sixty new power stations were built in Sweden, several of more than 40,000 kW. rating. The production of hydro-electric power in 1944 was 12,417 million kWh., and water-power plants now total about 2,500,000 kW. Further plants totalling about 600,000 kW. will be completed towards 1950, and works have been started for an additional 350,000 kW. The energy utilized in 1944 was 10,573 million kWh., of which 4,428 million kWh. were taken by the large industries for engine power, etc., while the electro-chemical and thermo-electric industries consumed 1,891 million kWh. About 1,040 million kWh. were used for traction purposes and 2,017 million kWh. for domestic purposes, etc. 1,093 million kWh. were used for electric steam-generation at night-time and during non-working hours.

Plants built during the War cost about £17,647,000, and subsequent investments amount to about £3,530,000 a year.

During 1944, plant additions amounted to 280,000 kW. Plants for a further 46,000 kW. will probably be completed before the end of 1945. Among the new power-works to be completed within the next five years or so are those at Hjälta (120,000 kW.), Forsmoforsen (75,000 kW.), and Nämforsen (46,000 kW.), situated on the Ångerman River with the tributary Fax River. One station of 46,000 kW. will be built at Skedvi on the Dalälven River and one of 34,000 kW. on the Ljusnan River.

A new station at the Harsprånget waterfall in the Lule River will have a capacity of 250,000 kW. The Suorva Dam at the Stora Lulevatten, the well-lake of the Lule River, has created a water storage with a maximum capacity of 113,000 million cu. ft. and a regulating height of 19½ ft. for the new station as well as for the existing Porjus plant.

The total Swedish water-power resources which can be exploited economically is estimated to correspond to about 36,000 million kWh., of which one third has been utilized, while the second third is now being gradually taken into use. The last third will, however, be comparatively expensive to develop.

The networks of all the large power-plants are interconnected, and four heavy copper and steel-aluminium transmission lines, stretching from the north of Sweden down through the whole country, constitute the arteries through which a large part of the country's electric energy is being supplied to industries, railways and households.

NON-METALLIC DEPOSITS OF THE U.S.S.R.

A NEW publication by the Academy of Sciences of the U.S.S.R., "Non-metallic Deposits of the U.S.S.R." (published in Russian), is planned on a large and comprehensive scale. Judging by the volume at hand (Vol. 2, 1943) this publication will extend over a number of volumes and greatly exceed a similar work published in four volumes during 1926–29, as the subjects discussed in the present volume cover 449 pages as compared with 96 pages of the previous edition. Volume 2 contains the following articles, each written by a specialist or a group of specialists on the given subject: (1) basalt and diabase, (2) barite and witherite, (3) beryl, (4) turquoise ("Biruza" in Russian), (5) bitumens and bitumenous rocks, and (6) bauxite. Each section comprises the following items: general description of the material and its mode of occurrence and genesis, localities where it is found, especially those in the U.S.S.R., methods of extraction and technology, economic statistics and bibliography.

The value of this work is manifold. First, because it gives a very full account of the localities and mode of occurrence of mineral deposits, especially those found in the Soviet Union, secondly because it provides new and interesting interpretations of data, classification, genesis and novel industrial uses. Such, for example, is the article by A. S. Ginsberg on basalt and diabase (dolerite), rocks which are extremely abundant in Britain and used mainly for road-metal, while in the Soviet Union, at the present time, they are also used for manufacturing electrical insulators, acid-resisting tanks and many other articles of industrial value, in the form of the so-called 'cast basalt'. The section on bitumens, written by a group of specialists, is of exceptional interest, especially the part written by V. A. Uspensky on the genesis and genetic classification of bitumens and also that written by A. P. Vinogradov on the occurrence of certain chemical elements, such as vanadium, nickel and uranium, in bitumens. Sections describing the occurrences of bitumens in the U.S.S.R. and the technology of bituminous products are also very interesting. The prospecting for bauxitic ores in the

U.S.S.R. has been carried out on a very extensive scale during the last two decades, and the collection of articles on this subject provides a valuable summary of the results achieved as it gives detailed accounts of the various new bauxite localities of the Urals, Kazakhstan, Siberia and Central Asia.

On the whole, this publication, of which many more volumes are projected, promises to serve as an authoritative reference book to the mineral resources of the U.S.S.R.; it is of interest to geologists, miners and technologists of all countries, provided that they are familiar with the Russian language.

S. I. TOMKEIEFF.

FORTHCOMING EVENTS

Thursday, December 27

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 2.30 p.m.—Sir Robert Watson-Watt, F.R.S.: "Wireless" (Christmas Lectures adapted to a Juvenile Auditory, 1).

Friday, December 28

INSTITUTE OF WELDING, EAST SCOTLAND BRANCH (at the Heriot-Watt College, Chambers Street, Edinburgh), at 7.30 p.m.—Mr. F. Clark: "Non-Ferrous Welding".

Saturday, December 29

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 2.30 p.m.—Sir Robert Watson-Watt, F.R.S.: "Wireless" (Christmas Lectures adapted to a Juvenile Auditory, 2).

