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TECHNICAL RESEARCH IN A REHOUSING PROGRAMME

ON the national plane, the present period of great technical advance involves special difficulties which have been well illustrated in recent debates on the place of temporary and permanent houses in our building plans. It is clear that we must be ready to take full advantage of the many technical advances and of any improved methods of construction, but it is more than doubtful whether we are yet ready to decide which methods are likely to be most suitable for our various purposes. This is necessary not only to ensure efficiency, economy and speed in the development of our building programme, but also to ensure that our plans for raising and training a vast labour force take proper account of possible changes in building technique.

In his book "Re-building Britain—a Twenty Year Plan"*, Sir Ernest Simon sets out to delineate the conditions for outstanding success in a rebuilding programme, and, drawing upon his years of experience in Manchester and as an adviser to the Government, he gives a comprehensive survey of the possibilities and difficulties of rebuilding both houses and communities in Britain after the War. The book falls into two parts, dealing with housing and planning, respectively. The first includes a concise account of the achievements and shortcomings of the housing programme of 1919–39 and at the same time indicates the better preparations which have been made by the Government during the present War to organize the production of houses when hostilities cease. But while Sir Ernest brings together many facts and figures not easily obtainable and his book constitutes a useful reference work both in respect of planning and of building and housing, the treatment is somewhat uneven. It can scarcely be maintained that even within the limits he sets himself—Sir Ernest explicitly excludes the problems of the country, some aspects of the building industry, and the general aspect of the rent problem—he deals adequately with all the factors involved.

This reservation applies to many of the questions in which scientific workers themselves are more particularly interested. He notes, it is true, in his appreciation of the inter-war effort, that there was little or no research on the structure or fittings of the house, but his two chapters on technical planning and on prefabrication lay nothing like the stress on the importance of research which is given by Mr. John Madge in "The Rehousing of Britain" in the Target for To-morrow Series†. Mr. Madge points out, notably in a section on "The Gaps in our Knowledge", that energetic research is essential if we are to avoid one of the greatest dangers in the immediate post-war period. Without full knowledge of conditions and of new materials and building technique, we may waste time and effort upon second-rate solutions, even if reconstruction does not cut right across any integrated plan.

* Rebuilding Britain—a Twenty Year Plan. By Sir Ernest Simon. Pp. 256+16 plates. (London : Victor Gollancz, Ltd., 1945.) 6*s*. net.

† The Rehousing of Britain. By John Madge. (Target for To-morrow Series.) Pp. 64. (London : Pilot Press, 1945.) 4*s*. 6*d*.

The greatest gaps are in the field surveys of the physical and economic conditions of our towns and regions, as was stressed by Mr. E. Carter in his paper on "Science and Housing" at the British Association Conference last January. If, in an attempt to alleviate the acute shortage which already exists and which will be accentuated by demobilization and the return of business offices to the towns, new housing and new office block schemes are rushed up in any space available before the new pattern has been determined, the future may be mortgaged to the immediate demand and the emergence of the new plan may be hampered for fifty years or more. This is the reason why it is so important to implement the social survey technique of obtaining housing information described by Dennis Chapman to the Agricultural Science Board in a paper appearing in the *Journal of the Royal Institute of British Architects* last year (51, 191 ; 1944) ; and in his paper on "Science and Housing" in the same journal, Mr. A. M. Chitty shows the part which science has to play in developing the designs which will embody the results of such investigations, whether on the material plane, in securing effective economy of labour, of material and of time in the home, and on the social plane in relating the home to the proper working of the community as a whole.

While there is every indication, as both Mr. Madge and Sir Ernest Simon recognize, that the Government is alive to the need for extended research in the technical field, it is less apparent that the bearing of such research on the labour situation is fully appreciated. Unless due attention is given to this aspect, however, we may find that after ten years or so we have a surplus of craftsmen trained in the traditional methods, and lack the skilled men we need to exploit modern processes. At present, as Mr. Madge points out, the numbers proposed in the Government's plan for the traditional crafts appear to be over-liberal, and the allowance for current and prospective changes in building technique inadequate. Foresight, in fact, is as essential as energy in planning the housing drive, but without adequate research and experiment foresight is impossible. The changes in organization of the Ministry of Works and Building outlined by Mr. Duncan Sandys in the House of Commons on March 23 may assist in repairing this defect, but only if the work of the separate department to undertake research and scientific development, of which Sir Reginald Stradling is to be the head, is effectively co-ordinated with the activities both of the Controller-General's department for building materials and building labour, and of the Director-General's department responsible for all building work, including factory production, transport, erection of temporary houses, etc.

So far as research into the properties and application of materials and techniques is concerned, the need for the future is for more research and, as Mr. Madge insists, for research subordinated to the interests of the consumer rather than designed to push the products of any one firm or group of firms. Mr. Chitty's paper demonstrates the need for more fundamental research, particularly in such fields as thermal insulation and sound, ventilation and lighting, the prevention of moisture penetration, elimination of fire hazards, and the utilization of new materials so as to secure the requisite strength and stability, and the comparatively neglected problems of durability and maintenance, without attention to which decisions regarding obsolescence and a statutory life of buildings, for which Sir Ernest Simon calls, cannot well be made.

Leaving on one side the further question of services, which also require research and development, if the scientific worker is to design for high standards which can effectively be translated into the service of the needs of the mass of the people, the question of standardization becomes important. Mr. Madge suggests that it should be possible for a member of the public to obtain official advice as to the qualities of any particular building material or product ; and restrictions should prevent any manufacturer from squandering national resources by investing labour and materials in unsatisfactory projects. The adoption of standards based on social and technical research, not merely for equipment but also in matters of space, permanence and amenity, would give a new positive basis to by-laws in place of the present restrictive system, and might go far to end the anarchic rule of jerry-building.

A scientific code of standards would also assist in securing the modification of those by-laws which, based on the assumption that only traditional materials are to be used, have seriously retarded the development of legitimate new techniques and materials. Adoption of specification on a basis of performance standards would mean that each material or technique would stand or fall by the vigorous application of scientific tests. The efficiency of traditional building methods and of factory technique would thus be judged solely on their relative fitness for the purpose and would not be subject to irrelevant prejudice. The six-year programme suggested by Mr. Madge, within which the temporary factory-made house programme would be completed, would provide an opportunity for this testing, and the Building Requisites Board suggested by Sir Ernest Simon, taking over the responsibility for research and development from the Ministry of Works, should make a valuable contribution to this essential definition and adoption of standards where a Ministry might be less effective.

But this question of development leads us into other fields also. We have already seen its bearing on the effective use of the available labour supply by its contribution to personnel management and in securing the integration of management and workers through the works committees and in other ways ; but there is a further aspect of even greater importance which is implied rather than discussed in a report on the interrelationship of architecture, engineering and the building and domestic equipment industries which has been prepared by Mr. R. Chapman and Mr. R. Perry at the request of the Engineering Industries Association. The sharing of housing among several industries, which is involved in factory production and the introduction of built-in equipment, is

probably the soundest means of dealing with the ultimate problem of contraction of the labour force which Sir Ernest Simon, for example, notably fails to discuss.

This report to the Engineering Industries Association declares that while the 'Portal house' has been evolved hastily to meet a political need without adequate research into the organizing, technical, economic and town-planning problems involved, it has opened the door to complete factory production and new technique. Based on an existing outline proposal, which by adequate research and development should readily and cheaply permit the evolution of a house without the defects of the Portal house, the report visualizes an annual target of 200,000 factory-made houses and 100,000 composite houses, partly traditional and partly steel frames. Small research groups are suggested to carry out the initial experimental work, and the need for combining the technical and the economic approach is stressed.

The immediate need, according to this report, is for 2,000,000 building workers, and the long-term need for 800,000, both figures being spread over the traditional building trades and the engineering industries. While the advantage of sharing building among several industries is undoubted, the absence of figures in the White Paper and elsewhere showing the relative costs of factory-produced houses built on traditional lines in terms of man-hours, for example, makes it difficult to decide which method to adopt for the immediate programme. The debate in the House of Commons on March 22 and 23 established very clearly the crucial importance of the labour supply, and it is doubtful whether even for our short-term programme we can afford to adopt the factory-produced house if it makes bigger demands on man-power than traditional construction. On this point the admirable account of the demonstration houses and flats erected at Northolt by the Ministry of Works* gives no more information than the White Paper.

Apart from Mr. Horabin, speakers in the debate on housing paid little attention to research. Mr. Horabin, however, stressing the fundamental importance of adequate housing research as a basis for the Government's programme—he is prepared to consider an expenditure of £7,000,000 for research—considers we have no adequate research in this field and urged that it is equally important to have the right people in charge of a department of housing research. They should be men of scientific standards, with engineering experience, and, besides technical research, we need statistical and consumer research. Statistical research is required to get an accurate, qualitative picture of the housing situation, and only through statistical and consumer research can we expect to arrive at the number of one-, two- and three-bedroom houses we require. Moreover, this co-ordinated research is required to reduce the number of man-hours on the site and to ensure that as much as possible of such work is done not only by craftsmen but also by erectors and assemblers.

Mr. Horabin recognizes the vital importance of efficient and enlightened management. Sir Ernest Simon stresses this point also; it is necessary to secure the utmost economy of labour and the whole-hearted co-operation of the workers in the building industries. But it was left for Commander King-Hall to point out that part of the price to be paid for a solution of the housing problem is direction of labour and material, and much better control of land. Mr. Madge in his book, like Sir Ernest Simon, recognizes the necessity of maintaining war-time controls over the costs and priorities of the building industry, and both books bring out this essential point that reconstruction must be within the framework of a national development pattern administered by a strong central planning authority. Rarely, however, is the fact faced frankly that labour, too, must accept restrictions; but it is essential, as Mr. Buchanan pointed out in the House of Commons, that private and sectional interests should be seen to be subordinated to the interests of the nation as a whole. That is a first step, if we are to secure a much more helpful attitude than has yet been apparent in the building trade unions to the changes that are involved in pre-fabrication and other developments coming from building research.

"Rebuilding Britain", however, is not merely a compendium of facts; as indicated by its sub-title, Sir Ernest Simon sets out a definite plan, and this as well as its critical appraisal of the inter-war achievement and of the Government's weaknesses in dealing with the wider issues give additional interest to the book. In regard to the inter-war achievement, Sir Ernest points out that while it took fifteen years to work up to the building of 350,000 houses per annum in the national housing effort as a whole, an entirely new minimum standard of working-class houses was adopted. A second outstanding achievement, which no other great country has approached, was the building during 1919–39 of no less than four million new houses. These achievements should be remembered, although it is admitted that for financial reasons the houses tended to be too small, there was no adequate control over jerry-building, planning of the housing estates tended to be rigid and unimaginative, and the bigger questions of planning were scarcely considered.

Sir Ernest Simon's own twenty-year plan consists of three stages. The first, aiming at a house for every family, would take about five years, with the provision of 400,000 houses in each year from the fourth after the end of hostilities. In his estimate, however, he only provides for 50,000 in the first, and 100,000 in the second year, as against the 220,000 completed houses and 80,000 in course of erection of the Government's White Paper (Cmd. 6609, H.M.S.O., 1945, 2*d.*). The second stage aims at clearing the slums, with four million new houses to be built in ten years for slum clearance and four million cleared houses to be pulled down. This should be completed in sixteen years after the end of the War, and the third stage, involving the clearance and replacement of one million and a half obsolescent houses, should be complete in a further four years.

It may be argued that Sir Ernest deals too summarily with the question of scrapping houses and of

* Demonstration Houses. Pp. 76. (London: H.M. Stationery Office, 1944.) 1*s.* net.

statutory life which P E P analysed last year in a broadsheet entitled "Old Houses". He does not allow in his estimate for the effect of a declining population upon demand, and he does not discuss at all the difficulty of contracting the labour force either on the completion of his twenty years plan or when the demand falls off under the influence of population changes and the altered obsolescence position. In his summary rejection of the Pole Report on "Private Enterprise Housing", he scarcely attempts to deal with the difficulties of sharing housing between municipal and private enterprise—his leaning to municipal housing makes him underestimate the difficulties, and he seems sometimes to forget that Manchester is scarcely a typical local authority and many local authorities have nothing like Manchester's resources or vision.

What Sir Ernest Simon does do in this book, especially in the last two parts, where he surveys first some foreign examples of planning and then the position in Britain, is to show how lamentably weak the Government's policy is in community planning, and how important the Government's failure to deal effectively with the question of the control of land in the national interest and the problem of compensation and betterment may yet prove to be. This is the second of the five conditions he lays down as essential for successful planning: the other four are suitable planning authorities—national, regional and local—adequate finance, proper national and local plans, and the necessary driving force to see that the job is done. But while he leans to national ownership as the only satisfactory solution of the land problem, he regards this as at present politically impracticable, and the examples he cites show what sound community planning can do without public ownership provided the planning powers are adequate.

Although Sir Ernest does not always answer the questions he raises, he puts the whole problem in a sound perspective and his book should assist in building up that intelligent public opinion which must supply the driving force in the successful rebuilding of Britain. The other two conditions are national prosperity and low rate of interest, and the book shows far more clearly than the White Paper how the housing question is related to the problem of full employment. Sir Ernest is optimistic. He recognizes the enthusiasm of the local authorities and the determination of many city councils to rebuild their cities on fine and imaginative lines. These, and the vigorous steps taken by the Government to ensure a strong building industry and to get to work quickly on housing after the War, are encouraging. If public opinion can bring the Government to take the major decisions of policy which are necessary to enable local authorities to go ahead with their planning, and if scientific workers see to it that research in regard to materials and methods at the technical level and the social level in regard to the needs of the home and of the community is prosecuted with vigour, with vision, and with adequate resources, there is no reason to doubt the ability of the country to seize the opportunity which is now arising in building, in housing and in planning.

THE FŒTAL CIRCULATION

The Fœtal Circulation and Cardiovascular System, and the Changes that they Undergo at Birth

By Dr. Alfred E. Barclay, Dr. Kenneth J. Franklin and Marjorie M. L. Prichard. Pp. xvi+275. (Oxford: Blackwell's Scientific Publications, Ltd., 1944.) 50*s*. net.

Of all the fœtal systems, the cardiovascular has been the most provocative of theorizing. Its anatomy is linked with such names as Galen, Fallopius, Vesalius, Harvey, Lower, His, Born and Tandler, its physiology with Galen, Harvey, Méry, Wolff, Haller and Sabatier. As regards the experimental approach, Wilhelm Preyer's masterly work, "Specielle Physiologie des Embryo" (1885), still remains unique.

In this volume Barclay, Franklin and Prichard, with due recognition of their Cambridge colleagues, Barcroft and Barron, present an account of their combined work with emphasis on that part which has been the more direct concern of the Oxford team. The manner in which the problem has been approached and the technical details overcome will command the respect and admiration of all, for, as the authors state, "it consists of an account of our own fœtal studies".

The first part, a historical survey, is followed by an account of the course of the blood through the fœtal heart of the lamb as seen radiographically, including a résumé of the technique and apparatus used. The third part deals with the anatomy and physiology of the circulation in the lamb fœtus, and the neonatal changes. The comparative anatomy and physiology of the fœtal cardiovascular system, mostly original work of the authors, constitute the fourth part of the book. Four chapters on the human cardiovascular system, with the speculations of the authors on the fœtal circulation in man, form the fifth part of the volume, which concludes with a brief summary and anticipations of the period when the work may be extended to the human fœtus.

The contributions of anatomists and physiologists from the time of Galen are recorded and assessed. Attention is again directed to the appropriation of the discovery of the ductus arteriosus and the foramen ovale (both known to Galen) by Botallo, and to the priority, often erroneously accorded to Arantius, for the discovery of the ductus venosus, already known to his teacher, Vesalius. Harvey's contribution is dealt with in great detail. Wolff's work receives a recognition which it duly deserves. He undoubtedly had a clear idea of the anatomical relations of the channels in the fœtal heart and obtained a more accurate insight into the fœtal circulation than any of his predecessors. The technical errors of Kellogg and Pohlmann are discussed, together with the inaccuracies of their interpretations. Among the more recent experimental work on the fœtal circulation the omission of that of Huggett is surprising. Indeed, his name does not appear in the whole volume. The authors credit Kellogg with the first experiments on the fœtus delivered by Cæsarean section, with the placental circulation intact and fœtal respiration inhibited. Huggett's first paper on the tension of the blood gases in the fœtus, with a description of his technique, appeared in 1927 (*J. Physiol.*, 62), antedating Kellogg's first paper (1928, *Amer. J. Anat.*, not *Anat. Rec.*) by more than eighteen months.

Part 2 is devoted to the actual course of the blood in the fœtal lamb as visualized by cine-radiography following the injection of opaque media into the circulation at various critical points. This part of the volume will undoubtedly receive the admiration of all who read it. The description, the twenty-eight figures and the four excellent frames of actual cinematographic records deserve the highest praise. It is the first depiction of the blood flow in the fœtal circulation under conditions approximating to those within the amniotic cavity. The division of the postcaval blood stream on the annulus fossæ ovalis, which the authors prefer to call the crista dividens, is described.

In the chapter on the cardiovascular system of the lamb the authors introduce new functional terms for the older and more established anatomical ones. Whether this change of terminology should be adopted is doubtful. Physiological discoveries must undoubtedly call for a constant revision and extension of terminology, but the term introduced must also be acceptable to those seeking to explain the mechanisms of morphological changes in the embryo and fœtus. For example, the authors suggest the name "umbilical venæ comites" for the paired umbilical veins in the cord, as distinct from umbilical vein for the unpaired structure within the abdomen. The evolution of right and left umbilical veins, primarily paired veins, is disregarded here. In the marsupials, McClure has named the fused umbilical veins within the abdomen the umbilical sinus. Umbilical venæ comites seems scarcely justifiable. On the grounds of priority, the authors dissociate the name of Arantius from the term ductus venosus. The particular channel so admirably described by Arantius in the human and its homologue in other mammals must be distinguished from other channels similarly named. Tribe, in her paper on the hepatic venous system, gives a translation by Thane of the original description by Arantius: "So that if you introduce a probe into the umbilical vein you will arrive in the trunk of the portal vein and then by means of the said communication (*the ductus venosus*) straight into the vena cava". Hill and Tribe prefer to retain the term ductus venosus Arantii for this type of channel, and to use the unqualified term ductus venosus for the channel connecting the vitelline vein with the postcaval vein. There is some justification for retaining the name of Arantius for this type of ductus venosus, if only by virtue of the clarity of the original description in 1564.

The comparative studies dealt with in Part 4 are fragmentary, but extremely valuable and stimulating. Discussion of the placenta is brief and follows the lines of Grosser's classification, the only one of value to both physiologist and embryologist. In many ways the sheep's placenta, despite the classical work of Assheton on the ungulate placenta, may still be termed, like the spleen, *organon plenum mysterium*.

The umbilical cord, despite its apparent simplicity, affords many unsolved problems. Of the smaller blood vessels in the stroma of the cord, the authors say little, and have omitted the observations of Wislocki, who credits Klaatsch with the first description of these vessels. It would appear that man and primates have an avascular stroma, while other mammals possess a capillary plexus. Except for its most proximal segment, capillary channels occur in the primate cord only during the early stages of formation of the paired umbilical arteries and veins. The authors consider that the vessels can scarcely be the vasa vasorum of the umbilical vessels, but do not mention Wislocki's view that they may be the vasa propria of the stroma. Of the blood vessels in the proximal portion of the human cord, they have little to add to that already written; but it is obvious that little is known of the nutrition of the umbilical cord, of the vascular plexus in the proximal segment of the human cord, or of the para-umbilical veins. Of the strength and site of rupture of the cord, it is doubtful if many would subscribe to the views of Shordania, for the site of rupture in the human is generally at or close to the umbilicus. The irritability of the umbilical vessels and their 'valves' are adequately considered so far as existing knowledge permits. One of the valuable features of this section of the book is that attention is forcibly directed to the series of unsolved problems in so simple an organ as the cord.

The territorial distribution of the umbilical and portal veins in the liver during fœtal life follows Mall's classical work on the liver lobule and the rather wider approach of Charnock Bradley. It is difficult to correlate the work that has been done on this aspect of the fœtal liver. It is surprising that the work of Bonne (1904) has not received the attention of the authors, since this was done on the fœtal liver in sheep and his results closely resemble theirs.

The tissue surrounding the 'sphincter' on the ductus venosus is described as 'œdematous'. This word implies a pathological state. The œdematous tissue is the normal loose areolar tissue of the type seen near contractile and expansile organs.

In the light of the authors' radiographic work, their 'via sinistra' (valve of foramen ovale) and 'via dextra' (right moiety of the postcaval channel) are carefully described in sheep and man. Little seems to be known of the functional closure of the via sinistra or of the ductus arteriosus, other than that provided by the work of this team.

The neonatal and postnatal changes in these transient channels have been studied microscopically by many anatomists, but the authors rightly omit these. Observations on the onset of respiration in sheep are discussed with reference to Barcroft's views. Huggett's prior observations on respiration following delivery of the goat fœtus are omitted. The statements about parturition in captive animals are not enlightening. Our knowledge of parturition in primates is meagre, but the position is being clarified by Elder and Yerkes in chimpanzees and by Hartmann in the macaque, reference to whom has been omitted. Of the actual time of birth the authors state, "Almost all primate births occur at night. . . ." Elder and Yerkes state the opposite. Concerning placentophagia in the primates, the experience of Dr. Vevers quoted here seems to be at variance with that of Hartmann and Elder and Yerkes. Hartmann states, "all deciduate mammals except man if left to themselves will eat the afterbirth and this holds for the herbivores". In the one series of chimpanzee births recorded by Elder and Yerkes, seventeen out of twenty-nine ate all or part of the placenta. They state that under natural conditions the percentage of placentophagia would probably have been considerably higher.

In Part 5 the authors attempt "to know better what happens in the human subject". In applying their results in sheep to the human, they have been justifiably cautious but effective. There is much support for the view that the circulation in the human is not dissimilar to that in the sheep. They

look to the time when more direct evidence of the changes occurring at birth in the human cardiovascular system will be available.

The foregoing criticisms must not be allowed to detract from the value of this publication. It is full of information and is the most comprehensive and satisfying account in existence of the fœtal circulation and its neonatal changes. The work done by this team should command the admiration of all interested in the young mammal. The gaps in our knowledge are clearly enunciated and should act as a stimulus to embryologists, obstetricians and physiologists alike. The numerous illustrations are of high quality, the bibliography and index most useful. The authors, publishers and the Nuffield Institute for Medical Research alike deserve credit and thanks for producing so monumental a treatise in war-time.

D. V. DAVIES.

TRAVELS OF A BOTANIST

A Life of Travels

By C. S. Rafinesque. Being a verbatim and literatim reprint of the original and only edition (Philadelphia, 1836). Foreword by Elmer D. Merrill; Critical Index by Francis W. Pennell. (Chronica Botañica, Vol. 8, No. 2.) Pp. 291–360+plates 5–8. (Waltham, Mass.: Chronica Botanica Company; London: Wm. Dawson and Sons, Ltd., 1944.) 2.50 dollars.

RAFINESQUE was a peculiar character who, in the course of a vagrant life of fifty-seven years, worked according to himself as "a Botanist, Naturalist, Geologist, Geographer, Historian, Poet, Philosopher, Philologist, Economist and Philanthropist". But primarily he was a plant taxonomist, whose work, Dr. Merrill suggests in the foreword to this volume, has not received the attention that it deserves.

