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LOCAL MUSEUMS AND EDUCATION

SUGGESTIONS FOR FUTURE POLICY

IT is not yet generally recognized that local museums can, or should be able to, make a substantial contribution towards the education of the adult and juvenile population of their respective areas. Even the Ministry of Education, in its various 'recommendations', appears to have overlooked these veritable treasure houses of visual aids. What are the reasons for this ? Mr. S. F. Markham's Report, "The Museums and Art Galleries of the British Isles" (1938), probably supplies the answer, namely, that the majority of local museums, for some reason or another, are failing to carry out their functions adequately (see *Nature*, **143**, 447 ; 1939).

The Report clearly shows that the main causes for such a condition are : (1) the absence of sufficient expenditure, (2) the constitution of the governing body, and (3) "the great number of untrained and part-time curators". All these are closely related. Indeed, condition (2) is directly responsible for conditions (1) and (3), for where the governing body is composed of only half-interested, or frankly uninterested individuals (and this is not a rarity), there is not likely to be much expenditure either on museum improvements or on trained curatorial service. In the present pressing need of education in all its forms, this is an expensive economy.

Again, there are still too many local museums under the 'administration' of a library authority—an unsuitable situation in view of the unrelated work of library and museum. The 'library and museum' committee is not infrequently composed of individuals unacquainted with the work and aims of museums. The result is that the affairs of the museum tend to become subservient to those of the library. In this connexion it is not sufficiently realized that the qualifications of a museum committee member require more than a mere interest in 'old things'.

Such, then, are some of the conditions prevailing in many of the local museums of Britain. These form obstructions to improvement and progress, and, as such, it is high time they were removed. The removal of 'obstructions', however, often requires the impetus of public opinion ; and in Great Britain, unfortunately, there is very little public opinion in relation to the local museums.

The United States demonstrates to the whole world the very real value of the museum. In that country, museums were "deliberately created as part of the educational system" ("Libraries and Museums", by Sir Frederic Kenyon, 1930, Benn's Sixpenny Library, No. 100). Consequently, American museums have won and maintained strong public interest, and they are, therefore, liberally supported. In view of the attainment in the United States, it seems absurd that the museum-movement (as a whole) in Britain—where so much fine educational material is preserved—should remain 'sleeping' beneath the

impediments mentioned. There are museums scattered all over the British Isles—good, bad, and indifferent—but a great many of the local institutions, at the present time, have to be written off as 'bad' or 'indifferent'. Unless these are reorganized, they can serve no useful purpose in the post-war world. It becomes increasingly apparent, however, that some sort of Government intervention will be necessary before reorganization on the required scale will take place—a poor reflexion, perhaps, on local initiative.

Before passing on to other considerations, it may be appropriate here to mention two museums under the direction of the Education Department of the London County Council which prove their very real educational value. These are the Horniman Museum at Forest Hill, and the Geffrye Museum in Kingsland Road, E.2. The work of both these are well worth the attention of other educational authorities and every public-spirited person.

Those with but a hazy notion of museum values might here ask: What can (or should) a local museum offer towards the education of the people of its area? At least the following: visual instruction; facilities for study; popular lectures; æsthetic recreation, and a service especially designed for schools in the district. The latter should include the distribution of museum loans and arrangements for school talks. The permanent exhibits should be reinforced by a frequently changing series dealing with various subjects, and from time to time there should be special exhibitions to illustrate local talents (art, etc.) and local industries. Only when these amenities are widely offered will public interest be fostered and the local museum serve a useful purpose.

Since a service for schools is an important side-line—and one that promises to increase in scope—we would direct attention to some of the school loans schemes already in operation in Britain. The success of these, even where they are still limited, shows that the museums are strongly placed with regard to the visual aids of which nowadays we read and hear so much.

In this work the Victoria and Albert Museum in London has set the example. In 1937 (see Markham's Report) this Museum had a collection of loans comprising 41,015 works of art, 15,427 lantern slides, and 300 books, and these were issued to 413 secondary schools, 224 art schools, 94 local museums and 96 other institutions. This lead has been followed (naturally on a smaller scale) by the Leicester City Museum (the modern pioneer of such work), and the Derbyshire Education Committee. Both are extensive and county-wide in application, and their 'aids to education' afford a wide selection of portable exhibits dealing with most school subjects. In addition, school talks are arranged, and lately, Leicester has also organized a children's club within the Museum.

The Warwick Museum (Warwickshire Education Committee) launched a scheme—based on those of Derby and Leicester—in 1940. There, it is still in its early stages, and war-time conditions have made the building-up of the collection difficult. Nevertheless, this year several hundreds of items, including cases of museum specimens, models, sets of lantern slides and other illustrative material have been distributed on loan to many schools in the county.

There is an important condition in museum work in relation to schools, and that is the necessity of teachers having enough opportunity of making direct contact with the local museum curator, or schools' loans organizer. The teacher should be in the best position to know what, out of the museum's reserves, can be of most use for his or her purpose. Again, any available museum service should be widely advertised in the schools. Experience has indicated that far too few teachers are acquainted with the services which many museums can, or should, render.

It is clear that a considerable amount of educational work lies within the scope of the average local museum, if that institution be given a fair chance. All over the world to-day, the wholesale destruction of cultural material goes on. Some of our own treasures have suffered. Is it not time, therefore, that the mass of material still remaining to us be put to its fullest possible use? If, in the words of Sir Frederic Kenyon, ". . . museums have an important part to play in the formation of the citizen . . .", it is vital that we do so.