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

ENGINEERING ASSISTANTS (2) in the Borough Engineer's Department—The Town Clerk, Municipal Buildings, West Hartlepool (December 29).

LABORATORY STEWARD at the Swindon Technical Institution and Secondary School—The Director of Education, Education Office, Civic Offices, Swindon (December 29).

MECHANICAL ENGINEER (to organize and supervise the design and manufacture of machinery for use on Peat Bogs)—The Secretary, Turf Development Board, Ltd., 21 Fitzwilliam Square, Dublin (December 31).

TEACHER (full-time) OF ENGINEERING WORKSHOP PRACTICE—The Principal, Twickenham Technical College, Egerton Road, Twickenham, Middx. (December 31).

ASSISTANT MASTER with qualifications in MATHEMATICS, PHYSICS, or ENGINEERING, a LECTURER IN BIOLOGY, and a LECTURER IN MECHANICAL ENGINEERING, in the Denbighshire Technical College—The Director of Education, Education Offices, Ruthin, Denbighshire (January 5).

ENGINEERING ASSISTANT, Grade E, in the Public Works Department—The Borough Engineer and Surveyor, Council House, Walsall, Staffs. (January 7).

CITY ENGINEER AND SURVEYOR—The Town Clerk, Town Clerk's Office, Bradford, endorsed "City Engineer and Surveyor" (January 8).

CIVIL ENGINEER (with special experience in Building), MECHANICAL ENGINEERS (with Works and Design experience), and MECHANICAL DRAUGHTSMEN (with Works Training and experience in Machine Design)—The Secretary, Turf Development Board, Ltd., 21 Fitzwilliam Square, Dublin (January 10).

DIRECTOR OF RESEARCH to take charge of a Research Laboratory to be set up in the Midlands for the British Sugar Corporation—The Ministry of Labour and National Service, Appointments Department, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting F.5532.XA (January 11).

PRINCIPAL OF THE DUNDEE TECHNICAL COLLEGE—The Clerk and Treasurer, Dundee Technical College, Bell Street, Dundee (January 12).

ASSISTANT ENGINEER—The Secretary, Burnham, Dorney and Hitcham Waterworks Company, High Street, Burnham, Bucks. (January 12).

ELECTRICAL ENGINEER AND MANAGER of the Brighouse Electricity Undertaking—The Town Clerk, Town Hall, Brighouse, endorsed 'Electrical Engineer and Manager' (January 16).

TEACHER (full-time) OF STRUCTURAL ENGINEERING SUBJECTS at the Brixton School of Building, Ferndale Road, London, S.W.4—The Education Officer (T.1), County Hall, London, S.E.1 (January 16).

TEACHERS (full-time) OF MECHANICAL ENGINEERING SUBJECTS at the South-East London Technical Institute, Lewisham Way, London, S.E.4—The Education Officer (T.1), County Hall, London, S.E.1 (January 18).

PRINCIPAL OF THE COUNTY TECHNICAL COLLEGE, Dartford, and REGIONAL PRINCIPAL FOR TECHNICAL EDUCATION in the area served by the College—County Education Officer, Kent Education Committee, Springfield, Maidstone (January 23).

PHYSICAL CHEMISTS OR PHYSICISTS in connexion with development of Plastics—The Ministry of Labour and National Service, Appointments Department, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting F.5261.XA (January 25).

LECTURER IN MECHANICAL ENGINEERING (Ref. No. C.2931.A), a LECTURER IN ELECTRICAL ENGINEERING (Ref. No. D.1557.A), a LECTURER IN WIRELESS ENGINEERING (Ref. No. D.1558.A), a LECTURER in INSTRUMENTS TECHNOLOGY (Mechanical) (Ref. No. C.2978.A), and a LECTURER IN INSTRUMENTS TECHNOLOGY (Optical) (Ref. No. C.2979.A), under the Military College of Science, Stoke-on-Trent—The Ministry of Labour and National Service, Appointments Department, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting the appropriate Ref. No. (January 26).

ILLUMINATION ENGINEER to undertake development work on the application of plastics in the lighting field—The Ministry of Labour and National Service, Appointments Department, Technical and Scientific Register, Room 670, York House, Kingsway, London, W.C.2, quoting D.1629.XA (January 28).

SECRETARY TO THE MEDICAL SCHOOL—The Dean, St. Thomas's Hospital Medical School, London, S.E.1 (January 31).

PHYSICAL CHEMIST to evolve polarographic and other methods suitable for the analysis of plants and supervise their routine use, and a PHYSICIST to evolve spectrographic and other methods suitable for the analysis of plants and supervise their routine use—The Secretary, East Malling Research Station, East Malling, Maidstone, Kent (January 31).

LECTURER IN CHEMICAL ENGINEERING in the Department of Oil Engineering and Refining—The Secretary, The University, Edmund Street, Birmingham 3 (February 1).

LECTURER IN THE DEPARTMENT OF LEATHER INDUSTRIES—The Registrar, The University, Leeds 2 (February 28).

LECTURER IN PHYSIOLOGY—The Secretary, Queen's University, Belfast (March 31).