Rafinesque was born "at Galata, a suburb of Constantinople", in 1783, and died in Philadelphia in 1840. With the exception of a period of ten years in Sicily, most of his adult life was spent in America. He was apparently an indefatigable traveller, who made long laborious journeys "by nearly all the possible manners except by camels and in balloons". He was also an extremely zealous collector, so much so that of one episode he writes: "In wading through Green R. I narrowly escaped drowning, but collected many fine shells". In the intervals between the successive journeys he led a busy life writing books, maintaining an elaborate correspondence with many of the leading naturalists of his day, and earning his living. Unfortunately in this last undertaking he was not very successful. On one occasion some of his collections "remained for a while in store and under a mortgage", and repeatedly in the account of his travels he complains of the duplicity of those who would not recognize his just financial claims. At one time he earned his living as a teacher of Italian, drawing and botany; later he was appointed to a "Professorship of Botany and Natural history, with the addition of modern languages, with lodgings, boarding and casual emoluments"; and later still he became "a Pulmist, who attended only to the diseases of the lungs, as a Dentist attends only to the teeth". And again towards the end of his life he became "the Actuary" for an institution that was born of his own ingenuity, "The Divitial Institution and six per cent Savings Bank".

These details are given in "A Life of Travels", which was first published in Philadelphia in 1836, and which has now been reprinted in this edition. The passages in which they are recorded, however, are only occasional interpolations in an arid recital of places visited and routes followed. Throughout there is little description either of the observations or of the collections made during the several journeys. There are a few incidental comments, but these are not expressed with any particular originality. For example, of himself, Rafinesque writes: "I never was happier than when alone in the woods, or resting near a limpid stream or spring, I enjoyed without control the gifts of Flora, and the beauties of Nature". Nevertheless, the journeys that are described in such minute detail are of considerable interest in relation to the taxonomic work that was based on them, and to that extent the publication of the present volume is fully justified. Evidently this edition has been prepared with considerable care, but since itineraries are a prominent feature of the text, the value of any future edition would be considerably enhanced by the provision of illustrative maps, of which there is none in this.

R. BROWN.

PHOTOGRAPHY OF THE RECIPROCAL LATTICE

The Photography of the Reciprocal Lattice

By M. J. Buerger. (ASXRED Monograph No. 1, published by the American Society for X-Ray and Electron Diffraction.) Pp. ix+37. (Cambridge, Mass.: Murray Printing Co., 1944.) 1.50 dollars.

NOT many years ago an account of single-crystal X-ray diffraction methods would have been considered complete if it had referred only to the Laue, rotation and oscillation methods, with particular emphasis on the last. There might also have been some mention of moving-film methods, such as the Weissenberg. All this is now changed. Moving-film methods bid fair to displace other methods completely, even for the study of polycrystalline aggregates.

Prof. Buerger's book adds another chapter to the use of these methods; he has been able to devise apparatus whereby sections of the reciprocal lattice —the representation of the diffraction pattern of a crystal—can be obtained without distortion. In other words, the indices of the X-ray reflexions can be read off without the use of charts, with a consequent saving of time and an increase in reliability.

The book gives full details of the theory of the apparatus and contains several beautiful examples of photographs taken with its use. The author also gives a very fair estimate of the advantages and disadvantages compared with other methods. In particular, it will not give part of the reciprocal lattice near the origin, in common with all methods except the equi-inclination Weissenberg.

There are a few criticisms that can be made. The author claims that the method is inherently more accurate than most other moving-film methods, but he does not give a clear statement of the accuracy that can be expected from it. Also the title of the book is misleading; all methods that use characteristic radiation give a representation of the reciprocal lattice. These, however, are small points, and the American Society for X-Ray and Electron Diffraction is to be congratulated on the first of the monographs it has produced.

H. LIPSON.

PLANT-GROWTH SUBSTANCES AS SELECTIVE WEED-KILLERS

Differential Effect of Plant-Growth Substances on Plant Species

STUDIES of the effect of growth substances upon plants began at Jealott's Hill Research Station in 1936. The earliest work dealt with the stimulation of the rooting of cuttings and it was extended to discover the effect of ranges of concentrations of β-indolylacetic acid and α-naphthylacetic acid upon seed germination, upon seedling growth and upon the growth of established plants of many species. The results of these experiments have already been reported[1,2], and in them it was observed that the higher concentrations of growth substance depressed growth. This observation, together with our knowledge of the variation of the extent of rooting and deformative effects produced in the different species, led us to expect that these growth substances in higher concentrations would show a selective phytocidal action. We found in August 1940 that applications at the rate of 25 lb. of α-naphthylacetic acid per acre to oats (*Avena sativa* L.) weedy with yellow charlock (*Brassica Sinapis Visiani*) killed the charlock. The oats only received a slight setback and recovered fully. The application was made just after seed-sowing time. 84 per cent of the charlock failed to germinate, and those seedlings which did emerge afterwards died. Several experiments in soil and sand and upon filter paper in the laboratory confirmed these results and indicated that wheat, barley and rye behaved similarly to oats, while greater plaintain (*Plantago major* L.) and yarrow (*Achillea millefolium* L.) were like yellow charlock in their responses.

Thus, we had confirmed our expectation of selective phytocidal action of certain growth substances, for β-indolylacetic acid and α-naphthylacetic acid killed some major weeds in cereal crops without harming the cereals, wheat, oats and barley. We followed up this lead by experimenting with other growth substances. For if growth substances were to be of use to the farmer as selective weed-killers, it was necessary to find one which was cheaper than or much more active than α-naphthylacetic acid. We already knew plant-growth action was not characterized by a close degree of correlation with chemical constitution, and consequently we made a chemical survey of known growth substances and substances structurally related to them. Substituted phenoxyacetic acids and naphthoxyacetic acids were found to be outstanding and certain of them were found to be fifty times as active as α-naphthylacetic acid. The following list shows some of the compounds tested :

Class A : Activity greater than α-naphthylacetic acid.

o-methyl-phenoxyacetic acid (Na salt).
m-methyl-phenoxyacetic acid.
p-methyl-phenoxyacetic acid.
2 : 4-dimethyl-phenoxyacetic acid.
2 : 5-dimethyl-phenoxyacetic acid.
3 : 4-dimethyl-phenoxyacetic acid.
p-chloro-phenoxyacetic acid (Na salt).
2 : 4-dichloro-phenoxyacetic acid.
2 : 5-dichloro-phenoxyacetic acid.
4-chloro-2-methyl-phenoxyacetic acid.
4-chloro-3-methyl-phenoxyacetic acid.
β-naphthoxyacetic acid (Na salt).
2 : 4-dichloro-α-naphthoxyacetic acid (Na salt).
methyl-β-naphthoxyacetate.
propyl(*n*)-β-naphthoxyacetate.
propyl(*iso*)-β-naphthoxyacetate.
butyl(*iso*)-β-naphthoxyacetate.

Class B : Activity approximately equal to α-naphthylacetic acid.

o-chloro-phenoxyacetic acid (Na salt).
2 : 4 : 6-trichloro-phenoxyacetic acid (Na salt).
ar-tetrahydro-β-naphthoxyacetic acid.
β-naphthoxyacetamide.
β-naphthoxyacetonitrile.
ethyl-β-naphthoxyacetate.
butyl(*n*)-β-naphthoxyacetate.
cyclohexyl-β-naphthoxyacetate.

Class C : Some activity.

phenoxyacetic acid.
p-nitro-phenoxyacetic acid (Na salt).
2 : 4-dinitrophenoxyacetic acid (Na salt).
α-naphthoxyacetic acid (Na salt).
1-chloro-2-naphthoxyacetic acid (Na salt).
2-chloro-1-naphthoxyacetic acid (Na salt).
ethyl-α-naphthoxyacetate.

Experiments carried out in November 1941 showed that 4-chloro-2-methyl-phenoxyacetic acid was one of the most active compounds tested, and that it readily depressed the germination and early seedling growth of corn buttercup (*Ranunculus arvensis* L.), fat hen (*Chenopodium album* L.), corn marigold (*Chrysanthemum segetum* L.), corn spurrey (*Spergula arvensis* L.) and field poppy (*Papaver Rhoeas* L.) at concentrations which were without effect on cereals. Pot and field experiments showed that germination of yellow charlock was suppressed and plants at any stage from seedling to flowering were readily killed by spraying with solutions of sodium 4-chloro-2-methyl-phenoxyacetate supplying only 10 mgm. per square foot (equivalent to approximately 1 lb. per acre) of active principle. The affected plants showed marked epinasty and died slowly. The substance appears to persist in the soil for several weeks. Work is at present in progress to extend our knowledge of this effect and to gather information on the underlying causes. In view of the very low concentrations of these substances which bring about such marked differences, it is possible that such materials present or produced in soils may have big influences on the nature and composition of the plant associations growing upon them.

In November 1942 we communicated all our knowledge on selective weed-killers to the late Dr. W. W. C. Topley, secretary of the Agricultural Research Council, and from that date we have co-operated with Dr. J. H. Quastel, Dr. H. G. Thornton, Dr. P. S. Nuttman and Mr. G. E. Blackman.

In 1943 a field trial showed that sodium 4-chloro-2-methyl-phenoxyacetate applied at 1 lb./acre gave 100 per cent eradication of yellow charlock in spring oats without damaging the cereal, and equally good results were obtained if application was made when the weeds were small or when they were in full flower.

We carried out a further programme of field experiments in 1944 which have confirmed and amplified the earlier results. Yellow charlock (*Brassica Sinapsis Visiani*), white charlock (*Raphanus Raphanistrum* L.), corn buttercup (*Ranunculus arvensis* L.) and pennycress (*Thlaspi arvense* L.) have all been eradicated from cereal crops by 1 lb./acre applied as a spray in 100 gallons/acre of water. If the application was made in a dry form using china clay as a diluent, 2 lb./acre of active principle gave similar results with these weeds. There are differences in the sensitivity of weeds ; for example, mayweed (*Anthemis Cotula* L.) appears to be somewhat resistant. It is evident that sodium 4-chloro-2-methyl-phenoxyacetate is effective whether absorbed through the root or leaf of the weed and can be used to prevent seed germination of the weeds specified above as well as to destroy established plants. This material is also non-corrosive. Further research is being done

to elucidate the usefulness of this material for a variety of weeds and crops.

Full details of this work are being published elsewhere.

Suggestions that synthetic growth substances might be used as selective weed-killers have been made recently and independently by Beal[3], Mitchell and Hamner[4] and by Hamner and Tukey[5].

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Inhibition of Plant Growth by 2:4-Dichlorophenoxyacetic Acid and other Plant-Growth Substances

THE work on this subject began at Rothamsted in November 1941 as a result of the previous study of the deformation of legume root hairs made by H. K. Chen, which indicated that the curling of root hairs by *Rhizobium* was due to the secretion by the latter of β-indolylacetic acid. Our first experiments were intended to discover whether a similar deformation of root hairs could be induced by β-indolylacetic acid itself and by related plant stimulants.

In these and in subsequent experiments, where agar was used, a basal medium was employed having the following percentage composition: K_2HPO_4, 0·05; KH_2PO_4, 0·05; $Ca_3(PO_4)_2$, 2·0; $FePO_4$, 0·5; $MgSO_4.7H_2O$, 0·02; NaCl, 0·01; $FeCl_3$, 0·001; KNO_3, 0·1; agar, 1·5. This medium was put up in test tubes, sterilized, and appropriate dilutions of the compounds to be tested were separately sterilized and added aseptically to the melted medium. Slopes were then made and two seeds of the test plant, externally sterilized by successive immersion in 80 per cent alcohol and 0·2 per cent mercuric chloride and washed in sterile water, were placed at the top of the slope in each tube.

In our early experiments we found that β-indolylacetic acid and α-naphthylacetic acid both produced marked root-hair deformation in the agar cultures of red clover, in the absence of *Rhizobium*. But it was also found that both compounds were toxic to germination and to subsequent growth at dilutions up to 1 in 10,000,000. Results of a typical experiment are given in Table 1.

In view of the possibility that tryptophane may be the parent source of β-indolylacetic acid produced by *Rhizobium*, the toxic action of tryptophane itself was tested. Results showed that, in sterile agar medium, tryptophane exerted a toxic action to red clover at concentrations of more than 10 parts per million, but that where *Rhizobium* was also present, the limit of toxicity was lowered to 1 part per million. These results support Thimann's[1] and Chen's[2] evidence, indicating that this organism is able to produce β-indolylacetic acid from tryptophane.

TABLE 1. TOXIC ACTION OF GROWTH SUBSTANCES ON RED CLOVER.

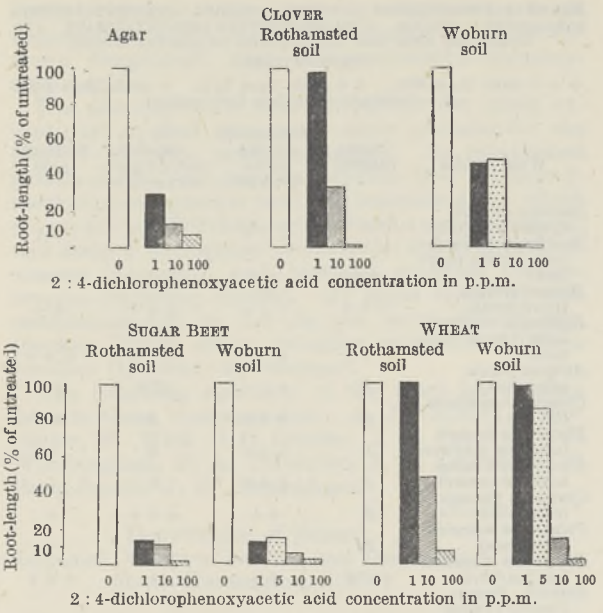
Compound	Concentration in p.p.m.	Total seeds sown	Germination affected	Subsequent growth affected	Normal seedlings
β-Indolylacetic acid	100	15	15	0	0
	10	15	12	2	1
	1	15	6	4	5
	0·1	12	0	11	1
α-Naphthylacetic acid	100	15	15	0	0
	10	15	15	0	0
	1	15	5	9	1
	0·1	15	4	8	3
Control	0	15	2	0	13

The high toxicity of β-indolylacetic acid and of α-naphthylacetic acid suggested the possibility of using such compounds for controlling plant-growth. We therefore made an experiment to determine whether the above compounds were equally toxic to other plants and whether toxicity was affected by conditions in soil. In this experiment we tested the two compounds at 10, 1 and 0·1 parts per million of the agar medium, or the soil solution, on red clover, lucerne, and mustard, and, in addition, β-indolylacetic acid on wheat. We made the tests in agar medium and in unsterilized Rothamsted soil. In the agar medium, β-indolylacetic acid was toxic to clover and lucerne at concentrations down to 0·1 part per million, and to mustard to 1 part per million, whereas wheat was affected only at 10 parts per million. α-Naphthylacetic acid was toxic to clover and lucerne at 0·1 part per million and to mustard at 1 part per million. In unsterilized soil, however, neither compound showed any toxicity except at the highest concentration of 10 parts per million, where there was a slight effect. A further experiment showed that the toxicity of β-indolylacetic acid remained in sterile soil though its loss in unsterilized soil was confirmed.

We concluded that neither compound would be of practical use for controlling plant growth in natural soil owing to the loss of its toxic action, probably through decomposition by soil micro-organisms. We therefore sought for a molecule of comparable toxicity, less liable to microbial attack. In view of the known activities of phenoxyacetic acids as plant stimulants, and the likelihood that chlorinated compounds would be resistant to bacterial action, we decided to investigate the comparative toxicity in agar and in unsterilized soil of 2 : 4-dichlorophenoxyacetic acid. We therefore made tests, in October 1942, on red clover, using 2 : 4-dichlorophenoxyacetic acid as well as the related compounds α-naphthoxyacetic acid and the methyl esters of α- and β-naphth-

TABLE 2. EFFECTS OF VARIOUS COMPOUNDS ON ROOT LENGTH IN CM. OF RED CLOVER IN AGAR AND IN WOBURN SOIL.

Compound	Concentration in p.p.m. of the agar medium and the soil solution.			
	in agar		in soil	
	1	100	1	100
α-Naphthylacetic acid	7·7	2·4	—	5·2
2,4-Dichlorophenoxyacetic acid	1·1	0·6	2·0	0·3
Methyl ester of α-naphthoxyacetic acid	7·2	6·3	—	6·7
Methyl ester of β-naphthoxyacetic acid	5·7	2·1	8·0	3·6
Methyl ester of phenoxyacetic acid	5·6	7·1	—	7·9
Control	7·9		6·8	



EFFECTS OF 2 : 4-DICHLOROPHENOXYACETIC ACID ON THREE TYPES OF PLANT IN TWO TYPES OF SOIL.

oxyacetic acids and phenoxyacetic acid, the last three compounds being included to test the effect of esterification. The results are given in Table 2. 2 : 4-Dichlorophenoxyacetic acid was much the most toxic of the compounds tested and, unlike the others, retained its toxicity to a satisfactory extent in unsterilized soil. We also noticed that its action, though affecting germination and top growth, showed most strikingly in its stunting effect on root growth. We therefore took root-length as a measure of toxic action.

The action of 2 : 4-dichlorophenoxyacetic acid in unsterilized soils of two widely different types and towards different types of plant was then tested. The test was made on red clover, sugar beet and wheat in test-tubes containing: (1) Woburn soil, a light sand low in organic matter, and (2) Rothamsted allotment soil, a clay loam rich in organic matter. The clover was also tested in the agar medium. The 2 : 4-dichlorophenoxyacetic acid was added in concentrations giving 1 to 100 parts per million in the soil solution. The diagram above shows the relative mean root-lengths from nine replicates, after four weeks growth, expressed as percentages of those in control plants. In Woburn soil 2 : 4-dichlorophenoxyacetic acid was strongly toxic to clover and especially to sugar beet, even at 1 part per million. Wheat, however, was not appreciably affected at the lower concentrations, though marked toxicity appeared at 10 parts per million. In the Rothamsted soil, toxicity towards clover and wheat was less marked, although the action on sugar beet was again very striking. In this soil also, wheat was the least affected of the three plants. In a further test on Woburn soil, we found that the addition of 0·1 per cent of potassium nitrate had no effect on the toxicity of 2 : 4-dichlorophenoxyacetic acid over the same range of concentrations.

TABLE 3. TOXICITY OF 2 : 4-DICHLOROPHENOXYACETIC ACID TO RED CLOVER WHEN SOWN IMMEDIATELY, AND AFTER 36 DAYS FROM TIME OF ADDING THE COMPOUND TO UNSTERILIZED WOBURN SOIL.

Concentration in p.p.m. of soil solution	Seedling root-length (cm.)	
	Sown immediately after addition of compound to soil	Sown 36 days after addition of compound to soil
100	0·0	8·2
10	0·0	8·5
1	3·8	8·4
Control 0	7·6	8·3

We then tested the persistence of the toxicity of 2 : 4-dichlorophenoxyacetic acid in unsterilized soil. The compound was added to Woburn soil in test tubes, at concentrations of 1, 10 and 100 parts per million of the soil solution with untreated controls. These tubes were stored for thirty-six days, after which a second set of tubes with similar treatments was set up and both sets were then sown with red clover. Table 3 shows the mean root-lengths (from nine replicates) after four weeks growth. The usual toxicity was shown in the tubes sown immediately after adding 2 : 4-dichlorophenoxyacetic acid, but storage had completely removed the toxicity.

The action of leaching on 2 : 4-dichlorophenoxyacetic acid in Rothamsted allotment soil was then tested. Air-dry soil was put up in two sets of 10 cm. glass pots. One set was allowed to take up water and the second a solution of 10 parts per million of 2 : 4-dichlorophenoxyacetic acid, up to saturation capacity. During the following three days, pots of each set were given 2, 4 and 8 successive leachings, equivalent respectively to the slow percolation at 1·4, 2·8 and 5·6 in. of water. Control unleached pots of each set were kept. After the leachings, the surface of each pot was sown with clover, sugar beet and wheat, and the pots were then normally watered. The results of this test showed that leaching equivalent to 1·4 in. water produced a reduction in toxicity that was just significant, while even that equivalent to 5·6 in. failed to remove all the toxic effect.

Discussion. The action of 2 : 4-dichlorophenoxyacetic acid on the plant varies according to concentration from complete inhibition of germination to a stunting of the root system and dwarfing of the leaves. This effect on the roots is accompanied by a marked thickening of the cortex and also, in certain cases, by the development of numerous short laterals. The formative effects noted by Zimmerman and Hitchcock[3] were also shown in clover chiefly by the multiplication and displacement of the leaflets and by inflation of the petiole. The macroscopic symptoms were similar to those produced by β-indolyl- and α-naphthylacetic acids.

Problems of great physiological interest are raised by the fact that these compounds, which are known to stimulate extension growth and to have profound developmental effects, are also toxic to the whole plant in doses of 0·1–1·0 part per million. 2 : 4-Dichlorophenoxyacetic acid, however, differs from such compounds as β-indolyl- and α-naphthylacetic acids in that it persists long enough in unsterilized soil to produce marked toxic effects. These effects show at a concentration of 1 part per million of soil solution, equivalent to 1 part in 4 millions of soil at 25 per cent moisture, representing on a field-scale about ½ lb. per acre of soil to 6 in. depth. It would thus seem that this compound should be of use in controlling plant-growth. It is not readily leached from soil but yet possesses the advantage of ultimately losing its toxicity; thus it would be unlikely to poison the land. The fact that

different types of plants vary greatly in susceptibility to its toxic action may here be of considerable practical importance.

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A Comparison of Certain Plant-Growth Substances with other Selective Herbicides

WITH grants from the Agricultural Research Council, a research team at the Imperial College of Science and Technology has been engaged in studying the general problem of weed eradication by chemical methods. Since April 1943, 4-chloro-2-methyl-phenoxyacetic acid and 2 : 4-dichlorophenoxyacetic acid have been included in the investigations. Their performance under field conditions has been compared with other herbicides in some forty-eight experiments involving more than 3,000 plots.

Special attention has been given to the control of annual weeds in cereal crops. In particular, a comparison has been made of the two phenoxyacetic acid derivatives with copper chloride, sulphuric acid and either a suspension of dinitro-*ortho*-cresol or a solution of the ammonium salt. When the spray concentrations —at the standard rate of 100 gallons per acre—are kept within the limits at which each herbicide is truly 'selective', then a feature of the results is the specific action of the several herbicides on different weed species.

The reactions of the chief annual weeds investigated are summarized in the accompanying table. It is seen that whereas for some weeds such as *Brassica arvensis* or *Thlaspi arvense* several herbicides are capable of giving a high degree of control, for other species like *Scandix Pecten-Veneris* or *Matricaria inodora* one compound is alone effective. Moreover, copper chloride and sodium 4 : chloro 2 : methyl phenoxyacetate are more specific than dinitro-*ortho*-cresol in the narrower range of weeds they can destroy or partially control.

Following on weed suppression, increases in the yield of grain up to 90 per cent have been recorded. There is no indication that dinitro-*ortho*-cresol at the concentrations required for weed control (0·3–1·2 per cent) has checked growth. Both copper chloride at the higher concentrations (3–4 per cent) and sulphuric acid (9–18 per cent) have given smaller increases in yield. 4-chloro-2-methyl-phenoxyacetic acid, over a range of 0·025–0·25 per cent, has no adverse effect on spring- and autumn-sown cereals; but at higher concentrations it may cause a reduction in yield of spring oats and barley. The evidence to date is that 2 : 4 dichlorophenoxyacetic acid is less selective in its action and is more likely to injure the crop.

The phenoxyacetic acid derivatives are outstanding in that they can operate through absorption by both the shoot or the root. In consequence, germination of weed seedlings in the soil may be suppressed for some weeks after application. The suppression of

RELATIVE EFFECTIVENESS OF CUPRIC CHLORIDE, AMMONIUM DINITRO-*ortho*-CRESOL, SODIUM 4-CHLORO-2-METHYL-PHENOXYACETATE AND SULPHURIC ACID FOR THE CONTROL OF ANNUAL WEEDS.

Degree of control.

+++ more than 90%. ++ more than 75%. + more than 50%. *R* = resistant—less than 50% control.

Weed species	Cupric chloride	Ammonium dinitro-*ortho*-cresylate	Na : 4-chloro-2-methyl-phenoxyacetate	Sulphuric acid
Brassica arvensis (yellow charlock)	+++	+++	+++	++
Raphanus raphanistrum (white charlock)	++	++	+++	++
Thlaspi arvense (pennycress)	+++	+++	+++	++
Erysimum cheiranthoides (treacle mustard)	+++	+++	+++	+++
Atriplex patula (goosefoot)	*R*	+++	(*R*)*	++
Chenopodium album (fat hen)	*R*	+++	*R*	++
Matricaria inodora (scentless mayweed)	*R*	+++	*R*	+
Matricaria Chamomilla (chamomile)	*R*	+++	*R*	+
Centaurea Cyanus (cornflower)	*R*	++	+++	+
Polygonum aviculare (knotgrass)	*R*	+	*R*	+
Polygonum Convolvulus (bearbind)	+++	++	+	+++
Scandix Pecten-Veneris (Venus' comb)	*R*	+	+++	+
Papaver Rhoeas (corn poppy)	*R*	+++	++	*R*
Fumaria officinalis (fumitory)	*R*	+++	(*R*)	*R*
Alchemilla arvensis (parsley piert)	*R*	+++	*R*	+++
Ranunculus arvensis (corn buttercup)	*R*	++	+++	+
Spergula arvensis (spurrey)	+	++	+	+++
Galeopsis Tetrahit (hemp nettle)	+	+++	+++	+++
Veronica hederifolia (ivy-leaved speedwell)	++	+	+++	+++

* Brackets indicate that category is tentative.

B. arvensis and *E. cheiranthoides* has been obtained with amounts of 0·5–1·0 lb. per acre, but for other species, such as *Polygonum Persicaria* (willow weed), there is no inhibiting effect. The minimal quantity required to kill susceptible weeds, once they have emerged through the soil, is less—an almost complete control of *B. arvensis* has been obtained at a spray concentration equivalent to 4 oz. per acre of the sodium salt.

In addition to the investigation of cereal crops, the research has been extended to cover annual weed control in grasses, clovers, etc., for seed production, flax and linseed, peas, onions, leeks, and perennial weeds in grassland. With grasses, once the seedling stage is passed, sulphuric acid, cupric chloride, dinitro-*ortho*-cresol and 4-chloro-2-methyl-phenoxyacetic acid can be employed for the destruction of annual weeds. The clovers in the early stages are either killed or severely checked by such spray treatments, though they are somewhat more resistant when fully established.

In flax, copper chloride and the *sodium* salt of dinitro-*ortho*-cresol have given the most promising results: the ammonium salt causes serious crop injury. 4-chloro-2-methyl-phenoxyacetic acid, although it may in no way affect the straw or seed yield of flax, will bring about a reduction in fibre yield. The extent of the reduction is dependent on the concentration and the time of spraying. Only sulphuric acid and cupric chloride can be employed in onion and leek crops for selective weed control; dinitro-*ortho*-cresol compounds and the phenoxy-

acetic acid derivatives are toxic. Preliminary trials suggest that maize is resistant to some dinitro-*ortho*-cresol compounds, and 4-chloro-2-methyl-phenoxy-acetic acid.

The phenoxyacetic acid derivatives are again exceptional in that they alone show promise for the control of some perennial weeds. In established grassland the experiments indicate that 4-chloro-2-methyl-phenoxyacetic acid, at concentrations which do not kill out *Trifolium repens*, from a single spraying, will destroy buttercups (*Ranunculus* spp.), *Equisetum arvense* (horsetail) and partially suppress *Cirsium arvense* (creeping thistle). At these levels of concentrations—up to 2·5 lb. per acre—*Rumex* spp. (docks), *Urtica dioica* (stinging nettle) and *Pteris aquilina* (bracken) are resistant.

The following members of the team have taken part in these investigations: A. J. Rutter, J. Carpenter, H. Wild, B. G. Goodey, J. L. Crosby, C. P. Whittingham, P. A. Tallentire, A. C. Crundwell, P. Greig-Smith, J. C. Gimingham.

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ARTIFICIAL PROTEIN FIBRES: THEIR CONCEPTION AND PREPARATION

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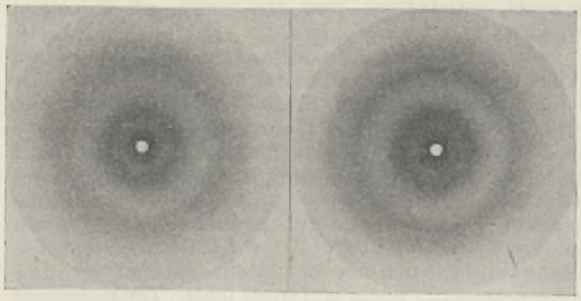
A PRINCIPAL aim of science in relation to industry is to elucidate for the industrialist the nature of his working materials. All fundamental research into the structure and properties of things is therefore of potential value to industry; but this is a platitude to the man of science. To the non-scientific majority, however, it is not yet so obvious, and it is still regrettably necessary to make play with the more spectacular discoveries in order to attract proper support for research.

A recent development that has captured the imagination of the public is the discovery how to make artificial protein fibres from nuts, beans, milk, and other not visibly fibrous protein sources, and the official announcement during December 1944 by Imperial Chemical Industries, Ltd., of the successful production of 'Ardil' from the protein of pea-nuts suggests an occasion for briefly re-telling the story, but this time more from the fundamental point of view than has been possible in the popular accounts that have appeared. I have no intention here of labouring the theme of what industry owes to science, or vice versa, or of emphasizing again that the great industries of the future must draw their sustenance from unremitting research; but I would certainly like to stress the case of the artificial protein fibres as being a most impressive example of the indivisibility of science. The discovery of the underlying principles was of purely academic origin, an outcome of X-ray and related studies of the molecular structure of biological tissues—studies that were neither supported by nor consciously dedicated to industry; the basic experiments were compounded of physics, chemistry, and biology, and were carried out in a university. Thereafter the development to commercial satisfaction was the work of industrial chemists and technologists.

It is not claimed, of course, that artificial protein fibres had never been produced previously—indeed, it happens that the casein fibre, 'Lanital', for example, was launched in Italy at almost the same time as the quite independent fundamental investigations about to be described were pointing the way to the general solution of the problem—but it is clear that in the absence of any structural picture of the protein molecule, and especially of the relation between the fibrous and the non-fibrous proteins, all such ventures were necessarily along empirical lines. The difference now is that what industry there was has been re-born as an inseparable part of protein science, with all the potentialities for advancement that this profoundest of molecular studies has to offer. It is in fact a logical *prediction* from the X-ray interpretation of protein denaturation that it should be possible to make fibrous from non-fibrous proteins: we can see now both what has to be done and the reason for it.

Since the beginning of the century, chemists have been increasingly convinced that all proteins are polypeptide chain systems, alone or in combination with various prosthetic groups; but there seemed to be a distinction between the fibrous and non-fibrous kinds in that the molecules of the latter are massive, rounded bodies that often aggregate to build orthodox, visible crystals; hence the name 'corpuscular' proteins. With the growth of the concept of fibres as 'molecular yarns' constructed from long chain-molecules, a concept to which many techniques have contributed but which first became 'real' under the methods of X-ray analysis, there ceased to be any formal difficulty with regard to the protein fibres, as was demonstrated by Meyer and Mark when, in 1928, they interpreted the diffraction pattern given by natural silk (fibroin[1]); but the problem of the arrangement inside the corpuscular proteins remained, for sometimes the X-ray photographs showed sharp reflexions—characteristic to be sure of regular crystal lattices but not to be carried much beyond that on account of the large number of atomic parameters involved—but more often they showed simply two diffuse rings of spacing about 9½ A. and 4½ A., respectively. The first requirement was to explain these two rings, and this was done[2] on the basis of the X-ray data given by the *elastic* fibrous protein, keratin. Keratin did not fit in with the idea of extended polypeptide chains that had been found to suffice for fibroin; only the stretched form (β-keratin) could be interpreted on such a view, but the normal, unstretched form (α-keratin) demanded the postulate of a regularly folded configuration besides. The reversible intramolecular transformation between α- and β-keratin, corresponding to the transition between two distinct types of diffraction pattern, was recognized as providing the explanation of the well-known long-range elasticity of mammalian hairs and other keratinous tissues[3].

Among other things, then, the X-ray study of keratin brought out the two main points from which in due course the theory of artificial protein fibres followed naturally. The α-form revealed for the first time the existence of polypeptide chains that are normally in a folded state, while the β-form gave the average dimensions per amino-acid residue (and therewith an estimate of the order of density of proteins and the mass per unit area of protein monolayers[2,4]), and so bridged the gap to analytical chemistry and laid the foundations of a structural



a *b*

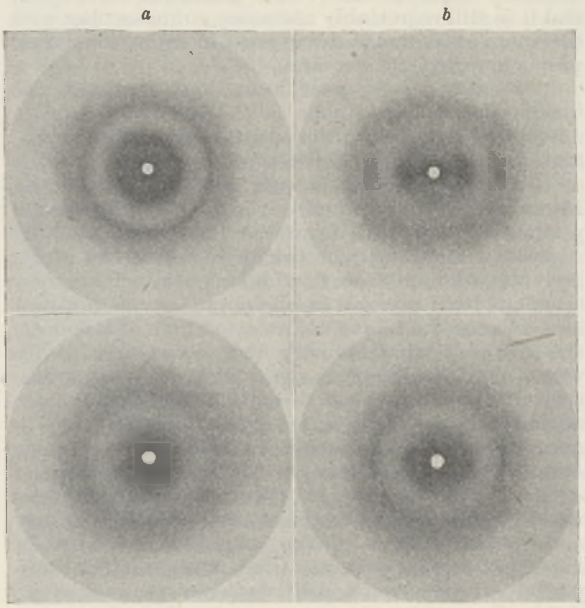
Fig. 1. X-RAY POWDER PHOTOGRAPHS OF (*a*) DRIED EGG WHITE, AND (*b*) DRIED BOILED EGG WHITE.

stoichiometry[5]. Thus was evolved the concept of the 'polypeptide grid', and the two rings so common in protein diffraction patterns were identified as arising from the two principal modes of linkage between neighbouring polypeptide chains, namely, that between the side-chains and that between the backbones[2]. The corresponding spacings are now always referred to as the 'side-chain' and 'backbone' spacings.

The next advance came from an X-ray comparison of a number of protein preparations before and after wetting[6]. It was found that not only did the inner ring show the greater spacing variation from protein to protein, but also it generally showed a spacing increase on wetting. Both these properties would be expected if, as had been inferred from keratin, the reflexion represented the lateral separation of the main chains in the direction of the side-chains, and the evidence was in fact accepted as establishing the inference.

In the same investigation, among the preparations examined were egg and serum albumins that had been denatured (and coagulated) by heat. The obvious change brought about by this treatment was seen to be a marked sharpening of the backbone reflexion, and, less obvious, the appearance of at least one other outer ring of spacing about 3·6 A. (see Figs. 1*a* and 1*b*). In short, it became clear—what has been demonstrated since on many other protein preparations—that *the denaturation and aggregation of a corpuscular protein leads ultimately to a diffraction pattern like that given by disoriented β-keratin*. When keratin is stretched from the α- to the β-form, the side-chain reflexion remains and a strong backbone reflexion arises by the process of flattening the polypeptide grids—pulling out the folds, that is, that lie in planes transverse to the side-chains. This is a mechanical operation, but it appeared now that a similar sort of change could be brought about in the structure of the corpuscular proteins by thermal agitation, for example. (Later, it was shown[7] that the muscle protein, myosin, belongs to the same molecular family as keratin, and myosin can give either an oriented β-photograph by stretching or a disoriented β-photograph by heating.) In some way, therefore, the arrangement inside the corpuscular proteins was a generalization of the α-keratin idea; the situation was like α-keratin only more so. The polypeptide chains were there, but presumably they were folded and grouped in specific configurations from which in most cases they could be liberated fairly easily to produce a variety of non-specific configurations. Subsequent aggregation (at least as regards the more organized regions that are responsible for the regular diffraction pattern) involved a building of 'crystallites'—sometimes more perfect, sometimes less perfect, but at any rate of the type of the aggregates of polypeptide grids that constitute the crystallites of β-keratin. (Denaturation of a corpuscular protein as here described is the thoroughgoing irreversible phenomenon as it is usually understood in Great Britain. Reversible loss of solubility and specificity is sometimes described as 'reversible denaturation', but such changes are conceivable without disorganization of the molecule as a whole. Sometimes, too, no clear distinction is recognized between denaturation and the aggregation of proteins that are already in an extended configuration[8]. Myosin 'denatured' by simple drying is still in the folded α-form, but there is disorganization of the folds if it is heated.)

It followed at once from this line of argument that if, after unfolding the polypeptide chains, they could be drawn parallel, or approximately parallel, then artificial *fibres* would result; and the test would be the production of an *oriented* β-photograph. Decisive orientation effects were first obtained with denatured preparations of the seed globulins, edestin (from hemp seed) and excelsin (from Brazil nuts), and also with 'poached' egg white[9]; while the first actual fibres were spun from strong urea solutions[9]. When edestin, for example, is dissolved in strong aqueous urea, the solution in time becomes very viscous, and elastic fibres may be produced either by drawing out the viscous mass or by squirting it through a capillary tube into water or dilute salt solution. (This sort of observation is not new, but it seems that X-rays first clearly exposed the reason for the rise in viscosity—the unfolding of round molecules to give polypeptide chains in extended configurations.) In general, though, such fibres have to be stretched farther in order to reveal definite orientation effects in the X-ray photographs. As a matter of interest, some of the early diffraction patterns are reproduced in Fig. 2. They illustrate the first demonstration by X-rays of the transformation of an originally crystalline protein into an elastic fibrous structure.



a *b* *c* *d*

Fig. 2. (*a*) DISORIENTED β-KERATIN; (*b*) ORIENTED β-KERATIN; (*c*) DISORIENTED DENATURED EDESTIN; (*d*) ORIENTED (STRETCHED) DENATURED EDESTIN.

Fig. 3. ARDIL FIBRES.

Experiments on continuous fibre production from urea solutions were then conducted in Prof. A. C. Chibnall's laboratory at the Imperial College of Science and Technology, London; but later, though considerable further advances were made, it was felt better to return to the more fundamental side of protein research. Thereafter, as indicated above, the production of 'Ardil' (see Fig. 3) and its development to commercial satisfaction was the work of chemists of Imperial Chemical Industries, Ltd.[10]. 'Ardil' is produced from the peanut globulin, arachin, and the original urea process has now been replaced by extraction and 'maturing' with dilute alkali; also, striking new after-treatments have been evolved for improving the tensile properties of the fibres and their resistance to dyeing and finishing processes. Actually, the present stage of development was reached by the beginning of the War, but since then further work has been held up.

'Ardil' is a cream-coloured, crimped, elastic fibre that is soft and warm: it is a kind of artificial wool without surface scales. It greatly enhances the felting of wool, however, and dyes like wool, but it is not attacked by moths. Its elasticity arises from the circumstance that the unfolding of the original corpuscular molecules is imperfect, vestigial folds remaining that are an irregular counterpart of the regular α-folds to which the elasticity of keratin is due. As now produced, 'Ardil' shows no orientation in its diffraction pattern, but an oriented β-pattern begins to appear on stretching. Fabrics have been made purely of 'Ardil', but its best use is likely to be in combination with wool and other fibres. A by-product of its manufacture is, of course, arachis oil, of which peanuts contain 48–50 per cent; furthermore, after the oil and protein have been extracted, the residue can be used for cattle food.

During the last few years a number of important papers have been published in America on the preparation of artificial protein fibres with the aid of detergents[11]. Products such as 'Nacconol NRSF' (which is essentially dodecyl benzene sodium sulphonate) are found to act as excellent unfolding agents for the corpuscular proteins, and with the technique described it is possible to make fibres much stronger than anything reported previously. Using egg albumin, for example, the complex formed by mixing equal portions of 3 per cent solutions of recrystallized egg albumin and detergent is first precipitated with saturated magnesium sulphate, the resulting 'dough' is drawn out into fibres, which are washed with water and extracted with 60 : 40 acetone-water solution, and then finally the fibres are stretched by about 400 per cent in live steam. Before the stretching in steam, the orientation in the X-ray diffraction pattern is of the 'crossed' type first observed with stretched films of poached egg white[9], that was interpreted as corresponding to chain-bundles broader than they are long; but after the stretching it is as in β-keratin with the chain-bundles lying along the direction of stretching. (The effect illustrated in Palmer and Galvin's paper[12] is slight and was at first thought not to be there; but it is now agreed that before stretching in steam there is no real discrepancy with the 'poached egg effect'. Private communication.) The oriented β-pattern given by these stretched egg albumin fibres is very good indeed: it is shown in Fig. 4, which is a reproduction of a photograph kindly sent me by Dr. Palmer. Egg albumin is one of the most typical and most studied of all the crystalline corpuscular proteins, and here in the end it is made to yield one of the best β-fibre photographs! The wheel has now come full circle.

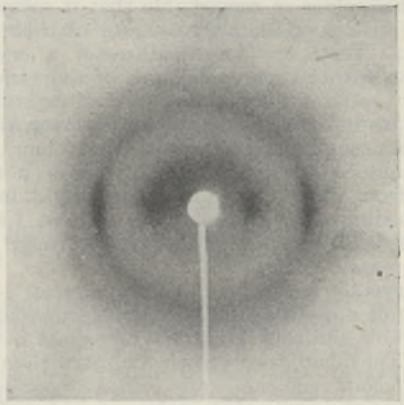


Fig. 4. FIBRE PATTERN (β-KERATIN TYPE) GIVEN BY EGG ALBUMIN FIBRES PREPARED BY LUNDGREN AND PHOTOGRAPHED BY PALMER AND GALVIN.

Lundgren and O'Connell report that artificial fibres from egg albumin (and from chicken-feather keratin, which also responds to the technique just described) have been prepared with breaking strengths of more than 70 × 10^3 lb. per sq. in.: to appreciate what this means, it may be noted that in the same table they quote 72–100 for nylon, 46–74 for natural silk, and 17–25 for wool. Incidentally, lest it should seem somewhat indecent these days to talk about using egg white for making fibres, it should be added that the same authors point out that there are available annually in the United States more than 26,000,000 lb. of inedible technical egg white (much of which goes to waste) and more than 170,000,000 lb. of chicken feathers.

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X-RAY CRYSTALLOGRAPHIC MEASUREMENTS ON A SINGLE CRYSTAL OF A TOBACCO NECROSIS VIRUS DERIVATIVE

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AND
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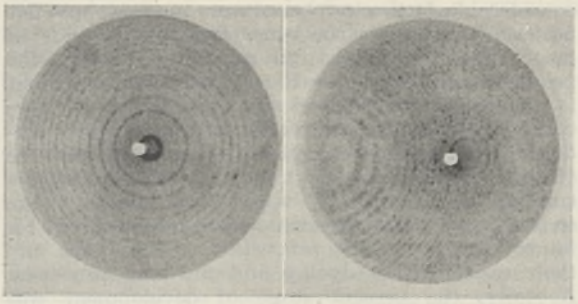
IN August 1944, N. W. Pirie, of Rothamsted, sent us a few large crystals of a tobacco necrosis virus derivative (Rothamsted strain)[1] on which we have been able to make some X-ray measurements. Before we could complete the investigation the preparation unfortunately deteriorated, but the crystals are in themselves so remarkable that it seems worth while to give a preliminary account of their characteristics. They are described as of a crystalline derivative rather than as crystals of the virus itself, since by the time the preparation had crystallized it had lost its infectivity. It shows, however, the same serological and ultracentrifugal behaviour as the active material and so probably does not differ greatly from this[2]. The molecular weight indicated by the sedimentation constant is at least 1,850,000, and this order of magnitude is confirmed by our present measurements.

The crystals were of quite unusual size, 1–5 mm. across, and appeared to be of two forms, thick triclinic prisms and thin hexagonal or pseudo-hexagonal plates. They had been grown for more than a year from aqueous solution, and when received were adhering to the walls of the tube covered with a film of moisture. The thickest crystal of all, which was of the first kind and measured approximately 2 mm. × 1 mm. × 0·5 mm., was washed into a drop of 1/10 saturated ammonium sulphate solution and drawn into a thin-walled Lindemann glass capillary which was then sealed off. In this tube it could be examined still covered with a film of liquid.

In form the crystal was a thick prism tending to be elongated along [111] and bounded by well-defined faces, probably {011}, {1$\bar{1}$0} and {$\bar{1}$01}. In polarized light it showed low birefringence with oblique extinction when viewed parallel to (100), and approximately straight extinction along [111] when viewed perpendicular to (100), which suggested monoclinic or pseudo-monoclinic symmetry. These measurements could not be made very accurately owing to the position of the crystal in the tube. But in essentials they are confirmed by the X-ray data, which show that the crystal is triclinic, with $a = 179$ A., $b = 219$ A., $c = 243$ A., $\alpha = 87\frac{1}{2}°$, $\beta = 97\frac{1}{2}°$, $\gamma = 97\frac{1}{2}°$ (reduced cell), space group $P1$; and that there is a marked approach to monoclinic symmetry, which is better illustrated by adopting the larger unit cell $a' = 179$ A., $b' = 303$ A., $c' = 346$ A., $\alpha' = 96°$, $\beta' = 101°$, $\gamma' = 90°$, space group $A1$. In the account which follows, the indices given refer to the pseudo-monoclinic cell.

In order to measure these very large cell dimensions, using copper $K\alpha$ radiation, five $2\frac{1}{2}°$ oscillation photographs were taken of the crystal rotated about the normal to (001), with a plate-to-crystal distance of 10·1 cm. (Fig. 1). The X-ray beam was defined by a lead glass capillary of bore 0·025 cm. kindly lent to us by Dr. W. T. Astbury. With this slit system there is some blurring of neighbouring hkl reflexions for which the index k varies only by 1, since the interval, $b = 303$ A., is outside the theoretical limit of resolution; but since there are sharp reflexions with indices $0kl$ differing by various multiples of k, the lattice is defined unambiguously. Apart from this difficulty, the patterns of the X-ray reflexions on the oscillation photographs are exactly comparable with those found on simpler crystals, the only difference being one scale.