REGIONAL DIRECTOR (West of Scotland) and DIRECTOR OF CENTRAL DEPOT (Western Area) at Glasgow (joint appointment), and a REGIONAL DIRECTOR (South-East Scotland) and DIRECTOR of CENTRAL DEPOT (Eastern Area) (embodying Plasma Drying Unit) at Edinburgh (joint appointment)—The Secretary, Scottish National Blood Transfusion Association, 10 Duke Street, Edinburgh 1 (April 20).

PROFESSOR OF PHYSICS—The Principal, Heriot-Watt College, Edinburgh (April 22).

TECHNICAL ASSISTANT (Hæmatology) (non-resident, male or female) for Clinical Laboratory Work—The General Superintendent and Secretary, Royal Infirmary, Manchester.

ASSISTANT PHYSICIST in the Radiotherapy Department—The House Governor, London Hospital, Mile End, London, E.1.

PROFESSOR OF PHYSIOLOGY in the University of Ceylon—The Secretary, Universities Bureau of the British Empire, c/o University College, Gower Street, London, W.C.1.

LECTURER IN MATHEMATICS in the Natal University College, Pietermaritzburg—The Secretary, Universities Bureau of the British Empire, c/o University College, Gower Street, London, W.C.1.

CURATOR-LIBRARIAN (experienced) by an Oil Company in Iraq, to take charge of literature and fossil collections in geological laboratory—The Ministry of Labour and National Service, London Appointments Office, 1–6 Tavistock Square, London, W.C.1, quoting F.A.158.

BIOCHEMIST OR CHEMICAL ANALYST (graduate, experienced), and HOSPITAL OR LABORATORY TECHNICIANS, for work with nutritional team in Germany—The Director, Oxford Nutrition Survey, 10 Parks Road, Oxford.

LECTURER (full-time) IN AUTOMOBILE ENGINEERING for Day and Evening work—The Principal, Leicester College of Technology and Commerce, The Newarke, Leicester.

RESEARCH ASSOCIATE—The Secretary, National Institute of Economic and Social Research, 53 Romney Street, London, S.W.1.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Freshwater Biological Association of the British Empire. Thirteenth Annual Report for the Year ending 31st March 1945. Pp. 42. (Ambleside: Freshwater Biological Association, 1945.) 1*s*. 6*d*. [69

Ministry of Health: Nurses Salaries Committee. Mental Nurses Sub-Committee. Further Recommendations. Mental Nurses S.C. Notes, No. 4. Pp. 4. (London: H.M. Stationery Office, 1945.) 1*d*. net. [69

The Journal of the Institute of Metals. Vol. 70, 1944. Edited by N. B. Vaughan. Pp. xlvii+614+65 plates. (London: Institute of Metals, 1944.) [69

Metallurgical Abstracts (General and Non-Ferrous). Vol. 11, 1944 (New Series). Edited by N. B. Vaughan. Pp. viii+514. (London: Institute of Metals, 1944.) [69

Lecture on Cement and Concrete. By Dr. F. M. Lea. Pp. 30. (London: Royal Institute of Chemistry, 1945.) [139

Lecture on Chemistry and Clothing. By D. A. Clibbens. (First Dalton Memorial Lecture.) Pp. 16. (London: Royal Institute of Chemistry, 1945.) [139

The Health Services in Austria: Problems of Medical Reconstruction and Rehabilitation. Pp. 40. (London: Association of Austrian Doctors in Great Britain, 1945.) 3*s*. [139

Ministry of Fuel and Power: Committee on Efficient Use of Fuel. Fuel Efficiency Bulletin No. 40: Combined Power and Heating. Fuel Efficiency Bulletin No. 42: The Recovery of Waste Heat from Fuel Gases. (London: Ministry of Fuel and Power, 1945.) [259

Catalogues

A Catalogue of Rare and Important Books on Zoology, including some Important Works on Fisheries and the Fine Ornithological Works of John Gould; Geology, Botany, Agriculture, Forestry, Fruit-Culture and Gardening. (No. 632.) Pp. 92. (London: Bernard Quaritch, Ltd., 1945.) 6*d*.

Priced Catalogue of B.D.H. Laboratory Chemicals and Testing Outfits. Pp. vi+242. (London: The British Drug Houses, Ltd., 1945.)

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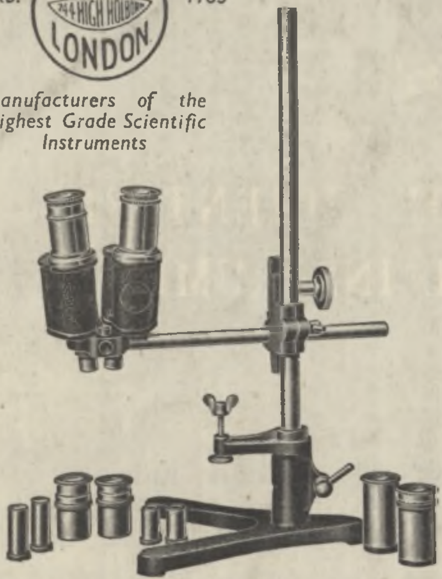

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