Fig. 1. TOBACCO NECROSIS VIRUS DERIVATIVE.
$2\frac{1}{2}°$ oscillation photograph, crystal rotating about normal to (001), X-ray beam parallel to (100). 90 hours exposure.



(*a*) (*b*)

Fig. 2. TOBACCO NECROSIS VIRUS DERIVATIVE.
(*a*) Crystal 'still'. X-ray beam nearly perpendicular to (h*00). Reflexions from (h*00). 12 hours exposure.
(*b*) Crystal 'still'. X-ray beam 60° from (h*00). Reflexions from (h*00), ($\frac{1}{4}h$*k*0), ($3h$*k*0). 10 hours exposure.

This difference of scale makes it possible, however, for us to estimate the unit cell dimensions in quite a different way on a new variety of X-ray photograph. The unit cell dimensions are so large that, even when the crystal is kept stationary, innumerable crystal planes are geometrically in a position to reflect monochromatic X-rays. Photographs obtained with the crystal 'still' accordingly show numbers of X-ray reflexions, and these, though individually unresolved, are characteristically arranged on the plate in series of concentric circles, or ellipses (Fig. 2). Their arrangement is most easily understood by referring the process of reflexion in the usual way to the interaction of the crystal reciprocal lattice with the sphere of reflexion[3,4]. In reciprocal space the lattice points are crowded together so that many will be in contact with the sphere of reflexion in any position of the

crystal relative to the X-ray beam. The points are, however, grouped into reciprocal lattice planes, and these intersect the sphere of reflexion in circles, where the interval between two concentric circles depends on the spacing of the series of parallel reciprocal lattice planes and hence inversely on the crystal cell dimensions. The diagrams in Fig. 3 illustrate the process.

Since only reciprocal lattice planes with large spacings and in a position not far from normal to the

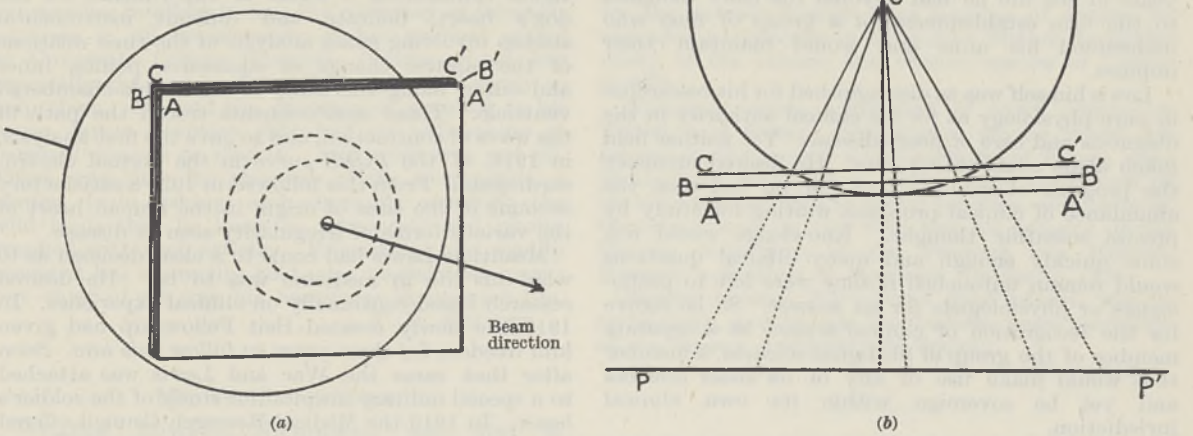


Fig. 3. INTERACTION OF RECIPROCAL LATTICE PLANES WITH THE SPHERE OF REFLEXION (*a*) IN PERSPECTIVE, (*b*) IN PROJECTION. *AA′*, *BB′*, *CC′*, reciprocal lattice planes; *O*, centre of sphere of reflexion; *PP′*, photographic plate.

beam direction can give well-defined circles or ellipses when projected on to the photographic plate, three or four 'still' photographs at intervals of 30° or so serve to define the lattice normal to any particular rotation axis. In the present case we first deduced two of the three reciprocal cell dimensions from a series of 'still' photographs which showed reflexions from the reciprocal lattice planes (h^*00), $(0k^*0)$, (h^*k^*0), $(h^*\bar{k}^*0)$, $(2h^*k^*0)$, $(3h^*k^*0)$, $(4h^*k^*0)$, $(h^*\overline{2k}^*0)$ and $(3h^*2k^*0)$, and afterwards took the oscillation photographs when we knew the degree of resolution we should need. With our apparatus a good 'still' photograph at a plate-to-crystal distance of 4 cm. could be obtained in twelve hours, while the best oscillation photograph took ninety hours. As these exposure times with our X-ray tubes were so long, we transferred the crystal, at the kind invitation of Sir Henry Dale, to the Davy Faraday Laboratory, at the Royal Institution, London, for photography with a more powerful tube. One trial photograph was taken in four hours, but at this stage an accident to the crystal prevented further work. In all, the crystal was exposed for approximately five hundred hours to X-radiation without apparent deterioration.

While the 'still' photographs provide good evidence on the cell dimensions, we have to turn to our very limited series of oscillation photographs for further details of the crystal structure. One striking feature of the X-ray photograph reproduced (Fig. 1) is the absence of a number of reflexions, $h0l$, where h is odd, which suggests that there is a pseudo-glide plane of symmetry present and that the arrangement of the molecules in the crystal approximates to that required by the space group Aa. In this event there must be four units in the face-centred unit cell or two in the primitive cell measured above. In the absence of measurements of crystal density and of water content, we can only give a rough estimate for this unit molecular weight based on measurements of these quantities on other virus crystals. If we take the figures for the crystalline bushy stunt virus[5], water content 55 per cent, density 1·286, the molecular weight of the tobacco necrosis virus derivative can be calculated as 1,600,000. But since the nucleic acid content of tobacco necrosis virus is higher than that of bushy stunt virus, it is probable that the density is greater, and this figure may therefore be regarded as a lower limit for its molecular weight. As such it compares well with the ultracentrifuge figure given above, 1,850,000, which is also a lower limit calculated[6] for a spherical molecule with the observed sedimentation constant, 50×10^{-13}.[1] Actually the crystal structure suggests that the molecules do not differ in shape very markedly from spheres. On this view the estimated molecular dimensions vary from about 80 A. as a minimum radius to a possible maximum of 100 A. radius in one direction.

The presence of even a pseudo-glide plane of symmetry in the crystal structure of a molecule certainly containing asymmetric centres may appear at first sight odd. But this effect again must be due to the enormous size o the molecule. It has only been observed so far for reflexions with spacings greater than 25 A., and the intensities of these can be sensitive to little more than the general shape and position of the molecules in the unit cell. With fresh material we hope to be able to extend our survey of the X-ray reflexions and to obtain a better idea of the actual dimensions of the virus molecule. That the required X-ray data can be got is proved by the 'still' photographs, which show X-ray reflexions from planes with spacings as small as 2·8 A.

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OBITUARIES

Sir Thomas Lewis, F.R.S.

SIR THOMAS LEWIS died on March 17, at the age of sixty-three, when still in full mental vigour. He leaves behind him a long train of brilliant achievements; and the path through which he moved for the illumination of medical science by clinical research will not remain void, for in the last twenty years of his life he had devoted his chief thoughts to the firm establishment of a group of men who understood his aims and would maintain their impulse.

Lewis himself was as distinguished for his researches in pure physiology as for his clinical authority in the diagnosis and care of heart disease. Yet neither field taken alone could satisfy him. He desired intensely the progress of medical skill, for he had seen the abundance of clinical problems waiting for study by precise scientific thought. Knowledge would not come quickly enough and many clinical questions would remain untouched if they were left to pathologists or physiologists for an answer. So he strove for the recognition of clinical science as a separate member of the group of biological sciences, a member that would make use of any of its sister sciences and yet be sovereign within its own clinical jurisdiction.

As the Physiological Society has been founded to bring physiologists together in helpful debate and fellowship, so Lewis in 1930 founded the Medical Research Society and transferred to it his journal *Clinical Science* with the aim of giving to that band of scientific workers their own tourney ground. The success of such a movement to bring scientific ways of thought into the heart of clinical experience was the dearest of Lewis's hopes, and despite his untimely death it will not now fail.

Lewis came of pure Welsh lineage, perhaps the most distinguished man of science as yet given to the world by the Principality. His father had an important place there in mining, having been president of the Coal Owners' Association; but Lewis when quite a young boy had preferred medicine for the quaint reason that the family doctor had delighted him by being a skilful conjurer. It was the side delights, apparently inhering in medicine, rather than the straightforward work of a doctor that entranced him. His abilities were soon proved by scholarships and gold medals in progress through medical training at Cardiff and later in London. But all this time his mind was finding chief satisfaction in the laboratories of physiology, where he had begun research and published papers even before qualification. In London, through acquaintance with Starling, Cushny and Leonard Hill, all busy with problems of the circulation, he met James Mackenzie and discussed with him the nodal rhythm that the latter had proposed in explanation of the particular type of irregularity of the heart-beat which he had studied clinically by venous tracings. Einthoven in Holland had recently devised a delicate galvanometer which could be used for analysis of the heart's action in man. Lewis seized the vital chance, obtained the instrument, and with it quickly proved, in 1909, that the particular irregularity in man was due to auricular fibrillation. This mechanism had been earlier identified in animal experiments by Cushny; but the physiological discovery was waiting its application to analysis of disorders of the human heart. The application was relatively easy and it was made at the same time by workers in Vienna. But Lewis's genius, now that fortune had granted him this opening to fruitful research, moved far and fast with it. He concentrated all his clinical experience upon analysis of the forms of irregular heart-beat, identifying each by its galvanometric curve and then studying separately the prognosis and treatment of each form. But more profound analysis of the character of each electrical curve of irregularity was needed, and to this end Lewis commenced a series of experiments on the dog's heart, delicate and difficult instrumental studies involving exact analysis of the time relations of the electric change at successive points, inner and outer, along the walls of the double-chambered ventricle. These measurements traced the path of the wave of contraction, and so gave the first analysis, in 1916, of the *PQRS* curve in the normal electrocardiogram. From this followed in 1920 a satisfactory account of the sites of origin in the human heart of the various forms of irregularity seen in disease.

Meantime Lewis had come to a clear decision as to what his life in medicine was to be. He desired research based continually on clinical experience. In 1910 the newly created Beit Fellowship had given him freedom for three years to follow this aim. Soon after that came the War and Lewis was attached to a special military hospital for study of the soldier's heart. In 1916 the Medical Research Council offered him a permanent position on its external staff, and Lewis gladly accepted this escape from the road that would have rapidly led to a great consulting practice in heart disease but away from intense and progressive research. At the end of the War he returned to the staff of University College Hospital and the enjoyment of a laboratory adequately staffed by the Medical Research Council. But he had shown during the war period that important discoveries could be made by use of the simplest apparatus, such as a sphygmomanometer, provided it were guided by subtle thinking; and his demonstration in 1916 of the contractility of the capillaries by adrenalin led him to the exploration later of entirely new fields of his own. The results were summarized in 1927 in his book on the "Bloodvessels of the Human Skin", in which he analysed their nervous control and particularly their inflammatory reaction to a chemical substance, akin to histamine, found by him to be normally released upon injury to skin cells. This idea of an autochthonous chemical irritant of nerves, now running parallel with the discoveries of physiology in respect of cholinergic nerves, was later extended by Lewis to include some forms of pain, for example that of angina pectoris; and from that he was moving to a hopeful investigation of deep visceral pain, a matter of intense importance to clinicians, when in 1939 war again came and ended the work.

Lewis had been elected fellow of the Royal Society in 1918, at the early age of thirty-six, and in 1941 he received its Copley Medal, the highest recognition that British science can give for fundamental discovery. Not since Lord Lister, in 1902, had this award been made to an active clinician. His world-wide reputation naturally brought him the honorary membership of many learned societies and the degrees of many universities at home and abroad. In practical medicine the teaching of two books upon heart diseases, which he wrote deliberately for medical practitioners—and they were translated into several foreign languages—had a transforming influence so

complete that it is now almost forgotten that the change was largely due to his researches. Otherwise he refused to spare time for routine discussions at medical societies, for he had no wish to stay fixed on a summit as an acknowledged heart specialist. Independence had been given to him by that intensely important move of the Medical Research Council when it led the way in creating whole-time posts for clinical research, and so he could go wherever his genius beckoned. With his amazingly clear and penetrating intellect he was the mainstay of scientific progress in all departments of his medical school at University College Hospital. Young or old, but especially the former, found him eager and ready to help them. He had the devotion of a zealot to his cause, but a devotion that had no self-seeking or personal conceit to mar it. And work was not all. There was time for pleasures of the countryside, a garden, trout fishing, and especially watching the wild birds that he knew so intimately and photographed with such artistry. Friendships, not readily accepted but staunchly held, also played their part; but chief of all was his happiness in married life.

T. R. ELLIOTT.

Mr. W. E. Nicholson

WILLIAM EDWARD NICHOLSON, who died at Mullion, Cornwall, on February 13, was, after his life-long friend, H. N. Dixon, the best known authority in Great Britain on the taxonomy of the bryophytes. He was interested particularly in the European mosses and in European and exotic hepatics.

By profession a solicitor, Nicholson was born in 1866 at Lewes, Sussex, where almost the whole of his life was spent. He was educated at Marlborough until ill-health compelled him to leave at an early age. From boyhood he was deeply interested in natural history, and until middle life as much in entomology as in botany. Towards the end of the last century, Nicholson began the study of the mosses and liverworts, receiving help and encouragement from William Mitten, the veteran Sussex bryologist and the greatest authority on the exotic mosses of his day. Later he came into contact with H. N. Dixon, and the two soon gained an international reputation for a number of remarkable additions to the European flora made on journeys ranging from southern Portugal to Tornean Lapland.

Though without a scientific training, Nicholson had a keenly critical mind and abnormally acute powers of observation, which were strikingly shown by the extraordinarily large number of species he was able to discover in his native Sussex, including many of the minute and elusive species of *Cephaloziella*.

Nicholson wrote a number of short papers on British and foreign bryophytes, among which his papers on the mosses and liverworts of Sussex were particularly noteworthy. In his later years most of his time was devoted to exotic hepatics, and his most important work in this field was his account of the Chinese collections of the Freiherr von Handel-Mazzetti published in *Symbolae Sinicae* in 1930.

Besides botany, Nicholson's chief interest was in archæology, and he was for fourteen years secretary of the Sussex Archæological Society.

P. W. RICHARDS.

WE regret to announce the following deaths:

Dr. Denis Coffey, formerly president of University College, Dublin, and vice-chancellor of the National University of Ireland, on April 3, aged seventy-nine.

Sir Ambrose Fleming, F.R.S., on April 18, aged eighty-five.

Mr. A. R. Hinks, F.R.S., C.B.E., secretary of the Royal Geographical Society since 1915, on April 14, aged seventy-one.

Dr. Bohuslav Vrbenský, a Czechoslovak medical man, who held office as Minister of Health and Minister of Public Works in several Governments, and in particular was instrumental in securing improved working conditions for Czechoslovak miners, on November 25, 1944, aged sixty-two.

NEWS and VIEWS

Pure Mathematics at Cambridge: Prof. L. J. Mordell, F.R.S.

THE election of Prof. L. J. Mordell as successor to Prof. G. H. Hardy in the Sadleirian chair of pure mathematics at Cambridge will not have come as a surprise to those who are aware of his international reputation as a leading British mathematician. His main interest has always been in the theory of numbers, and to this subject he has made perhaps as great an original contribution as any mathematician in Britain, past or present. Among outstanding achievements one may mention (1) the proof that all rational solutions of a cubic equation $f(x,y) = 0$ can be derived by a well-known rational process from a finite number of them (Mordell's finite basis theorem), (2) his work on the representation of a number as a sum of squares, which has its root in Mordell's mastery of the theory of modular functions, (3) his work, mostly in recent years, on the geometry of numbers, where he has opened up new avenues of investigation. Characteristic of Mordell's work is the *significance* of the problems he has formulated and attacked. The advances he has made, apart from their intrinsic importance, have often been the starting point for work by other distinguished mathematicians.

Mordell went from Philadelphia to St. John's College, Cambridge, in 1907 with a senior scholarship in mathematics awarded on the scholarship examination of December 1906. He was third wrangler in the 1909 Tripos. After being lecturer at Birkbeck College, London, he went to Manchester, where he has been Fielden professor of pure mathematics since 1923. In 1941 he was awarded the De Morgan Medal of the London Mathematical Society (of which he is now president). From about 1933 onwards, Mordell gathered round himself at Manchester a group of mathematicians from all over the world, whose interests were mainly in number-theory. The success of the 'Manchester school' in original work owed everything to the fertility of Mordell's suggestions, to the keen interest and zest with which he followed their discoveries (even the least significant),

and to the constant inspiration of his own work. It cannot be an easy task to succeed Prof. Hardy; but we can feel confident that the high tradition associated with the Sadleirian chair will be fully maintained.

Prof. E. R. Gilliland

PROF. EDWIN RICHARD GILLILAND, professor of chemical engineering at the Massachusetts Institute of Technology, has been awarded the Leo Hendrick Baekeland Award of the North Jersey Section of the American Chemical Society, "for outstanding achievement in the fields of heat transmission, diffusion, distillation, and high-pressure synthetic chemistry". The award, consisting of 1,000 dollars in cash and a gold medal, is to be given biennially to an American chemist less than forty years of age in recognition of accomplishments in pure or industrial chemistry, and was founded to commemorate the technical and industrial work of the late Dr. Baekeland. Prof. Gilliland, who is thirty-six, is at present working in the Office of Scientific Research and Development, Washington, D.C. He is known as an advocate of the maintenance of a post-war synthetic rubber industry in the United States as essential to the national interest. Prof. Gilliland joined the teaching staff at the Massachusetts Institute of Technology as instructor in 1934, and in 1944 was made a full professor.

Prestwich Prize of the Geological Society of France

PROF. W. B. R. KING, Woodwardian professor of geology in the University of Cambridge, has been awarded the Prestwich Prize of the Geological Society of France. This prize is the only one which the Society can give to a non-member, and has been given to Prof. King in recognition of his distinguished researches and also for his services to France in "la géologie militaire" during the War of 1914–18 and 1939–45 as geological adviser to the British Army.

Medal of the Liverpool Geological Society

THE Liverpool Geological Society has awarded its medal to Dr. Douglas A. Allan, director of the Royal Scottish Museum, Edinburgh, in recognition of his original geological work, mainly on problems connected with the structure and petrology of the Highland Border of Scotland and Angus. The award is also an acknowledgment of the great services rendered to the Society by Dr. Allan during the fifteen years he spent in Liverpool as director of the City museums. He has edited its *Proceedings* for fourteen years, and by his lectures and writings has done much to develop and extend an interest in geological science which has been of great benefit to the Society.

Antonín Bělohoubek (1845–1910)

THE centenary of the birth of the elder of the brothers Bělohoubek falls on April 28. Overcoming initial disabilities, both these Czechs became distinguished chemists and influenced the progress of science in Central Europe. Born at Jeřice in Bohemia, Antonín Bělohoubek was the pupil of Balling and began his scientific career as a demonstrator at the Prague Technical College in 1865. He was appointed *docent* in 1871 and professor of applied chemistry and the technology of fermentation in 1880. In this field he was a pioneer in utilizing the microscope to study biochemical problems. His published researches, mostly conducted during the 'seventies and 'eighties of last century, relate mainly to contemporary analytical procedure. From chemical and bacteriological investigations of thermal and river waters he was led to make geological surveys, and to take an interest in water purification and the disposal of industrial fluids.

Antonín Bělohoubek came before the general public in Central Europe in 1887 during the controversy concerning the genuineness of certain old Czech manuscripts found at Kraluv Dvůr in 1818, for he was the expert called in to make the microscopic and chemical examination. He was also the first to show that chloroform could not be obtained from methyl alcohol by the use of bleaching powder. Later, he became director of the chemical section of the Austro-Hungarian Patent Office in Vienna, but eventually returned to Prague, where he died in 1910. His brother, Augustus Bělohoubek (1847–1908), became lecturer in pharmacy at the Prague Technical College and is best known for his comprehensive survey of the solubility of organic compounds. He also wrote a treatise on drugs that had a considerable vogue towards the end of last century.

Exchange of Diagrams and Data between Radiotherapy Centres

MEDICAL physics is rapidly expanding into an important and well-defined branch of applied physics. At the present stage of development much information is becoming available for everyday use in hospitals, but the information is scattered through a number of journals and sometimes difficult of access. Of particular importance is the distribution of radiation throughout the soft tissues of a patient during irradiation with high-voltage X-rays or gamma rays. This distribution of radiation is a complex function of the physical conditions, and recently Prof. W. V. Mayneord published a set of X-ray isodose curves giving data of direct practical application. At a meeting of the Hospital Physicists' Association it was suggested that this idea might be applied to teleradium isodose curves, and to other data. It was thought that often graphs and diagrams which appear in the literature were too small to be useful, and that many workers would like to have full-size copies of the originals. In addition, various institutions accumulate diagrams, nomograms, and data for their own use, which would be very useful to others if an interchange of copies could be arranged.

The Hospital Physicists' Association therefore appointed a sub-committee with Dr. John Read, of the London Hospital, Whitechapel Road, London, E.1, as secretary. This sub-committee has collected lists of diagrams, data, etc., which institutions are willing to lend for reproduction, and prepared a catalogue of copies which can be supplied. The catalogue includes books not easily obtainable, a short loan of which can be arranged. Some idea of the contents of the catalogue may be gathered from the fact that it covers isodose curves of a large variety of size of field and other conditions; distributions of energy in X-ray spectra; isodose curves around radium tubes and needles of various sizes, and the distribution and amounts of radium to be used to obtain uniform fields in various conditions; photo-electric absorption coefficients in various elements; energy absorption in water and other biological media; and tables of electronic functions.

Chemical Society : Annual General Meeting

THE annual general meeting of the Chemical Society was held in London on April 19, the president, Prof. W. N. Haworth, in the chair. During the business portion of the meeting, Prof. Haworth referred to a letter he had received from N. Derzhavin, conveying greetings from men of science in Latvia and directing attention to the crimes committed by the German invaders on the Latvian people, and his proposed reply was read and endorsed. Prof. Haworth also agreed to convey the deep sympathy of the Council to the American Chemical Society on the loss of their great national leader, and also greetings to the Belgian, French and Russian Chemical Societies, expressing joy at the liberation of their countries from the foreign invader. In presenting the Report of Council for 1944, reference was made to the retirement of the general secretary and to the following resolution passed by the Council: "The Council has received with profound regret the news of the coming retirement of Mr. S. E. Carr from the office of General Secretary. It desires to place on record its deep feeling of gratitude for the great services he has rendered to the Society over a period of forty-two years." It was declared that Prof. H. V. A. Briscoe had been elected honorary secretary, and the following as new members of Council: *as Vice-Presidents*, Prof. A. J. Allmand, Prof. J. W. Cook and Prof. I. M. Heilbron; *as Elected Ordinary Members of Council*, Prof. Wilson Baker, Mr. R. P. Bell, Prof. H. T. S. Britton, Dr. D. H. Hey, Dr. B. Jones, Dr. R. J. W. Le Fèvre, Prof. J. M. Robertson, Dr. F. S. Spring, Dr. M. Stacey and Prof. A. R. Todd.

Philosophical Magazine

THE *Philosophical Magazine* is now within two years of celebrating the hundred and fiftieth anniversary of its first publication. The outlook of physical science has changed in the intervening years almost beyond recognition, and it has seemed desirable to the proprietors of the journal to alter the title, while retaining the familiar name of the *Philosophical Magazine*, in such a way as to indicate more clearly the scope of the journal. The old read "The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science, being a continuation of Tulloch's 'Philosophical Magazine'; Nicholson's 'Journal' and Thomson's 'Annals of Philosophy'"! The new title, which appears for the first time in the current number, is "The Philosophical Magazine, a Journal of Theoretical, Experimental and Applied Physics". The journal has had a long and distinguished career and has numbered among its editors some of the most eminent physicists of the nineteenth and twentieth centuries. In its pages have appeared many papers of fundamental importance; during the present century, for example, classic papers by J. J. Thomson, Moseley, Bohr and Rutherford have been published in the journal which, despite a marked increase in the number and publications of the learned societies, still holds an honoured place among its contemporaries.

New Theory of the Origin of the Planets

A BRIEF reference is made in the *Observatory* of December to a new theory of the origin of the planets, which, according to the *Moscow News*, was communicated to the Academy of Sciences of the U.S.S.R. on June 1, by O. J. Schmidt, of Arctic fame—a celebrated mathematician and geophysicist. The theory is based on the rotation of the galaxy, as a result of which two stars sometimes approach so closely that the capture of one by the other occurs, a stable system—a binary—being formed. A star—our sun, for example—passing through one of the meteorite clouds in the Milky Way captures part of the meteorites, and this capture leads to the formation of planets revolving round the sun. It is stated that Schmidt's theory leads to mathematical formulæ which agree with astronomical observation; but it is necessary to suspend judgment until the original paper is available.

Isovol Map of the South Wales Coalfield

AN important contribution to our knowledge of the coals of South Wales has been issued by the Fuel Research Organisation of the Department of Scientific and Industrial Research in the form of an isovol map (1*s*. 3*d*.) and an explanatory booklet (6*d*.) which describes how the map was compiled, its implications and uses (*Fuel Research Survey Paper* No. 56). Both are published by H.M. Stationery Office, 1944. The 'isovols' plotted on the map are lines of equal volatile content and show graphically the results of the determinations of volatile matter in samples of all the seams so far examined. Further editions will be issued as the survey proceeds. Since volatile matter constitutes, especially in South Wales, a valuable index of the physical and chemical nature of the coal, the map summarizes much of the work of the coal survey in this area up to date, and shows, simply and accurately, where each of the widely differing types of coal occurs. The booklet describes methods of analysis and contains a useful discussion of the relationships between volatile percentages, carbon percentages, calorific value and caking properties—all important factors in assessing the value of the coals.

British Simuliidæ

THE Simuliidæ or 'black flies' are represented in Britain by nineteen species. Their larvæ and pupæ are always aquatic, and the adult females are active blood-suckers. In the British Isles the attacks of the adult insects on man are seldom more than a nuisance; but to horses and cattle they cause much irritation and the animals are often compelled to seek shelter at times when they should be feeding or resting. In such circumstances they will not thrive or put on weight as they should do in normal circumstances. A useful guide to these insects entitled "The British Simuliidæ" by Dr. John Smart has recently come to hand. It is published by the Freshwater Biological Association, Wray Castle, Ambleside, from which it can be purchased, price 2*s*. 6*d*. The pamphlet is well printed and admirably illustrated with numerous original figures. By its aid it should be possible to identify not only the adults but also the larvæ and pupæ of these insects.

University Electoral Registers

IN view of the impending general election in Britain, Mr. D. Veale, registration officer, University of Oxford, writing on behalf of all university registration officers, urges every voter for a university constituency who has changed his address since registering as an elector and who has not notified the electoral registration officer of his university of such change, to do so as soon as possible. Prompt response to this appeal would not only save many last-minute applications

for voting papers but would also help to ensure that those who have a right to receive voting papers receive them in time to record their votes.

The Night Sky in May

NEW moon occurs on May 11d. 20h. 21m. U.T., and full moon on May 27d. 01h. 49m. The following conjunctions with the moon take place: May 8d. 16h., Mars 4° N.; May 9d. 11h., Venus 7° N.; May 9d. 23h., Mercury 2° N.; May 15d. 03h., Saturn 0·2° S.; May 20d. 11h., Jupiter 4° S. Occultations of stars brighter than magnitude 6 are as follows: May 15d. 20h. 23·2m., δ Gemi. (*D*); May 15d. 20h. 56·4m., δ Gemi. (*R*); May 16d. 21h. 27·0m., 49 B. Canc. (*D*). The times refer to the latitude of Greenwich and *D* and *R* refer to disappearance and reappearance respectively. Mercury rises about half an hour before sunrise throughout the month and will not be an easy object to observe. It reaches its greatest westerly elongation on May 11. Venus is conspicuous in the eastern sky, rising an hour before the sun on May 1 and about an hour and a quarter before the sun on May 31. The planet is stationary on May 4 and May 24. Jupiter can be seen throughout the early morning hours, setting at 3h. 11m., 2h. 15m. and 1h. 10m. at the beginning, middle and end of the month, respectively. Saturn is becoming more difficult to observe as it sets at midnight on May 1 and at 22h. 26m. on May 31. The η Aquarid meteor shower is due in the early part of May. The meteors appear in the early morning hours but moonlight will interfere with observations.

Comments on Chromosome Structure

IN *Nature* of April 21, p. 471, an article by Dr. I. Manton under this title was published. The accompanying illustration, however, was very unsatisfactory, due apparently to defects in the block. Accordingly a new block was prepared and the illustration is repeated below, with the explanatory legend.

Announcements

SIR JOHN ORR, professor of agriculture in the University of Aberdeen, has been elected a member of Parliament for the Scottish universities, taking the place of Mr. G. A. Morrison, who has resigned.

THE following appointments have been made in the University of London:

Dr. Edmund Giffen, since 1940 director of research at the Institution of Automobile Engineers, to the University chair of civil and mechanical engineering tenable at Queen Mary College, from October 1, 1945.

Prof. Harold Davenport, professor of mathematics at University College, Bangor, to the Astor chair of mathematics at University College, from October 1, 1945.

DR. NORMAN WRIGHT, director of the Hannah Dairy Research Institute, who has been undertaking a survey of the livestock in the Middle East territories during the past five months, has interrupted his tour in order to make a brief visit to Ceylon, where he is to advise the Ceylon Government on the future development of the cattle and dairy industries of the island. Dr. Wright will shortly resume his Middle East tour. He is expected back in Great Britain by the end of June.

THE International Commission on Zoological Nomenclature announces the election of Dr. James L. Peters, curator of birds, Museum of Comparative Zoology at Harvard College (assistant secretary to the Commission), to be vice-president of the Commission in succession to the late Dr. C. W. Stiles, Smithsonian Institution, Washington.

THE following appointments have recently been made in the Colonial Services: J. E. Cobby, to be assistant conservator of forests, Kenya; K. G. N. Willcocks, to be government chemist, Gold Coast; H. Hirst, acting director of agriculture, Malta, to be assistant director of agriculture, Cyprus.

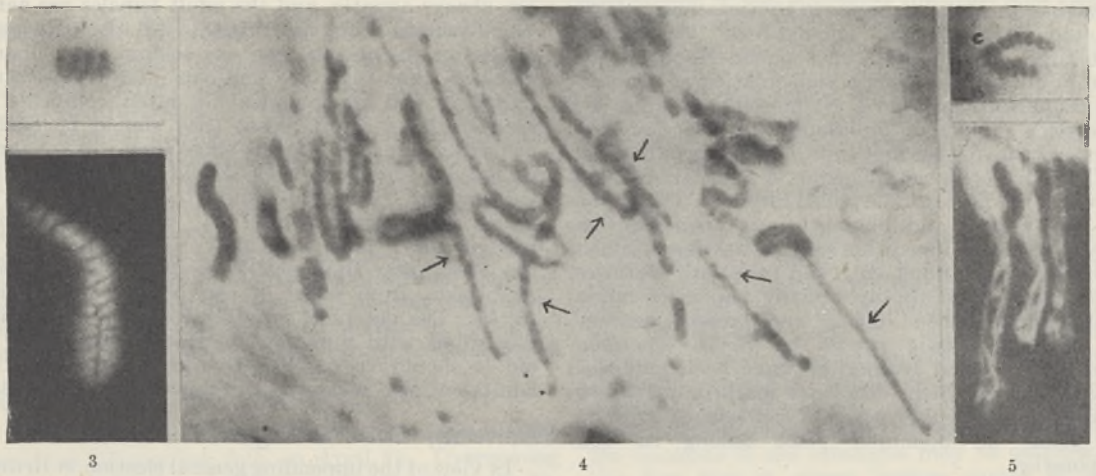

Fig. 1. Unpaired chromosome of *Osmunda* at the first meiotic division after ammonia treatment for spiral structure. Acetocarmine preparation photographed in clove oil by visual light. (× 2000.)

Fig. 2. Split chromosome at the second meiotic division in *Osmunda* after ammonia treatment for spiral structure, the two chromatids are attached only at the centromere (*c*). Acetocarmine preparation, ultra-violet photograph. (× 2000.)

Fig. 3. Split chromosome at metaphase of the third spore division in *Osmunda* showing spiral structure without special treatment. Acetocarmine preparation, ultra-violet photograph (× 4000) negative print (the positive of this and others in Manton and Smiles, 1943 [5]).

Fig. 4. Anaphase of the first spore division in *Osmunda* fixed 30 hours after irradiation of the uninucleate spore with X-rays at 2,500 r. showing fractures, fusions and abnormally clear spiral structure. Acetocarmine preparation, visual light photograph. (× 1000.)

Fig. 5. Anaphase of the third spore division in *Todea* after ammonia treatment, showing lateral separation of component strands. Acetocarmine preparation, ultra-violet photograph (× 4000), negative print (the positive of this and other chromosomes in Manton, 1945 [5]).

LETTERS TO THE EDITORS

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

Kinematical Relativity and the Nebular Red-Shift

Prof. J. B. S. Haldane's far-reaching deductions[1] from Prof. E. A. Milne's "kinematical relativity" excite our admiration, but they must also excite in many a feeling of apprehension lest, in our eagerness to build, we ignore defects in the foundations. Before extrapolating so extensively as Prof. Haldane has done, we should surely ascertain that kinematical relativity can explain what we do know. I have already, in *Nature* and elsewhere, objected to its essentially unscientific character; the purpose of this letter is to point out what seems to me its failure to explain the one fact we have relating to the distant past, namely, the nebular red-shift.

In Prof. Milne's recent letter on "The Ageing of Light"[2] the "usual Doppler effect" is assumed without comment. That letter, of course, is concerned not with the nebular red-shift, which is shown by light originating in the nebulæ, but with the behaviour of hypothetical light originating in our galaxy and supposed to have been reflected to and from the same nebula from the beginning of time until now. Its validity also, however, depends on a satisfactory explanation of an essentially similar phenomenon.

The *observation* is that nebular spectrum lines are less refrangible than lines emitted here and now from (presumably) similar atoms. We all agree to express the problem thus raised by asking why the frequency of old nebular light is smaller than that of young terrestrial light from similar atoms.

Kinematical relativity answers in terms of arbitrarily chosen time-scales, of which two are particularly relevant: (*a*) the *kinematic* scale (*t*-time), in which all 'rigid' bodies are expanding and separating, but the frequency on emission of light from a particular kind of atom (Z) is the same at all times; (*b*) the *dynamical* scale (τ-time), in which the sizes of and distances between rigid bodies remain constant, but the frequency on emission of light from Z is greater the later it is emitted. The red-shift, it is held, is explicable on either scale; but this I cannot see.

Consider first the *t*-scale. The frequency on emission is the same at all times, but the nebula is receding. Only two causes of red-shift are then conceivable: (1) relative motion of earth and nebula; (2) decrease in frequency of *light* (not atomic processes) during passage through space. General relativity accepts (1), but kinematical relativity cannot do so because it holds that even *relative* motion can be transformed away mathematically, and we cannot attribute an objective fact to a cause destructible by "an arbitrary act on the part of the thinker"[3]. Neither is (2) available, for in his comments[4] on Prof. Haldane's paper, Milne says (this seems also to be implied in previous papers, but I have not before seen so unambiguous a statement): "This degradation of the individual photons due to interacting with matter must be distinguished from their constancy of frequency in time (*t*-scale) as they are propagated through empty space". The only possibility left thus seems to be the arbitrary postulate that inter-nebular space contains matter which can affect light in a way unknown elsewhere in Nature. This has not to my knowledge been suggested, and would be a purely *ad hoc*, and therefore worthless, explanation.

The same difficulty in different form is, of course, experienced on the τ-scale. Here the sizes of bodies (and therefore of the atoms which compose them) remain constant, but *atomic frequencies* change with time. Hence atomic frequencies must depend on something unstated which does not affect the linear dimensions, thus apparently ruling out all possibility of correlating radiation with atomic structure. Moreover, if inter-nebular matter is to be admitted, we must suppose its effect on light-frequencies to be exactly neutralized by an *ad hoc* change of light-frequency (on the τ-scale) during propagation through space, for the explanation is complete without change of frequency after emission. But change with propagation, even on the τ-scale, is denied by Milne[5], so the inter-nebular matter essential on the *t*-scale becomes fatal on the τ-scale. Kinematical, unlike general, relativity therefore appears unable to account for the red-shift.

This comparative impotence seems to depend ultimately on the restriction of observations to readings of a single clock, which are insufficient data for describing the full variety of physical experience. Consider a missile which, by ordinary measure, is approaching an observer. By the principle which is held to validate the t and τ scales, the observer can properly regard its distance as remaining constant. He has simply to send a beam of light to and from the missile without pause, and regard its successive returns as equally spaced in time. The ultimate impact then does not occur until $\overline{\tau} = \infty$ on this scale, and so causes no mathematical embarrassment unless the observer survives; but the comfort thus afforded is perhaps questionable. A portion of the observer's experience is made inaccessible in the description given by kinematical relativity, but it is perfectly accessible to him. One cannot help recalling Achilles and the tortoise.

It would, I believe, give satisfaction to many who find a genuine difficulty here if some kinematical relativist would (if possible at no greater length and with no more recourse to mathematics than this letter has required) explain how the theory meets the objection I have tried to express. It is clearly desirable that, before the valuable time of men like Prof. Haldane is further employed in making unverifiable deductions from the theory, we should be assured that the theory can satisfactorily account for what we do experience.

Herbert Dingle.
Imperial College of Science and Technology,
London, S.W.7.
Feb. 26.

[1] *Nature*, 155, 133 (1945).
[2] *Nature*, 155, 234 (1945).
[3] Milne, E. A., *Astrophys. J.*, 91, 157 (1940).
[4] *Nature*, 155, 136 (1945).
[5] *Proc. Roy. Soc.*, A, 158, 328 (1937).

Prof. Dingle makes Prof. Haldane's contribution to kinematical relativity the occasion for what he thinks is an attack on its foundations. He is very simply answered: the red-shifts in the spectra of the galaxies are described in kinematic relativity as a Doppler effect due to recession. The consequences of this in relation to the ageing of light and Prof. Haldane's work have been discussed by me in a letter to *Nature* written before I saw Prof. Dingle's letter.

I see no reason to take up Prof. Dingle's challenge to answer his misunderstandings without the use of mathematics. The consequences which he finds so difficult are *mathematical* deductions from a simple model of the expanding universe; I attach no weight whatever to the verbal descriptions I have occasionally attempted—the core of the matter lies in the mathematics. Prof. Dingle takes from time to time certain English sentences of mine and criticizes their content; but I have never felt that he has understood the theory itself, as embodied in the mathematics. It is no use objecting to the results themselves; the critic should find flaws in the trains of mathematical deduction. When Prof. Dingle finds such flaws, I shall be the first to examine the situation, and to attempt to correct the conclusions. Until then, I have nothing to add to my constructive papers.

Prof. Dingle's attempt to pour ridicule on some of the consequences of kinematic relativity reminds me of Bishop Wilberforce's attempt to discredit the theory of evolution. It is the fate of most essentially new theories to be either ridiculed or ignored. Prof. Dingle should remember, for example, that the ideas of non-Euclidean geometry took a long time to become accepted—they are *prima facie* as absurd as he makes out kinematical relativity to be. I have no personal responsibility for the deduction that the Newtonian scale of time is not the scale of time in which the galaxies are receding; it is an inevitable consequence of the circumstance that the equation of motion of a free particle, at large among the receding galaxies, comes out to be non-Newtonian, but becomes Newtonian when the logarithmic change of time-scale is applied. One of the fundamental problems of physics is the question whether the dynamical scale of time is identical with that corresponding to the recession of the galaxies, and to this problem kinematic relativity finds a solution.

I would add that I have repeatedly shown that clock-readings alone are adequate to give measures of epoch and distance—this lies at the basis of modern determinations of *range* (for war-like purposes) by radar; that general relativity, unlike kinematic relativity, is unable to incorporate the essentially *forward* march of time; and that Prof. Dingle uses an antiquated theory of causation which plays no part in kinematic relativity.

E. A. MILNE.

Relationship between Nuclear Force and Gravitational Force

ALL we know of nuclear force is that it is of the short-range type. Its analytical form is still obscure, and the meson theory of its origin is, up to the present, inconclusive, to say the least[1]. On the other hand, gravitational force is explained by the theory of general relativity. Temporarily disregarding the theory of general relativity and considering only the important fact that both nuclear force and gravitational force are attractive, let us assume that they are of the same origin. Let us tentatively take the function

$$V = -Ae^{K/r} \quad . \quad . \quad . \quad . \quad (1)$$

as the mutual potential between any two nuclear particles; for example, between two protons, two neutrons or one proton and one neutron, with A and K as constants. Then, as the distance r between the particles becomes much larger than K, equation (1) becomes

$$V = -A - \frac{AK}{r} - \quad . \quad . \quad . \quad . \quad (2)$$

In this expression, the first term on the right-hand side is a constant and therefore can be neglected. Thus

$$V = -\frac{AK}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

On the other hand, the gravitational potential between the two nuclear particles of masses M and M' should be given by

$$V = -G\frac{MM'}{r} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If the nuclear force and gravitational force are really of the same origin, we must have, from (3) and (4),

$$AK = GMM' = 1{\cdot}83 \times 10^{-55} \text{ erg. cm.} \quad . \quad (5)$$

since $G = 6{\cdot}66 \times 10^{-8}$ and $M = M' = 1{\cdot}66 \times 10^{-24}$. Recently, Ragan, Kanne and Taschek[2], from their proton-proton scattering experiment, found that the potential between two protons can be represented by a rectangular well of width $2\frac{e^2}{mc^2}$ and depth 10·5 Mev. Now, if from our equation (5) we make K the Compton wave-length of the electron, that is, $K = \hbar/mc = 3{\cdot}83 \times 10^{-11}$ cm., we get $A = 4{\cdot}80 \times 10^{-45}$, and if we substitute these values of K and A into (1), we find the potential $V = 10{\cdot}5$ Mev. at $r = 1{\cdot}51\frac{e^2}{mc^2}$, which is only slightly smaller than the experimental value of $2\frac{e^2}{mc^2}$. In fact, if the potential is not a straight rectangular well but a slightly curved one, for the same depth of potential a smaller value of r would be expected. If, instead of the equation (1), we take

$$V = -\frac{B}{r}e^{K/r} = -\frac{AK}{r}e^{K/r} \quad . \quad . \quad . \quad (6)$$

as the nuclear potential function, we get the result $V = 10{\cdot}5$ Mev. at $r = 1{\cdot}62\frac{e^2}{mc^2}$, which agrees even better with the experimental value. Thus we may tentatively state that

$$\left.\begin{aligned} V &= -\frac{B}{r}e^{K/r} \quad \text{for } r > a \\ V &= -\frac{B}{a}e^{K/a} \quad \text{for } r \leqslant a \end{aligned}\right\} \quad . \quad . \quad . \quad (7)$$

for a certain value of a. Since the value of K is not purely arbitrary, but is chosen as the Compton wave-length of the electron, the agreement between the experimental and the calculated value is perhaps not accidental, and may indicate that the electron, as has long been suggested, plays an important part in nuclear interactions. If it is required to avoid the rather artificial way of 'cutting off' the expression (7) at $r = a$, we give an alternative expression

$$V = -Ae^{\left[\frac{K}{r}-\left(\frac{K'}{r}\right)^n\right]}$$

with n lying between 1 and 2, and K' a new constant which may tentatively be put as the Compton wave-length of the meson, that is, $K' = \frac{\hbar}{m'c} \simeq \frac{\hbar}{180\ mc}$,

but this makes the width of the potential well smaller than $1{\cdot}51\,\frac{e^2}{mc^2}$.

It has been shown by M. S. Wang[3] that satisfactory values for the binding energy of the deuteron and the scattering cross-section of protons by fast neutrons can be obtained if either of the potential function (1) or (6) is used. It may also be mentioned that since, of all the fundamental particles, the neutrino is the only one the interaction of which with others is, up to the present, entirely obscure, it is possible that the gravitational field is simply a neutrino field which alone, or together with the electron field, causes material entities to attract each other.

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[1] See, for example, Bethe, H. A., *Phys. Rev.*, 57, 260, 390 (1940).
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Action of Ionizing Radiations on Carotene and Vitamin A

MANY authors[1] have directed attention to the chemical effects of ionizing radiations, with the view of elucidating the sequence of events between the actual absorption of ionizing radiations in tissues and the biological effects which are known to be produced in radiotherapy. Experiments on enzyme and hormone preparations[2] have shown that the action of ionizing radiations upon aqueous solutions is indirect. It is well known that genetic mutations may be produced by ionizing radiations[3], possibly by a direct effect on the hypothetical macro-molecule of the gene.

Some observations are recorded below on provitamin A (β-carotene) ($C_{40}H_{56}$) and vitamin A ($C_{20}H_{29}OH$), indicating that the effect on these fairly large molecules is direct.

It was observed some time ago[4] that ($\beta + \gamma$) rays from radon in glass capillaries would bleach butter at a range of 8–9 mm. from the capillary. It was later established that the colour of butter is due to carotenoids (*c.* 94 per cent carotene; 6 per cent xanthophyllic). Carotene isomers and vitamin A are normal constituents of human blood. In mammals generally, carotene is converted *in vivo* to vitamin A, relatively large quantities of which are stored in the liver. Apart from quite small amounts of carotene in yellow bone-marrow and body fat, mammals store little carotene, and the normal blood-level is only maintained as the result of day-to-day intake of dietary carotene (green vegetables, carrots, butter, etc.). It is obviously of interest to determine the effect of ionizing radiations used in radiotherapy on carotene and vitamin A.

In one series of experiments, it was established that $\beta + \gamma$ rays from radon in glass capillaries and also γ rays from radium in platinum containers destroyed the carotene and vitamin A in butter. Since butter contains *c.* 15 per cent of water, it was possible that the primary action of the radiation was to activate the water. To test this point, a sample of molten whey butter fat (containing little or no water) was irradiated at 45° C. using a current of nitrogen for the dual purpose of stirring and providing a non-oxidizing atmosphere. The destruction of carotene and vitamin A was complete after exposure to radon in glass capillaries (150 mc. 48 hr.). Further, crystalline β-carotene dissolved in pure *n*-hexane was also destroyed, so that effects due to water and oxygen were excluded. Vitamin A, in the form of a rich ester concentrate dissolved in *n*-hexane, was likewise destroyed. In view of the difficulty of estimating the dose in r. units from $\beta + \gamma$ rays of radon in glass capillaries, parallel experiments using therapeutic doses of X-rays at 220 kV. and 1,000 kV. were carried out with β-carotene in *n*-hexane. In a series of experiments it was found that the 'ionic yield' M/N (M is the number of molecules destroyed; N is the number of ion-pairs produced) was nearly unity, and in one test, at a favourable concentration, exactly unity.

Carotene and vitamin A were present in the butter used in concentrations of 16 I.U./ml. and 20 I.U./ml. respectively (I.U. = 0·6 μgm. β-carotene), and in blood serum at concentrations of 1–2 I.U./ml. and 0·7–1·3 I.U./ml. respectively.

The action of ionizing radiations on carotene concentrations of 1,000, 100, 10 and 1 I.U./ml. has been tested. The proportion M/M_0 (M_0 is number of molecules present initially) of molecules inactivated is greater in the dilute solutions, as would be expected.

In concentrations of 1 I.U./ml., the absorption of only 64 r. of radiation would produce an inactivation of 10 per cent of the molecules. The effect thus approaches in sensitivity Dale's experiments[2] on the inactivation of enzymes in dilute aqueous solution. These results made it feasible to examine *in vitro* the effect of therapeutic doses of ionizing radiation on the carotene and vitamin A of human blood serum. It was found that 6,000 r. of X-rays produced no detectable change, and that exposure to ($\beta + \gamma$) radiation to an extent estimated at 50,000 r. was needed to destroy the carotene. The colour test for vitamin A became quite anomalous ($\lambda_{max.}$ at 595 mμ instead of 617 mμ), which made it difficult to estimate the loss accurately, but probably half the vitamin was destroyed. The nature of the chemical change which causes the colour test maximum to be displaced in this way is imperfectly understood.

The net result of this preliminary work is to establish that there is a direct action of ionizing radiations on provitamin A and vitamin A with an ionic yield of approximately unity in non-aqueous media. The effect is irreversible and is almost certainly accompanied by a loss of physiological activity. In human blood serum the vitamin and provitamin are protected in some way, with the result that by the criterion here adopted, the ionic yield is reduced to 0·01.

Further experiments are in progress and a detailed account will be established elsewhere. One of us (R. A. M.) is indebted to the Medical Research Council for assistance.

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Role of Structural Homo- and Heterozygosity in Mosaic Formation

AN investigation into the nature of mosaicism and the regularities of inheritance of mosaic characters in *Drosophila melanogaster* has revealed two essentials. Mosaicism of the ever-sporting displacement type is always connected with a chromosome re-arrangement involving heterochromatin regions of chromosomes. Insertion of extra heterochromatin of *X*- or *Y*-chromosome or arms of the latter causes strong suppression of mosaicism.

As my investigations have shown, one of the peculiarities of heterochromatin action in mosaic formation is the maternal effect, that is, extra heterochromatin of the mother greatly decreases the development of mosaic individuals among those of her offspring that have no extra heterochromatin. Further investigations have now shown that the influence of heterochromatin in mosaic formation also exists in the form of a paternal effect. The two types of influence are not specific for any one mosaic strain but are inherent in all mosaic forms of the type of ever-sporting displacements.

The discovery of the paternal and maternal effect of heterochromatin in mosaic formation as well as the uniformity of its action was sufficient to suggest some general mechanism for these phenomena. The existing point of view according to which the action of heterochromatin is to be considered as the result of the breaking down of the quantitative balance between the hetero- and eu-chromatin of the chromosomes of the nucleus is not in agreement with these results.

By studying the influence of different heterochromatin components of chromosomes in a number of mosaic strains, I have collected a large body of data which has made it possible to analyse the regularities of the influence of heterochromatin in mosaic formation. The regularities found are as follows: (1) All heterochromatic components of chromosomes are able to suppress the development of mosaic characters. (2) The influence of extra heterochromatic components in mosaic development is found to exist in the form of a maternal, paternal or immediate (direct) effect. The results of the last-mentioned may be complicated by the additive influence of maternal or paternal effects. (3) Simple quantitative relations of heterochromatin action in mosaicism take place only in the case of the immediate effect when maternal or paternal effects are absent. When these latter are involved, simple quantitative regularity of heterochromatin influence in mosaicism breaks down. (4) Maternal and paternal effects do not follow from simple quantitative relations of hetero- and eu-chromatin, their effect being absent or present even if the quantitative relations of hetero- and eu-chromatin are the same. Only in the case of structural homozygosity of heterochromatin components of the chromosome does the maternal or paternal effect of the influence of heterochromatin in mosaicism take place. In the case of structural heterozygosity both are absent.

Structural homozygosity, in my opinion, means a structural identity in relation to heterochromatin components of chromosomes that secures the complete conjugation of the chromosome during meiosis and possibly during mitosis. It is necessary to emphasize that the term 'structural homozygosity' does not necessarily imply recognition of the pairing of heterochromatin regions, but only recognition of the possibility of their conjugation.

The influence of heterochromatin components of chromosomes in mosaicism in the form of maternal or paternal effects is transmitted from the parents to their offspring by the heterochromatin components themselves, no matter whether heterochromatin is present from a *Y*-chromosome or from one of its arms, from an *X*-chromosome or any autosome or from the heterochromatin of a re-arranged chromosome or scute-eight type.

Under the influence of structural homo- and heterozygosity there take place qualitative changes of chromosomes depending upon intracellular processes. I have shown that in homozygous scute-eight females, genetic differences between *X*-chromosomes can be observed. A chromosome transmitted by the father, owing to its structural heterozygosity, is qualitatively different from one transmitted by the mother.

My genetic studies were verified by cytological investigations (by Prokofieva-Belgovskaya). According to these, one of the *X*-chromosomes in homozygous scute-eight females has a *heterochromatized* yellow-achæte region, while in the opposite *X*-chromosome this region is *euchromatized*. These differences may go so far as to make the distal ends of the *X*-chromosome unable to conjugate at all.

Quantitative changes of chromosomes resulting from the influence of structural homo- and heterozygosity are not connected only with mosaic strains. They arise in every stock where the appropriate conditions above mentioned are present. Owing to the instability of its features, mosaicism is only a sensitive indicator, allowing us to detect these qualitative changes of chromosomes.

Since extra heterochromatin leads to euchromatization, it is possible to say that structural homozygosity leads also to euchromatization of chromosomes, while structural heterozygosity leads to heterochromatization of their parts. It is the cause of increased frequency of mosaic individuals under conditions of structural heterozygosity and decreased frequency under conditions of structural homozygosity in genetic structures of type scute-eight. I believe that the phenomenon of heteropycnosis which heterochromosomes undergo in spermatogenesis is the result of structural heterozygosity of *X–Y* composition. *XX* composition, being of homozygous structure, does not show heteropycnosis during oogenesis.

Using the phenomenon of structural homo- and heterozygosity in chromosome changes, I have obtained stable changes that have been transmitted during many generations without any decreasing effect.

The data described above enable us not only to understand the nature of mosaicism and the nature of the influence of heterochromatin in mosaicism but also to regard structural homo- and heterozygosity as one of the essential intracellular factors responsible for causing stable changes in chromosomes and their genetic peculiarities. This throws new light on the problem of hybridization, as a factor of variability responsible not only for recombinations of parental characters, but also for new qualitative changes. The importance of the latter is enhanced in the light of recent data on the presence of intrachromosome inert regions, and on the role of chromosome rearrangements among natural populations.

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Molecular Rearrangement of Arylimino Diaryl Carbonates

IN the course of work on the properties of arylimino diaryl carbonates, we have found that phenylimino diphenyl carbonate (I)[1,2,3] rearranges quantitatively to phenyl diphenyl carbamate (II)[4] on heating at 330° for one hour, or on slow distillation at *c.* 360°. No by-products could be detected, and no indication of reversibility of the rearrangement could be found.

$$\underset{\text{(I)}}{PhN : C(OPh)_2} \rightarrow \underset{\text{(II)}}{Ph_2N.COOPh}$$

This rearrangement of the triad system

$$-N=C-OR \rightarrow -RN-C=O$$

recalls the observations of Chapman[5], who found that N-phenyl benziminophenyl ether, PhC(NPh)OPh, rearranged irreversibly to give N-benzoyl diphenylamine, $PhCONPh_2$, on heating for one hour at 270–300°.

We have been unable to demonstrate a similar rearrangement of the sulphur analogue of (I), phenylimino diphenyl dithiocarbonate $PhN : C(SPh)_2$, prepared from phenyl carbylamine chloride and sodium thiophenate. This compound remained unchanged on heating at 330°, and on distillation at *c.* 370° was recovered unaltered, though a little disruptive decomposition may have occurred, indicated by the odour of thiophenol produced. This finds a parallel in the work of Chapman[6], who found that the sulphur analogue of N-phenyl benziminophenyl ether, namely, PhC(NPh)SPh, remained largely unchanged under conditions which sufficed for the complete rearrangement of the former. At higher temperatures (> 320°), however, decomposition occurred giving products indicative of reversible rearrangement having occurred.

Unsymmetrical analogues of phenylimino diphenylcarbonate, $PhN : C(OR_1)(OR_2)$, where R_1 and R_2 are alkyl or aryl groups, have been prepared via the iminochloride $PhN : CCl(OR)$, and work on their rearrangement, aimed at determining the relative ease of migration of different groups, is in progress and will be reported in full elsewhere.

The rearrangement affords a convenient method for the synthesis of unsymmetrically substituted derivatives of phenyl diphenyl carbamate and hence of diphenylamines.

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Green Pea Juice as a Medium for the Production of Penicillin

AQUEOUS extracts of ground dried peas form a good medium for the production of penicillin[1]. There are, however, certain disadvantages in the use of such material on a large scale, although it forms a very convenient basis for a scheme of fractionation of the active constituents concerned[2]. Last summer we found that a press juice made from entire green peas (seeds and pods) formed an excellent medium for penicillin production.

2,200 gm. of entire peas were put through a juice extractor and yielded 1,450 ml. (1,520 gm.) of juice. A medium was made up containing NaCl 10 gm., $NaNO_3$ 3 gm., KH_2PO_4 0·5 gm., $MgSO_4.7H_2O$ 0·25 gm., lactose 30 gm., pea juice 100 ml. and tap water to 1 litre. The *p*H was 5·8. After mixing well and bringing to the boil, the medium was clarified either by passing through paper pulp or by centrifuging.

The medium was placed either in 40 ml. amounts in 250 ml. conical flasks or 200 ml. amounts in Roux bottles. After autoclaving for 15 min. at 15 lb. pressure the medium was seeded with a culture of *Penicillium notatum* 1,249 *B* 21. The flasks were incubated at 24°. Flasks containing the Coghill medium were set up at the same time.

Penicillin production was assayed both by the dilution method and by Brodie's[3] method. Typical results obtained were: Pea medium 150 units per ml. after 9 days, Coghill medium 150 units per ml. after 9 days. With both media values ranging from 125–225 units per ml. were found in different flasks.

A striking feature of the pea medium is the rapid covering of the surface of the culture medium.

Yields of 80–100 units per ml. were obtained with the diluted pea juice to which only sodium chloride had been added. The residue left after making the pea juice is inactive. The pea juice may be preserved either in a dry form or in the frozen state. The cost of peas to prepare such a medium would be, for peas at 54*s.* per cwt., 9*d.*–10*d.* per gallon of medium.

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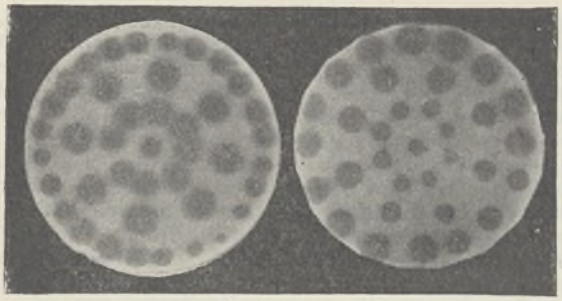
Assay of the Rates of Secretion of Antibiotic in Different Regions of a Growing Mould Colony

IN research on antibiotics, one field is conspicuously blank: that of how a species produces a certain antibiotic. There is no definite answer for any antibiotic to even such elementary questions as: Is the metabolism of the antibiotic linked up with cell multiplication and growth? Is the antibiotic an intra- or extra-cellular metabolite? Is it secreted from the same cells that produce it, or is it secreted from a different part of the mycelium? A technique which may help in solving parts of these problems has now been worked out.

Colonies are grown on disks of permeable 'Cellophane 600' over a very dilute agar medium (for example, Czapek-Dox diluted fifty times). The medium may be kept constant, if the aim of the experiment requires it, by transferring the 'Cellophane' disk with the colony to another Petri dish as often as necessary. When the colony has reached the desired diameter, it is transferred for periods of one hour three or more times on to fresh medium: this removes any appreciable amount of lingering antibiotic. The colony is then transferred on to hard (3 per cent) agar 3 mm. deep and left there for a short time (10 min.–1 hr.): the antibiotic secreted during this time diffuses into the agar. The colony is removed and can be tested again if required. The region of the agar over which the colony was lying is immediately punched all over with a glass tube 3 mm. in diameter. The resulting small cylinders of



(*a*) (*b*)

agar impregnated with antibiotic are then put, face down, on the surface of a testing agar medium (for example, the usual agar-nutrient, *Staphylococcus*). They are arranged in the same disposition as originally, but with proportional increases in the distances between them. The size of the inhibition ring formed after incubation around each agar block gives an estimate of the amount of the antibiotic secreted into the block by the small region of the colony which was in contact with it for the time of testing, that is, an estimate of the *rate* of secretion.

In the accompanying reproductions, (*a*) shows an assay on a 10 cm. Petri dish of a 5 cm. colony of a good strain of *Penicillium notatum*. The rate of secretion of the antibiotic is at its minimum at the centre and the growing edge of the colony; at its maximum about 1 cm. behind the growing edge. A good rate is kept up for a considerable distance towards the older parts of the colony. Assays on a 'poor' strain present the same trend (*b*), but in this case the high secretion is of short duration (growing edge not assayed in this photograph). If synthesis and secretion occurred at the same place, one could deduce that in this species the antibiotic is a metabolic product of already established hyphæ; it is not directly connected with cell-multiplication, and its production falls off in old cells, less rapidly so, however, in a good than in a bad strain. By this technique the different rates of secretion of different 'sectors' of a colony can also be estimated.

I am indebted to Mr. P. Bruce White for a most helpful suggestion without which this technique could not have been refined.

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Hæmoglobins of *Ascaris lumbricoides* var. *suis*

ALTHOUGH hæmoglobin has been recorded in a number of species of parasitic nematodes, the occurrence of the pigment in this class of invertebrates, as in most of the others, is sporadic. Little work has been done upon the properties and function of these nematode hæmoglobins.

Keilin observed[1], in *Ascaris lumbricoides* from the pig, two hæmoglobins which differed from each other, and from the hæmoglobin of the host, in the position of the bands in the absorption spectra. The perienteric fluid and body-wall of the parasite each contained a characteristic hæmoglobin. Some further observations have now been made upon these pigments.

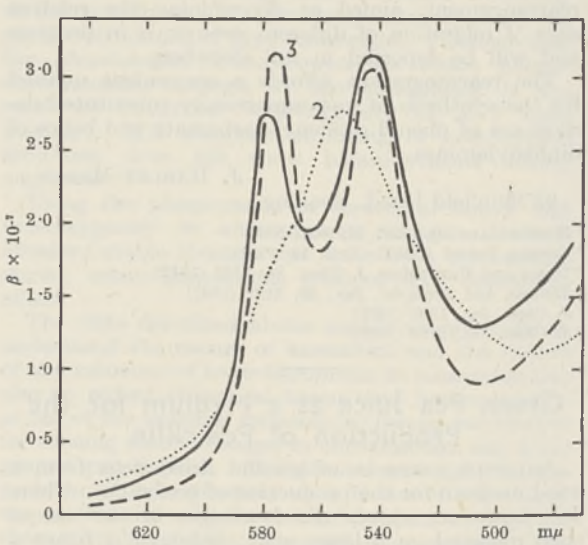
When *Ascaris*, kept under strictly anaerobic conditions, was observed spectroscopically, the oxyhæmoglobin of the body wall was seen to become deoxygenated. Extracts of this hæmoglobin proved to have a very high affinity for oxygen, it being impossible to secure complete deoxygenation of the oxyhæmoglobin by equilibrating against a vacuum at 20° C. Sodium hyposulphite, which rapidly takes up oxygen in solution, produced a slow deoxygenation. Thus *in vacuo* at *p*H 7 and 8° C., 250 sec. (t_{50}) were required for half dissociation of the oxygen from the hæmoglobin.

Spectrophotometric measurements upon concentrated extracts revealed that the body-wall oxyhæmoglobin has a spectrum of unusual type (Curve 1). The α-band is narrower and less intense than the β-band. Keilin and Wang have recently described[2] an oxyhæmoglobin from the root nodules of a leguminous plant which has a spectrum of similar form. The spectra of other animal oxyhæmoglobins which have been described hitherto have bands of approximately equal height, resembling those of the human oxyhæmoglobin shown in Curve 3. The deoxygenated body-wall pigment has a normal spectrum (Curve 2).

A still greater resistance to deoxygenation is shown by the perienteric fluid oxyhæmoglobin. Under the influence of sodium hyposulphite *in vacuo* at 8° C. and *p*H 7, $t_{50} = 1{,}000 \pm 100$ sec. The reaction thus proceeds about four times more slowly than with the body-wall pigment. The effects of various factors upon the velocity of deoxygenation were examined.

(1) An eight-fold variation in the concentration of the reducer was without effect upon the reaction velocity. The reaction is therefore not an oxido-reduction but a true deoxygenation.

(2) Assuming the temperature effect follows the Arrhenius equation, extrapolation from the curve of temperature against reaction velocity to 37° C. gave a value t_{50} of about 15·0 sec. Hartridge and Roughton[3] deduced that, for sheep hæmoglobin, t_{50} at 37° C. was 0·0025 sec.



ABSORPTION SPECTRA OF: (1) OXYHÆMOGLOBIN OF THE BODY WALL OF *Ascaris*. (2) BODY WALL HÆMOGLOBIN. (3) HUMAN OXYHÆMOGLOBIN. ORDINATES. THE ABSORPTION COEFFICIENT $\beta = \frac{1}{cd} \log_e \frac{I_0}{I}$, WHERE c IS THE CONCENTRATION OF HÆMATIN EXPRESSED AS gm. mol./c.c., d IS THE DEPTH OF THE ABSORBING SOLUTION IN cm., I_0/I IS THE RATIO OF THE INTENSITIES OF INCIDENT TO TRANSMITTED LIGHT. ABSCISSÆ ARE WAVE-LENGTH OF LIGHT IN mμ.

The spectrum of the perienteric fluid oxyhæmoglobin is of the same type as that of the body-wall pigment.

The main derivatives of the *Ascaris* pigments were prepared and were found to be related to each other in the manner characteristic of hæmoglobin derivatives. Such differences as were observed in the mode of their formation could be attributed to the low deoxygenation velocity of the oxyhæmoglobins.

Millikan[4,5] has shown that interspecific differences in the oxygen affinities of hæmoglobins are the result chiefly of variations in the velocity of association of the pigments with oxygen. He found the dissociation velocities to be of the same order in hæmoglobins having widely different oxygen affinities. The hæmoglobins of *Ascaris* depart from this rule. Oxygen dissociates from the perienteric fluid hæmoglobin 10,000 times, and from the body-wall hæmoglobin 2,500 times more slowly than it does from sheep hæmoglobin.

The ability of *Ascaris* to deoxygenate the hæmoglobin of the body-wall provides evidence that this pigment may have significance as an oxygen carrier at the low tensions of the gas which prevail in the mammalian gut. Laser has shown[6] that the oxidative enzymic system of the parasite is well adapted to such low tensions of oxygen.

A pigment having similar spectroscopic properties and oxygen relations to that of the perienteric fluid of *Ascaris* was found to occur in the perienteric fluid of *Strongylus* sp. from the horse.

A more detailed account of this work will be published elsewhere.

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'Ant Butter'

Recently my father-in-law, M. Platonoff, asked me whether I could give him any information concerning a soft pliable yellow substance which he had found in the nests of wood ants in the forests of the U.S.S.R. The substance was in the form of small lumps and was sought by the peasants, who termed it 'ant butter' (Mouroveenue Maaslo). He did not know for what purpose the peasants used it. It is said that bears seek the 'ant butter', and a disturbed ants' nest used to be taken as an indication of their presence.

This substance was undoubtedly the ant 'incense' of Linnaeus. The occurrence of ant 'incense' ('wirak') was first recorded by Linnæus[1], who described ants collecting resin from juniper bushes and stated that the peasants gathered the pieces of resin from the nests of *Formica rufa* L. and used it as incense, terming it 'wirak'. Wheeler[2] points out that this is probably derived from the German "Weihrauch", incense. De Geer[3] both records and also figures the resin, and states that the wood ants collect it from the pine and fir trees. He says ". . . the ants collect it [the resin] in little masses of irregular form and varying size, of which the colour is sometimes white, sometimes yellow, and often of a dirty white; the substance is more or less hard except for that which has been amassed fairly recently. . . ."

Réaumur[4] experimented by placing bits of resin in ants' nests, "to see whether they will take on the peculiar quality that makes them resemble myrrh or amber". He also had some correspondence with De Geer on the subject.

Donisthorpe[5] states that "In Northumberland and Scotland these ants (*Formica rufa*, L.) collect huge quantities of yellow resin—'ant amber'—from the fir trees; I have seen nests full of it".

I have often observed wood ants climbing pine trees in the New Forest (presumably to collect resin) and coming down with their crops full, but have never actually seen the 'ant amber'.

The practice of collecting resin seems to be a widespread habit among the wood ants of the coniferous forests and woods of Europe, but I know of no record of this behaviour from America, nor has anyone yet determined for what purpose, if any, the ants use the resin.

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[3] "Memoires", 2, Pt. 2, 1066 (1771), (Fig. 15 of plate 41).
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Protein Content of Earthworms

Certain historical events in 1940 interested us in the possible dietetic value of the protein content of earthworms. We found that two Germans[1] and two repetitive Japanese[2] had shown that all the usual amino-acids produced by hydrolysis of mammalian protein were also obtained from earthworms (*Lumbricus terrestris*), but they gave no quantitative figures of total protein with which we were concerned and which we now report.

Technique. We dealt with common earthworms of a variety of colours and sizes dug from Wiltshire and the London area. At first we analysed worms after keeping them 4–6 days in moss or oatmeal (the scouring process employed by anglers), but found that much 'earth' matter remained, varying from 25 to 45 per cent of the fresh weight. We therefore took large worms, killed them in ethyl alcohol, split and washed them earth-free and dried them roughly to their original moisture. Of these a known weight (sometimes one worm, sometimes two to four) was then desiccated and the nitrogen estimated by Kjeldahl ($P = N \times 6{\cdot}25$).

	1	2	3	4	5
Fresh worm (cleaned) (gm.)	4·66	8·0	5·0	15·0	26·0
Desiccated (gm.)	0·91	1·66	0·95	2·2	3·7
Water (per cent)	80·4	79·0	81·0	86·0	85·0
Protein, dry worm (per cent)	71·5	62·0	62·0	69·0	71·0
Protein, fresh worm (per cent)	13·95	12·9	11·7	9·9	10·0

Total fat estimation (ether and petroleum ether extracts) on three uncleaned samples were 1·3, 1·0 and 0·76 per cent of fresh weight, equals 1·5 per cent average on the de-earthed worm.

The free sugar or total carbohydrate was not estimated.

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[1] Ackermann, D., and Kutscher, F., *Z. Biol.*, 75, 315 (1922).
[2] Murayama, Y., and Aoyama, S., *Yakugakuzasshi*, No. 469, 221 (1921); No. 484, 482 (1922).

RESEARCH ITEMS

Avian Tumour Virus and 'Carrier' Hens

IT is stated that losses in poultry flocks due to neoplastic diseases amount to 20 per cent or more, and despite the urgency of the problem of etiology and control of avian cancer, there is little information which can be used as a basis for prevention. Although it is not known whether all spontaneous tumours are virus-induced, most of the successfully transplanted sarcomas were found to be associated with a causative virus. Susceptibility to Rous No. 1 filterable sarcoma is inherited, and resistant birds frequently respond to infection by this virus by the production of no growth or by a small and transient growth. The virus remains latent in these birds for years after clinical recovery from the infection. Fowl-tumour viruses are disseminated about the body of a host bearing a freshly induced filterable tumour and, although such fowls have only a brief reproductive period left, the virus passes to the egg and hence to the offspring of the tumour-host. The laying capacity of the 'carrier' fowls, on the other hand, appears not to be affected, and it would be expected that their immunity would be passed similarly to their eggs. In attempting to find whether these virus 'carriers' might be responsible for the spread of malignant diseases in such flocks, J. G. Carr (*Proc. Roy. Soc. Edin.*, **52**, B, 54 ; 1944) has investigated the neutralizing ability of the eggs of the 'carriers' and of normal hens, and has found that 'carriers' lay eggs which contain a considerable amount of virus-neutralizing antibody in the yolk. Virus could not be detected in the egg-embryo or chick from such birds, nor did the 'carriers' infect other birds in the same pen with the sarcoma virus. The fact that no neoplasms developed in a flock which, by careful husbandry, had been raised free from parasites or infectious diseases, suggests the elimination of some factor operating in ordinary flocks.

Feeding Apparatus of Biting and Sucking Insects

UNDER the above title, R. E. Snodgrass has contributed an important paper in the Smithsonian Miscellaneous Collections (**104**, No. 7, Oct. 1944). This work is an extension of the same author's previous memoir on the feeding apparatus in biting and disease-carrying flies published in the Smithsonian Miscellaneous Collections for July 1943. The letterpress and illustrations in the last-named paper are included in the new publication, while the additional matter comprises accounts of the feeding organs of Anoplura, Siphonaptera, Thysanoptera and Hemiptera. Dr. Snodgrass's work supplements, and also brings up to date, the accounts given of those organs in present-day text-books. It makes available a thoroughly modern interpretation of the organs of feeding among both biting and sucking insects. For this reason it will be found to be a valuable aid to students and teachers of medical entomology in particular. The general entomologist also cannot afford to pass over this paper since the author gives an entirely new interpretation of the morphology of the mouth-parts in the Siphonaptera. The actual organs used for piercing, it is claimed, are not the mandibles, as almost every authority has maintained for many years past, but are the laciniæ of the maxillæ. A further point of importance is that the median stylet, almost always regarded as the labrum, is interpreted as being the hypopharynx. It is maintained that blood is sucked up through the fine channel in this organ and so reaches the gut. This interesting, and by no means improbable, re-interpretation of the mouth-parts of Siphonaptera must wait the verdict of future embryological study as a likely means of testing its claims.

Germ Cell Cycle in Trematodes

RECENT work by W. W. Cort on reproduction of the intermediate stages of digenetic trematodes supports the germinal lineage hypothesis of multiplication of the cells of the germinal line (*Quart. Rev. Biol.*, **19**, No. 4 ; Dec. 1944). It is a polyembryony by which great numbers of germinal cells are produced from the original zygote. These germinal cells remain distinct from the soma of the germinal sacs (sporocysts and rediæ) and come to lie in their body cavities. Finally, the germinal cells form the adult gonads and the polyembryony is succeeded by gametogenesis with the production of haploid spermatozoa and ova. The germinal lineal hypothesis maintains that these gametes are the only cells in the entire life-cycle that have the reduced number of chromosomes. The theory therefore disagrees with the early explanation of Steenstrup (1842), who said that reproduction in sporocysts and rediæ was a process of asexual budding ; with Grobben (1882) who maintained that it was parthenogenesis, and Woodhead (1931) who described in the Bucephalidæ ovaries and testes of sporocysts and rediæ and who believed that true bisexual reproduction occurred even in the intermediate stages.

Vitamin C in Drumstick Leaf

OF the many rich natural sources of ascorbic acid, some, such as lucerne, rose hip, blackcurrant and gooseberry, have been successfully processed to yield concentrates which can be used in small quantities to meet human daily requirements of this vitamin. In a communication to the editors, T. B. Panse and A. Sreenivasan, of the Department of Chemical Technology, University of Bombay, now suggest adding the leaf of the drumstick (*Moringa oleifera*) to this list since the leaf by titrimetric estimation contains 900–1,100 mgm. ascorbic acid per 100 gm. Furthermore the leaf provides in addition 100–120 mgm. of β-carotene per 100 gm. and is thus richer in this respect than either lucerne or rose hip. Aqueous extracts of drumstick leaves, if sampled during the pre-flowering stage, are reasonably stable, losing only 25 per cent of their ascorbic acid after three days storage. On the other hand, a powerful oxidase system is apparently developed in the leaves when the tree is in flower, so that larger amounts of the vitamin are lost during the extraction. This is particularly marked with the flowers themselves, the ascorbic acid of which is practically instantaneously destroyed in the aqueous extract.

Physiological Specialization of Oat Stem Rust

Newton and Johnson (*Can. J. Research*, Sec. C, **22**, Oct. 1944) have identified twelve physiological races of *Puccinia graminis Avenæ* in Canada from 2,586 isolates studied during the period 1921–43. Annual surveys of the prevalence of physiological races during this period show that each year races 1, 2 and 5 have comprised the bulk of the oat stem rust in Canada. The predominance of these races has been greatest in the three Prairie Provinces, where barberry is virtually non-existent. In regions where barberry is present, other races of greater range of pathogenicity have been found more frequently. The

occurrence of such races, however, was sporadic until 1943, in which year races 8, 10 and 11 attained a wide distribution, apparently traceable to wind-borne uredospores from the south. There is evidence that the strains of these races present in 1943 remain in the uredospore stage for much longer periods than do strains of the same races collected in previous years, a fact that may have favoured their spread that year. The role of the barberry in the origination of generally virulent physiological races is discussed. The possibility that such races may also originate by mutation is suggested by spontaneous pathogenic changes that occurred in the greenhouse, in a culture of race 3 that gave rise to several cultures of race 7.

Effect of Stock on Grafted Plants

'A LOVER of planting' in the "Complete Planter and Cyderist" (1685) writes: "It's manifest that amongst fruit trees of one kind in the same orchard some shall bear better fruit than any of the rest and it's not known what to impute this excellency to than to the rootstocks they were grafted on". The variability in tree growth and development and in fruit quality is greatly reduced by grafting the trees on to clonal rootstocks vegetatively raised and hence of uniform genetical constitution. Nevertheless, trees grafted on to seedling stocks of varying genetical constitution are often remarkably uniform in character. Especially does this seem to be so under American practice, where trees are generally bench-grafted on to 'root pieces' so that the root-stock does not include any stem piece, and R. H. Sudds and P. C. Marth (*Proc. Amer. Soc. Hort. Sci.*, 42, 326; 1943) find that trees on seedling stocks are almost as uniform over a period of nine years as those on a number of clonal stocks, while E. W. Greve (*ibid.*, 337) in experiments with five varieties of apples on their own roots and on seedling stocks reports no greater variability in the grafted trees than in the 'own rooted' ones.

Theory of the Focault Test

S. C. B. GASCOIGNE has discussed (*Mon. Not. Roy. Astro. Soc.*, 104, 326; 1944) a diffraction theory for the Focault knife-edge test. His paper is highly mathematical, and only a brief summary of it is possible. A short account of the theory of the Focault test is given and then a closed expression for the variation of intensity over the mirror with arbitrary error, not necessarily small compared with λ, when it is tested with a knife-edge, is derived. From the equation giving the disturbance $D(x)$, it is shown how the usual explanation of the knife-edge test follows from the diffraction theory by considering the limit of the equation when $\lambda \to 0$. Examples are then given of a number of commonly occurring errors. The well-known phenomenon of the appearance of fringes on a mirror with a turned edge is explained, and intensity curves for mirrors with central and zonal errors are computed. Most of the work was carried out during the author's tenure of the Michael Hiatt Baker Scholarship, University of Bristol.

Chain Reactions

THE breaking of chains on the walls of the vessel is one of the most characteristic and essential features of chain reactions and was introduced by Semenoff in 1927 and by Hinshelwood in 1928, its existence being quantitatively proved by Trifonoff in 1929. Since then, many mathematical and experimental memoirs on the subject have appeared. N. N. Semenoff (*Acta Physicochim. U.R.S.S.*, 18, 93; 1943) has now presented a clear account of the theory in a form convenient for applications, partly because he has himself found difficulty in working out experimental results, and partly because serious errors have been made in mathematical papers by other authors. A careful consideration of the possibility of small values of the chain-breaking probability ε is presented, and results for spherical and cylindrical vessels are worked out. The effect of inert gases is also considered, and some attention is given to the action of solid dusts as 'anti-knocks'. It is, of course, quite impossible to summarize a paper of this character; but attention is directed to it as an important contribution to the literature of chain reactions.

Permanent Magnets

THE economic utilization of modern permanent magnets was the subject of a paper read by D. J. Desmond on February 16 before the Institution of Electrical Engineers. In this paper the author first establishes the equation to the demagnetization curve and then proceeds to discuss the uses of permanent magnets in typical pieces of apparatus. The working of the magnet under these various conditions is considered and the useful part of the magnetic energy is calculated, this introducing a new method by making use of the unit permeance of a circuit. Certain approximations are made in this calculation and the limitations of the simple theory are then discussed. A method is given of designing a magnet in terms of the constants of the iron circuit. Figures are provided for two modern alloys in common use, and curves are plotted for the complete solution to all design problems. The interchangeability of these two alloys is discussed, and it is pointed out that not all the additional energy of the anisotropic alloy can usefully be employed. This is due to the larger curve factor, which reduces the recovery when a demagnetizing force is removed. It is shown that the $(BH)_{max.}$ value is not the criterion of usefulness of a magnet, except in the simplest case, nor is it necessary for the magnet to work at the $(BH)_{max.}$ point.

Radioactivity of Sedimentary Rocks

A RAPID method for the determination of uranium, thorium and potassium in rocks (based on the use of β-ray counters calibrated with radioactive standards) has been devised by R. F. Beers and C. Goodman, and applied to more than three hundred samples of various types of sediments from oil wells drilled through Palæozoic formations (*Bull. Geol. Soc., America*, 55, 1229; 1944). The results indicate that the principal loci of radioactivity are in (*a*) heavy minerals in sands and sandstones; (*b*) potash-rich sediments; and (*c*) sediments of colloidal deposition. It is shown that organic black shales show excellent correlation of uranium content, organic matter, abundance of grades of colloidal size, and a high ratio of thorium to uranium. All these correlations have an important bearing on genetic relationships in petroleum source beds. Goodman's prediction (*J. App. Phys.*, 13, 276; 1942) that petroleum source beds should have high thorium-uranium values has been supported. Evidence is recorded showing that in primary igneous rocks and organic black shales, potassium and uranium increase directly with one another. It is also established that rocks of high uranium and thorium content possess high emanating power, a feature which complicates the technique of the radioactivity measurements.

THE DISPLACEMENT METHOD OF WEIGHING LIVING AQUATIC ORGANISMS

By ASHLEY G. LOWNDES

IN 1927 a method was devised by which living aquatic organisms can be weighed without removing them from their environment. Two accounts of the method were published in 1938[1,2], but a much fuller account of the technique was published in 1942[3] and 1943[4]. In outline the method is very simple since, in the case of a marine animal such as a prawn, it consists of placing it in a suitable density bottle which is otherwise full of sea water and weighing the whole. The exact volume of the bottle and the density of the sea water are readily obtainable, and it is then only necessary to know the exact volume of the prawn to ascertain its weight.

The exact volume of the prawn can also be obtained. The bottle is filled with sea water and the contents poured into excess of silver nitrate, and the silver halide weighed. The experiment is then repeated, but with the prawn in the bottle. From the weights of the two lots of halide, the volume of the prawn is obtained with considerable accuracy. Alternatively, the sea water can be poured into a standard flask and an aliquot part titrated. The prawn itself is retained by some suitable means and rinsed with a suitable non-halide solution which is at the same time isotonic with the sea water.

In actual practice the technical details are rather laborious, but the method lends itself to extreme accuracy. It has been reviewed in *Nature*[5]; but since the accuracy of the results obtained caused at one time considerable criticism, a full account of the method was submitted to the Director of the National Physical Laboratory, to whom I wish to extend my sincere thanks, and a report obtained from Mr. Verney Stott, who stated that errors should occur in the fourth place of decimals *only*.

Actually the limiting factor does not lie in the technique, but in the accuracy with which it is possible to obtain the density of the sea water in circulation in the aquarium tanks. This could be filtered and the dissolved air driven off by boiling, but the resulting sea water would then no longer represent the real external environment of the organisms.

The method is applicable to freshwater organisms but a rather different technique is necessary. Large crabs and lobsters weighing several pounds have been tackled, as well as nursehounds up to 4 ft. in length. At the other end of the scale, a single specimen of the second larva of the common lobster has been weighed, which amounted to 0·0074 gm.; and by taking a large number of specimens such as are obtained from an artificial fertilization of sea-urchins, it is possible to find the density of the various embryonic stages.

Some Applications of the Method. First and foremost, there is the question of analysis, for clearly you can only analyse correctly provided you can first weigh correctly. Usually all analysis has been carried out on air-dried or oven-dried material, and hence many of the results are open to considerable criticism.

Then there is the relationship between the organism and its external environment, or what is known as the *sinking factor*. The impression has been given by some biologists that a marine organism, since it lives in an environment with a density of about 1·03, is unaffected by gravity. Thus to quote one instance, "Fishes, *by evading gravity*, increase their range of magnitude both above and below that of terrestrial animals"[6]. True, on *a priori* grounds, we might expect that those organisms with the lowest sinking factor would be the most consistent in their swimming or floating habits, but direct measurement shows that no such simple direct relationship exists.

A consideration of density would have prevented many of the existing fallacies from permeating current biological literature to the extent to which they do at present. Thus for years it has been stated and accepted that the common jelly-fish comprises 99·8 per cent water. This is, of course, clearly impossible, since it implies that the remaining 0·2 per cent would have to have a density of 16 or so.

Again, Stokes's Law has been used for trying to find the density of such diverse material as the bits of a squashed egg of a sea-urchin, and a narcotized Calanoid or a Cladoceran the surface of which is practically covered with spines; yet some of the first conditions for the application of the law are that the particles must be smooth and spherical, and the radius known or at least measurable.

If, on the other hand, we can weigh accurately a living Calanoid and obtain its density, and then find its rate of sinking through unadulterated sea water, we should be able to get a close approximation to the amount of energy needed to keep such an animal near the surface, or possibly the energy expended during rapid swimming movements.

Finally, there is the question of the resolution of forces about the centre of gravity. If the tissues are more or less homogeneous and arranged more or less symmetrically about a point, that point will constitute the centre of gravity of the organism. But, if such an organism is situated in an external environment the density of which is equal to the density of the whole organism, then there can be no *functional* centre of gravity. This seldom occurs in actual fact, but many of the teleost fishes with large central swim-bladders approximate closely to it.

Embryology. Embryologists still talk about the high density of 'yolk' and its effect on segmentation. The density of the yolk of the hen's egg is, however, 1·025, while that of the dogfish egg is 1·1. Thus the density of the yolk in the one far exceeds the density of protoplasm, while in the other it is considerably below it; yet in both cases the segmentation is meroblastic and this is always attributed to the presence of the yolk. Alternatively, the ova of *Echinus* are small and have a low density and very little yolk, while the ova of the spider crab *Maia squinado* are very much larger and have a density greater than 1·1; yet in both cases the segmentation is holoblastic and equal, or at any rate it appears to be so.

Evolution. The real interest of the method, however, lies in the light that it may possibly throw on the primary cause of evolution. Almost ever since the publication of the "Origin of Species", most biologists seem to have regarded evolution as exclusively a biological process, with most disastrous effects on the study of biology itself. It is true that Herbert Spencer was careful to describe the origin of organisms and the increase in complexity of tissues as *organic evolution*, but his example has certainly not been followed recently. Moreover, the still worse error of regarding natural selection or the struggle for existence between individuals as one of the primary causes of organic evolution is still rampant. Though natural selection has now been cleared of much of

superfluous material and constitutes a sound and mathematical concept, chiefly owing to the work of R. A. Fisher, yet, whatever the importance of the process within organic evolution itself, it was not a primary cause.

Clearly cosmic or inorganic evolution must have been at work long before organic evolution ever started, and the primary cause of organic evolution is to be sought for within the cause of inorganic evolution. It is true that the origin of the universe or even of the solar system is still a much debated question, but no scientific theory yet put forward disregards the law of gravitation, and most include either the concept of maximum entropy or of thermodynamic potential. Clearly none of these concepts is applicable to organisms or tissues of unknown weight or density. One very elementary theoretical outcome of this view may be cited briefly. Without disregarding the fact that there may be no real distinction between the living and non-living in the initial stages, we are justified in assuming that the first *free-swimming* organisms consisted of protoplasm and existed near the surface of water. Since, however, the density of protoplasm must always exceed that of the water constituting its external environment, the first struggle for existence must have been influenced by gravity and not primarily by competition between individuals. It would appear, therefore, that the accurate determination of weight and density is of fundamental importance for an accurate study of aquatic organisms.

[1] *Nature*, 141, 289 (1938).
[2] Lowndes, *Proc. Linn. Soc., London*, 150, Pt. 2 (1937–38).
[3] Lowndes, *J. Mar. Biol. Assoc.*, 25, No. 3, 555 (1942).
[4] Lowndes, *Proc. Zool. Soc. London*, A, 113, 28 (1943).
[5] *Nature*, 150, 695 (1942).
[6] Thompson, Sir D'Arcy, "Growth and Form", 67 (Cambridge, 1942).

THE HORSE-CHESTNUT TREE *(AESCULUS HIPPOCASTANUM)*

By ALEXANDER L. HOWARD

"Oh ! there the chestnuts, summer through,
Beside the river make for you
A tunnel of green gloom, and sleep
Deeply above."

RUPERT BROOKE.

THE horse-chestnut tree is not indigenous to Great Britain but was introduced in the middle of the sixteenth century ; there is some doubt as to its origin, but the general opinion is that it was a native of the Balkan mountains, or perhaps that it was introduced there from Iran, northern India or Tibet. Gerrard speaks of the first tree being heard of in 1579, and from this date it became generally planted throughout western Europe, especially in Sardinia, Sicily and Corsica. In the most favourable circumstances it may attain a height of about 120 ft., with a girth of 18 ft., and in Ireland Elwes mentions a tree at Woodstock, Kilkenny, which was 93 ft. in height and 18 ft. 1 in. in girth. The tree is well known to men, women and children, the former principally for the beauty of its flowers in the early summer, and the latter who wait impatiently for the autumn when they can throw sticks at the branches to dislodge the fruits which contain the seeds or "conkers".

The horse-chestnut (*Aesculus hippocastanum*) is often mistakenly confused with the sweet-chestnut (*Castanea sativa*) to which it is not related either in fashion or family. C. A. Johns says that the origin of the name is in doubt. "The name Aesculus, from *esca*, food, was applied originally to a species of Oak, which, according to Pliny, was highly prized for its acorns, but how it came to be transferred to the Horse Chestnut is very uncertain ; perhaps, as Loudon suggests, it was given ironically because its nuts, which are unfit for food, bear a great resemblance externally to those of the Spanish Chestnut. *Hippocastanum* is a translation of its modern name, which was given 'from its curing horses broken-winded and other cattle of coughs' " (Evelyn).

Pliny, according to the translation by Dr. Philemon Holland, does not mention horse-chestnut, but he describes what he calls Esculus, terming it oak. This does not apply to the oak but fits the horse-chestnut. In his time the mountains of Greece abounded with these trees, and it is thought that this was its native habitat. It was introduced and freely planted in Sardinia, Sicily, and Corsica, and Pliny refers to what he calls "the Sardinian nut". Whether by this name he was speaking of the sweet-chestnut or the horse-chestnut is not clear ; but it is probable that he was referring to the sweet-chestnut. It seems evident that in his time the tree was valued for its fruit but not for its timber. He says :

"The best mast (nut) and the biggest is the acorne growing upon the common Oke, next to it is that of the Esculus ; as for that of the Robur it is but small. The cerrus carrieth a mast unpleasant to the eie, and rough to be handled, for clad it is with a cup beset with sharpe prick like to the Chestnut tree. . . .

We entitule Chestens also by the name of Nuts, although indeed they are more aptly to be called a kind of Mast. This fruit whatever it be, is enclosed within a huske, and the same defended and armed all over with a rampier and palaisade (as it were) of sharpe pricks like the skin of an Urchin ; whereas the Acorne and other Mast is but half covered, and that defence in them is begun only. And certes, a wonderful matter it is, that we set little store by this fruit, which Nature is so careful to hide and defend. Under one of these husks ye shall find sometime three Chestnuts, and those having certain tough pills or shells very pliable. But the skin or filme within, and which is next to the bodie or substance of the fruit, unless it bee pilled off and taken away, marroth the tast of it, like as it doth also in other nut kernels. Chestnuts, if they be rosted, are better and more pleasant meat than otherwise. They use also to grind them to meale, and thereof is made a kind of bread that poore women for hunger will eat. The first Chestnuts were knowne to grow about Sardis and from thence were brought and therefore the Greeks call them Sardinian nuts."

It is impossible to decide for certain whether he was speaking of the sweet-chestnut or the horse-chestnut, but it seems probable that although his description would apply equally to the nuts of either tree, he must have been referring to the former.

At the time of John Evelyn it became fashionable to plant the horse-chestnut in avenues, and many famous examples dating from this time can still be seen in different parts of the British Isles—as well as on the Continent, in private and public parks and gardens. In this way the chestnut shows its full beauty and dignity at all seasons of the year, but specially when its candle-like flowers are in full bloom. One of the most famous chestnut avenues in England is at Bushey Park, Hampton Court, which is visited year by year by many people on Chestnut Sunday.

However it is situated, the horse-chestnut is the

first of our trees to clothe itself with green leaves, which are different in shape from those of our other forest trees, having five to seven leaflets, 4–9 in. long, and brilliant light green in early spring, deepening to a heavy darker shade and turning to golden yellow before they fall in early autumn.

The bark is smooth of a greenish-brown colour, and the crown generally becomes pyramidal in shape. Sometimes the branches droop, and in cases dig into the ground all round the base of the tree, whence they take root and spring up again, often attaining a much increased girth in the upward growth than that proceeding from the tree in the downward bend. A remarkable tree of this habit is in vigorous health at Hawkhurst Moor, with a height of about 90 ft., and covering the ground to a circumference of 285 ft.

The yellow horse-chestnut (*Aesculus octandra*) was first introduced into England in 1764. The red horse-chestnut (*Ae. carnea*) was, according to Elwes, first introduced from Germany in 1818. Both may be seen in almost every garden and park, especially around London and other cities. Neither of these trees attains the dimensions of the common horse-chestnut (*Ae. hippocastanum*).

Ae. pavia, a shrub bearing a red flower, is referred to by Elwes, who says "it closely resembles Aesculus octandra" and supposed to have produced with *Ae. hippocastanum* the hybrid *Ae. carnea*. Loudon describes this as "The Pavia, or smooth-fruited Horse-chestnut tree" and also mentions *P. rubra*, but the whole subject is so confused that it is difficult, if not impossible, to state accurately the botanical variations.

Both the horse-chestnut and the sweet-chestnut, for some unexplained reason, are subject to spiral growth in the British Isles. During the War of 1914–18, the subject of spiral growth came very much to the fore, as apparently sitka spruce (*Picea sitchensis*) imported from British Columbia was found to carry this defect more than any other tree for which there was an equal demand. Inquiry into the subject brought suggestions, one of which was that it was a hereditary feature. Although this may be possible, I am more inclined to think that spiral growth is caused by the situation of the tree and its relation to the sun and wind. In Hyde Park, in the triangular piece of ground between the Magazine and the Epstein statue, by the Serpentine, there are twenty-four horse-chestnut trees, all but two of which show spiral growth, a number proportionately much greater than in any other part of the Park. This site must be subject to varying winds from different quarters, while the rays of the sun may influence the direction of the growth. At the same time, it is strange that one particular tree should be subject to more spiral growth than another—a subject well worth inquiry.

About 1920, shipments arrived in London, and probably Paris, Hamburg, and Rotterdam, of Japanese horse-chestnut (*Aesculus turbinata*—the Japanese name for which is 'Tochi-noki'). These shipments consisted of large-sized slabs, or flitches and planks. The wood was similar in colour and texture to that grown in England, but mainly consisted of highly figured wood, rich in roe and splash-mottle, some bright and clean, while other was streaked with black marks—denoting incipient decay. All this timber was equally sound and good, while that streaked with black was particularly decorative, and was greatly in demand for art furniture in which it realized high values.

Speaking of the British variety, Elwes mistakenly says :

"The wood of the Horse Chestnut is one of the poorest and least valuable we have, on account of its softness and want of strength and durability. Though it has a fine close and even grain, white or yellowish-white colour, and is not liable to twist or warp so much as most woods, it does not cut cleanly."

Elwes cannot have known the many purposes for which chestnut has been used.

Quoting from "Timbers of the World" :

"If the tree be cut down early in winter, promptly sawn into boards, etc., and carefully stored, wood of extreme whiteness may be obtained. If, however, the trunks are felled later in winter, or are allowed to lie for any length of time, the wood assumes a yellowish-brown tint. It is of moderate weight, soft fine-grained, but perishable ; used for making soap-bowls, brush-backs, in turnery, and occasionally for veneers. Even with the developments which have occurred since the war (1914–1918), supplies of horse chestnut are not sufficiently valued or appreciated. In Southern Europe it is said to have been used for fruit-storing shelves ; the porous nature of the wood absorbs the moisture from the fruit, the preservation of which is thereby assisted."

Besides its use for gunpowder and charcoal, the timber is most valuable for furniture and works of decorative art, for which purpose it has been by far too little appreciated.

STATISTICS IN GOVERNMENT DEPARTMENTS

A MEMORANDUM on the Post-War Organization of Statistics in Government Departments, which has been prepared by the Institution of Professional Civil Servants for submission to the Treasury, draws largely on material in the Memorandum on Official Statistics issued by the Royal Statistical Society, with the recommendations of which the Institution to a considerable extent agrees, apart from certain proposals with regard to the structure of the statistical service and the status of its officers. The Institution's own proposals are set out under five headings. First, with regard to the functions of a statistical service, it is considered that the work of the statistical service should be an integral part of the administration, including such public functions as the collection, presentation and publication of a continuous series of statistics relating to the economic and social condition of the nation ; the interpretation and enlargement for publication of material already covered in part by existing statistics, together with explorations into fields not hitherto covered, so as to provide industry and trade with information which by its nature can only be collected through the agency of the Government ; and research into the statistical aspects of social questions. One internal function should be provision of the pre-requisites of policy formation, and the memorandum claims that the statistician, in providing material on the quantitative aspects of a problem, should be called upon to play his part in deciding how things planned for the future are to be attained.

Next, with regard to grades, salary, promotion and superannuation of statistical staff, the Institution urges that there should be only two classes of statistician ; a principal statistician class, with career value comparable with that of the administrative class of the Civil Service, and an executive

statistician class, working normally under the direction of members of the principal class and with a career value comparable with that of the executive class. Superannuation should be provided on terms comparable with those available to the rest of the Civil Service, and in addition to facilities for post-entry training and for opportunities for widening experience by seconding for limited periods to statistical posts outside the Government service, the Institution emphasizes the desirability of transfer of statisticians to administrative posts, as well as of transfer between departments and with the Central Statistical Office. The views of the Royal Statistical Society on the value of mechanical aids are endorsed.

With regard to the qualifications and recruitment of statistical staff, the memorandum welcomes the scheme to set up a certificate and diploma in statistics proposed by the Royal Statistical Society, and although the syllabus appears to demand a rather higher standard of mathematical statistics than would in general be required for entry into Government service, possession of the diploma is suggested as alternative qualification to a university degree with first- or second-class honours in the relevant subject, including statistics, for eligibility for appointment to the principal statistician class. For the executive statistician class, the higher school certificate with distinction in statistics, or the Royal Statistical Society's certificate is suggested. A central recruiting body for statistical officers should certify the candidate's eligibility for appointment, and appointments should be made by a selection board from eligible candidates on the basis of interview, the Board including representatives of the central recruiting body, the Establishment Department, the Institution of Professional Civil Servants and the head of the statistical section of the department concerned. These conditions are not intended to apply to existing members of the Government statistical service or to persons who have done outstanding statistical work elsewhere.

With regard to the transition to the post-war period, sufficient of the existing personnel should be retained to ensure the immediate establishment of the comprehensive statistical service envisaged, and an immediate decision on the questions of post-war organization, recruitment, salaries and promotion is a matter of urgency. With regard to statistical raw material, it is important to ensure that existing collections and compilations are preserved, and pressure from commercial firms in favour of discontinuing the making of returns necessary for statistical continuity should be resisted. Statistical branches should collect basic data from the source.

With regard to the relation between statistical branches and the Central Statistical Office, the Institution examined the suggestion for a Government statisticians' department, but decided that the type of organization recommended by the Royal Statistical Society would give a more efficient statistical service, and accepted the recommendations of the Society's memorandum with regard to the functions of the Central Statistical Office. That Office should maintain a central library and records office staffed by people competent to give advice. It should contain a full record of confidential statistics compiled by all the various Government departments, and available to authorized persons in the Government service. The library would also be the depository for statistical records of Government departments which close down.

TOXIC SPRAY SUBSTANCES AND PLANT GROWTH

ONE result of the continued use of sprays containing toxic inorganic constituents may be the accumulation in the soil of substances deleterious to plant growth. Of practical interest, therefore, are experiments described by N. F. Childers[1], who found that the presence of sodium arsenate in the soil had a stimulating effect on 'Delicious' apple seedlings when the concentration was not more than 20 parts per million. Between 60 and 160 parts per million, toxic effects were apparent, and if the concentration was more than 100 parts per million the seedlings died. Equivalent amounts of arsenic in the form of lead arsenate were less toxic, and lead chloride even at 160 parts per million exerted no deleterious effect. More carefully controlled experiments with plants in gravel culture showed that 20 parts of arsenic per million of culture solution stimulated the growth of apples and of rye but depressed the growth of grapes. Both apples and grapes were tolerant of 200 parts of lead per million of solution and both showed a progressive decrease in growth as the copper content of the solution rose from 10 to 30 parts per million.

Different fruits show differing tolerance of arsenic, but R. C. Linder[2] reports that peaches are especially susceptible, and if the arsenic content of the leaves exceeds 2 parts per million of dry matter, marginal and interveinal burning and shot-holing of the leaves occur, with, in severe cases, defoliation. Apricots show a similar effect, but plums, pears and apples are more resistant, and plum foliage containing as much as 13 parts of arsenic per million of dry matter appeared healthy.

[1] *Proc. Amer. Soc. Hort. Sci.*, **38**, 157 (1941).
[2] *Proc. Amer. Soc. Hort. Sci.*, **42**, 275 (1943).

FORTHCOMING EVENTS

Saturday, April 28

NUTRITION SOCIETY (joint meeting with the BIOCHEMICAL SOCIETY) (at the London School of Hygiene, Keppel Street, Gower Street, London, W.C.1), at 11 a.m.—Discussion on "The Vitamin-B Complex".

INSTITUTE OF PHYSICS (ELECTRONICS GROUP) (joint meeting with the MIDLAND BRANCH) (at the University, Edmund Street, Birmingham), at 2.30 p.m.—Dr. H. Kuhn: "Atomic and Molecular Beams".

Sunday, April 29

ASSOCIATION OF AUSTRIAN ENGINEERS, CHEMISTS AND SCIENTIFIC WORKERS IN GREAT BRITAIN (at the Austrian Centre, 69 Eton Avenue, Hampstead, London, N.W.3), at 11.30 a.m.—Dr. B. Burztyn: "On Recent Applications of Thermo-setting Resins on Paper and Textiles".

Monday, April 30

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 1.45 p.m.—Sir Frank Smith, G.C.B., F.R.S.: "Chemicals from Petroleum" (Cantor Lectures, 3).

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (Mining Institute, Newcastle-upon-Tyne), at 6 p.m.—Sir Amos Ayre, K.B.E.: "An Approximate and Simple Formula concerning Four-Bladed Propellers of Single-Screw Cargo Ships".

Tuesday, May 1

SHEFFIELD METALLURGICAL ASSOCIATION (at 198 West Street, Sheffield 1), at 6.30 p.m.—Dr. N. P. Inglis: "Some Views on Materials of Construction and their Fabrication, in the Light of Present Research and Likely Requirements".

INSTITUTION OF ELECTRICAL ENGINEERS (LONDON STUDENTS' SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 7 p.m.—Discussion on the Report on "Education and Training for Engineers" and a subsequent Report on "Part-time Further Education".

Wednesday, May 2

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Mr. G. W. Osborn: "A Rapid and Simple Method for the Determination of Calcium in presence of Strontium and Barium"; Mr. W. B. Wragge: "'Lead Printing' of Ferrous and Non-Ferrous

Metals"; Mr. E. Collins: "Reaction of Diazotised *p*-Nitraniline with Phenols; Detection of Tricresyl Phosphate in Edible Oil": Dr. A. J. Henry: "A Simple Apparatus for Handling Standard Solutions of Bromine in Potassium Bromide".

INSTITUTION OF ELECTRICAL ENGINEERS (RADIO SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. N. Lea: "Notes on the Stabilities of L.C. Oscillators".

Thursday, May 3

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (PHYSICAL METHODS GROUP) (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 3 p.m.—Dr. H. W. Thompson: "Infra-red Spectrography in relation to Chemical Analysis".

THE BEDSON CLUB (King's College, Newcastle-upon-Tyne, 2), at 5.30 p.m.—Dr. U. R. Evans: "The Prevention of Metallic Corrosion by means of Soluble Inhibitors" (Bedson Lecture).

INSTITUTION OF ELECTRICAL ENGINEERS (INSTALLATIONS SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. R. T. Lythall: "Excess-Current Protection by H.R.C. Fuses on Medium-Voltage Circuits"; Mr. A. G. Shreve and Mr. P. J. Shipton: "Excess-Current Protection by Overcurrent Relays on Medium-Voltage Circuits".

Friday, May 4

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5 p.m.—Sir William Halcrow: "Tidal Power".

INSTITUTION OF ELECTRICAL ENGINEERS (MEASUREMENTS SECTION), (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. G. F. Shotter: "Meter and Instrument Jewels and Pivots".

GEOLOGISTS' ASSOCIATION (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 6 p.m.: H. G. Dines: "Mining Geology of Cornwall and Devon".

Saturday, May 5

AMATEUR ENTOMOLOGISTS' SOCIETY (at Buckingham Gate Central Schools, Wilfred Street, Victoria, London, S.W.1), at 2 p.m.—Exhibition of Entomological Apparatus and Technique.

APPOINTMENTS VACANT

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

BOROUGH ANALYST—The Town Clerk, Town Clerk's Office, Civic Centre, Southampton (endorsed 'Appointment of Borough Analyst') (May 4).

HISTOLOGICAL TECHNICIAN FOR THE DEPARTMENT OF ANATOMY—The Secretary and Registrar, The University, Bristol (May 4).

SOIL CHEMIST—The Secretary, Edinburgh and East of Scotland College of Agriculture, 13 George Square, Edinburgh 8 (May 7).

RESEARCH PHYSICIST for advanced experimental work in connexion with Electron Tube Research and Development, and an Officer for Microwave Crystal Development work on the staff of the Division of Radiophysics, Sydney—The Secretary, Australian Scientific Research Liaison Office, Australia House, Strand, London, W.C.2 (May 11).

LECTURESHIP IN MECHANICAL ENGINEERING—The Principal, Heriot-Watt College, Edinburgh (May 14).

ASSISTANT INSPECTOR (male) OF FISHERIES in the Department of Agriculture—The Secretary, 45 Upper O'Connell Street, Dublin (May 18).

ASSISTANT IN THE BURSAR'S OFFICE—The Bursar, Bedford College, Regent's Park, London, N.W.1.

ASSISTANT PHYSICIST to the Deep X-Ray Therapy and Radium Centre—Dr. H. Canwarden, Medical Superintendent, Warren Road Hospital, Guildford, Surrey.

LECTURER IN BACTERIOLOGY to students preparing for the National Diploma in Dairying—The Principal, Studley Agricultural College, Studley, Warwickshire.

LABORATORY STEWARD FOR PHYSIOLOGICAL LABORATORY—St. Mary's Hospital Medical School, London, W.2.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Historic St. Andrews and its University. By Prof. John Read. Second edition. Pp. 30+8 plates. (St. Andrews: W. C. Henderson and Co., Ltd., 1945.) 1*s.* 6*d.* [213

University College of Wales, Aberystwyth: Welsh Plant Breeding Station. Leaflet Series S, No. 5: Aberystwyth-bred Varieties of Oats and Pure Line Selections of Hen Gymro Wheat. By E. T. Jones. Pp. ii+10. (Aberystwyth: Welsh Plant Breeding Station, 1945.) 1*s.* [213

Science in Peace: an Account of the Conference held at the Caxton Hall, 17th-18th February 1945. Pp. 16. (London: Association of Scientific Workers, 1945.) 3*d.* [223

Scotland, a Wealthy Country. By Dr. Archie Lamont. Pp. 64. (Glasgow: Scottish Secretariat, Ltd., 1945.) 1*s.* [223

Research Reports of the British Non-Ferrous Metals Research Association. Association Series, No. 657: Damping Capacity, a General Survey of Existing Information. By Prof. F. C. Thompson. Pp. ii+38. 3*s.* 6*d.* Association Series, No. 661: Shrinkage and Gas Effects in the Casting of Non-Ferrous Metals and Alloys. By W. A. Baker. Pp. 44. 7*s.* 6*d.* (London: British Non-Ferrous Metals Research Association, 1944.) [223

Imperial Institute. Annual Report, 1944, by the Director, Sir Harry Lindsay, to the Board of Governors. Pp. 67. (London: Imperial Institute, 1945.) [283

Proceedings of the Royal Irish Academy. Vol. 50, Section A, No. 6: Quaternion Centenary Celebration. By R. I. Best, J. L. Synge, G. D. Birkhoff, A. J. McConnell, E. T. Whittaker, A. W. Conway, F. D. Murnaghan and J. R. Colthurst. Pp. 69–122+2 plates. 3*s.* Vol. 50, Section A, No. 7: The Spectrum of Manganese Hydride MnH, 2: The Structure of the $A^{7}\Pi$ and $X\ ^{7}\Sigma$ States. By T. E. Nevin. Pp. 123–138. 1*s.* Vol. 50, Section A, No. 8: Note on the Influence of Damping on the Compton Scattering. By Sheila C. Power. Pp. 139–142. n.p. Vol. 50, Section B, No. 5: The Polinagollum Cave, Co. Clare. By J. C. Coleman and N. J. Dunnington. Pp. 105 132+plates 15–17. 2*s.* 6*d.* Vol. 50, Section B, No. 6: The Glacial Drifts of the District around Enniskerry, Co. Wicklow. By A. Farrington. Pp. 133–158 +plate 18. 1*s.* 6*d.* Vol. 50, Section B, No. 7: The Mechanical Anatomy of the Acromio-Clavicular Joint of Man. By M. A. MacConaill. Pp. 159–166. 1*s.* Vol. 50, Section B, No. 8: The Distribution of Irish Char (*Salvelinus* spp.). By Arthur E. J. Went. Pp. 167–190. 1*s.* (Dublin: Hodges, Figgis and Co. Ltd.; London: Williams and Norgate, Ltd., 1945.) [283

Ministry of Agriculture and Fisheries. Bulletin No. 129: Cereal Diseases. By W. C. Moore. Pp. ii+42+8 plates. (London: H.M. Stationery Office, 1945.). 1*s.* net. [283

Ministry of Fuel and Power. Durham Coalfield: Regional Survey Report (Northern 'B' Region). Pp. 48. (London: H.M. Stationery Office, 1945.) 1*s.* net. [283

Ministry of Fuel and Power. Coal Mining: Report of the Technical Advisory Committee. (Cmd. 6610.) Pp. ix+150. (London: H.M. Stationery Office, 1945.) 1*s.* net. [54

Smokeless Zones. Pp. 12. (London: National Smoke Abatement Society, 1945.) 3*d.* [54

Scientific Proceedings of the Royal Dublin Society. Vol. 24 (N.S.), No. 1: Irish Previously Spawned Salmon. By Arthur E. J. Went. Pp. 8. (Dublin: Hodges, Figgis and Co., Ltd., London: Williams and Norgate, Ltd., 1945.) 1*s.* [54

Other Countries

A Contribution to the Prehistory of Moçambique, by Prof. C. van Riet Lowe; and Report on Material from Kitchen-Middens near the Mouth of the Limpopo River, by L. H. Wells. Pp. 16. (Johannesburg: Prof. C. van Riet Lowe, University of the Witwatersrand, 1944.) [143

U.S. Department of Agriculture. Circular No. 708: The Citrus Thrips; Measures for its Control, and their Effect on other Citrus Pests. By E. A. McGregor. Pp. 12. Miscellaneous Publication No. 550: Family Food Consumption in the United States, Spring 1942. Pp. vi+157. 20 cents. (Washington, D.C.: Government Printing Office, 1944.) [143

The Demonstration of Euclid's Fifth Axiom. By George Pineau. Pp. 6. (San Jose, Calif.: The Author, 552 North 4th Street, 1945.) [193

Indian Forest Leaflet No. 53: Indigenous Charcoal Kilns. By K. L. Budhiraja and A. C. Dey. Pp. ii+13. 6 annas; 7*d.* Indian Forest Leaflet No. 54: Brick-Walled Charcoal Kilns. Pp. ii+10. 6 annas; 7*d.* Indian Forest Leaflet No. 55: Metal Charcoal Kilns. Pp. ii+5. 6 annas; 7*d.* (Dehra Dun: Forest Research Institute, 1943.) [213

U.S. Department of Agriculture. Technical Bulletin No. 880: Mineral Oils, Alone or Combined with Insecticides, for Control of Earworms in Sweet Corn. By G. W. Barber. Pp. 83. (Washington, D.C.: Government Printing Office, 1944.) 15 cents. [213

Causes of the Zebra and other Patterns. By Alan Deakin. Pp. 17. Identical Cattle Twins and Causes of Spotted Patterns. By Alan Deakin. Pp. 16. Causes of Color Patterns in Plants and an Inhibiting Factor Hypothesis. By Alan Deakin. Pp. 24. (Westboro, Ontario: The Author, Ottawa-Prescott Highway, 1944.) 70 cents each. [233

National Research Council of Canada. Wood and Charcoal as Fuel for Vehicles. By R. Ruedy. (N.R.C. No. 1187.) Third edition. Pp. iv+68+8 plates. (Ottawa: National Research Council of Canada, 1944.) 1 dollar. [283

Imperial College of Tropical Agriculture: Low Temperature Research Station. Memoir No. 21: Studies in Tropical Fruits, 15, Hemicellulose Metabolism of the Banana Fruit during Storage and Ripening. By H. R. Barnell. Pp. 297–324. (Trinidad: Imperial College of Tropical Agriculture, 1943.) [54

Transactions of the American Philosophical Society, New Series. Vol. 34, Part 1: The Velocity of Light. By N. Ernest Dorsey. Pp. vi+110. 2.25 dollars. Vol. 34, Part 2: The Arc Spectrum of Iron (Fe 1). Part 1: Analysis of the Spectrum, based on the Work of many investigators and including unpublished Studies by Miguel A. Catalán, by Henry Norris Russell and Charlotte E. Moore; Part 2: The Zeeman Effect, by Dorothy W. Weeks. Pp. ii+111–208. 2.25 dollars. (Philadelphia: American Philosophical Society, 1944.) [54

Twenty-seventh Annual Report of the National Research Council of Canada, 1943–44. (N.R.C. No. 1243.) Pp. 32. (Ottawa: National Research Council of Canada, 1945.) [54

Sargentia: a Continuation of the Contributions from the Arnold Arboretum of Harvard University. 5: Fragmenta Papuana (Observations of a Naturalist in Netherlands New Guinea). By H. J. Lam. Translated from the Dutch by Lily M. Perry. Pp. iv+196. (Jamaica Plain, Mass.: Arnold Arboretum, Harvard University, 1945.) 3 dollars. [54

Scientific Survey of Porto Rico and the Virgin Islands. Vol. 12, Part 4: Insects of Porto Rico and the Virgin Islands; Lepidoptera (suborder) Rhopalocera, (superfamily) Papilionoidea (True Butterflies), (superfamily) Hesperioidea (Skippers). By Williams Phillips Comstock. Pp. ii+421–622. (New York: New York Academy of Sciences, 1945.) [54

Catalogues

Redux Metal Bonding. Pp. 28. (Duxford: Aero Research, Ltd., 1945.)

History of Science. (Catalogue 6.) Pp. 40. (London: E. Weil, 1945.)