PHYSICAL ADSORPTION

The Adsorption of Gases and Vapours

By Stephen Brunauer. Vol. 1: Physical Adsorption. Pp. vii+511. (London: Oxford University Press, 1944.) 35*s*. net.

DR. BRUNAUER has given in this, the first part of a work designed to cover all the phenomena of adsorption of gases on solids, by far the most thorough and comprehensive account that has yet appeared. It deals mainly with what is often called 'physical' adsorption, that is, adsorption in which the bulk of the adsorbate is held to the surface by van der Waals' forces, and the gas is below its critical temperature; but sufficient reference is made to 'chemisorption', where co-valent forces come into play, to put the whole subject of adsorption into proper perspective.

The relations between pressure, temperature, and the quantity adsorbed are treated with very great thoroughness; so is the heat of adsorption, and all the various theories which have been advanced to explain the shape of the isotherms. The account of the nature of the binding forces is also very full; indeed, the general account of the theories of the nature of van der Waals' attractive forces is as good as that in any text-book. Experimental methods are adequately treated, in all essentials, though rather less completely than is done for theories. The structure of the adsorbent, and also of the adsorbed film, receive as full attention as is possible in the present state of knowledge.

Perhaps the most important section of the book is that which expounds, in full detail, the 'multimole-

cular' adsorption theory of the author, Emmett and Teller. This theory takes account of the fraction of surface covered and the heat of adsorption, in each of a large number of layers on the surface; in the layers beyond the first, the heat of adsorption is taken, as a first approximation, as equal to the heat of condensation to bulk liquid. Most of the numerous forms which the adsorption isotherms can take are explained, quantitatively, by this theory, with allowance for capillary condensation in the pores: it marks a very great advance on any preceding theories, and covers the great bulk of all possible cases of adsorption.

In the discussion of the structure of porous adsorbents, some of the theories quoted, notably those dealing with the apparent density of adsorbents such as charcoal, and the hysteresis sometimes found in adsorption, might perhaps be considerably clarified if more attention were paid to the contact angle between the substances adsorbed, when liquid, and the solid adsorbent. It would seem much more likely that the contact angle governs the extent of penetration of liquid into minute pores than the viscosity; indeed, I think that possibly this angle is a dominating factor in the values found for the apparent density, measured by the displacement of liquids. Perhaps also the well-known hysteresis of the contact angle, coming into play as an 'advancing' angle when capillary spaces are being filled with adsorbate, and a 'receding' angle, always smaller than the advancing angle, during desorption and emptying of capillaries, is largely responsible for adsorption hysteresis. The rather far-fetched theory which claims that pores in some adsorbents resemble 'ink-bottles' in shape might prove superfluous if attention were paid to this hysteresis of contact angles.

The treatment of the state of the adsorbed film is cautious; the author does not commit himself for, or against, recent theories which regard the adsorbed layer as possessing most of the properties of a two-dimensional gas even to the extent that the outward surface pressure of these gases can cause expansion of the adsorbent. Neither are the discontinuities in the adsorption isotherms, now based on experimental evidence from a number of sources, treated, except as a curious phenomenon not at present to be integrated with the rest of the theory. This caution would, however, seem well justified in the present state of knowledge.

The author is to be congratulated on a very fine account of a complex subject, more complex perhaps than most non-specialists will have suspected. He has given a very learned and well-balanced picture of practically all the important theories that have been proposed, and there are many of them. It is impartial—perhaps for some tastes a trifle too impartial, for the book has, in places, to be very carefully read to ascertain what the author's own views are; but the reader is always given very full material on which to base his own judgment.

The preface warns that we shall have to wait some time for the second volume, which is to deal with chemical adsorption. This is tantalizing, for one who can give such a stimulating account of what many people would expect to be the less interesting half of the subject should have something enthralling to say about the other half. He does, however, let fall one provocative remark, to whet our appetite for what is still to come, saying on p. 10, "there is some opposition even to-day to accepting the concept of activated adsorption". I myself, being still somewhat sceptical that slow adsorption can legitimately be ascribed to an energy of activation in the chemisorption of gases in the first layer, look forward impatiently to the author's promised marshalling of the evidence in its favour. It is a very important point of theory, and no one can be better fitted than the author for bringing all the evidence into proper focus; his knowledge is unsurpassed, and his fairness as an expositor seldom equalled. N. K. ADAM.

AVIATION MEDICINE

A Bibliography of Aviation Medicine and Supplement

By Phebe Margaret Hoff, Ebbe Curtis Hoff and John Farquhar Fulton. (Publication No. 9, Historical Library, Yale Medical Library.) Pp. xiv+109. (Washington, D.C.: National Research Council, 1944.) n.p.

AVIATION medicine has become a subject of prime practical importance in the present War. It is a subject in which the basic data from many fields of physiology are required in new applications. The compilation of a bibliography of aviation medicine is therefore a work of great value to those interested in this field. A large volume of work done in Service laboratories is not included, as this has not yet been published in open form; this is often less due to secrecy provisions than to the extreme preoccupation of the research workers with urgent operational problems. For, while many of the applications of physiology to aviation carried out in Service laboratories are at present restricted by secrecy considerations, the scientific basis of most of these rests on fundamental physiology often published many years before the War; this bibliography covers the field in all its ramifications. Only a few applied sections are seriously incomplete and must remain so until after the War.

This Bibliography, first published in 1942, is now followed by a supplement, mostly of work published since 1940.

The classification by subject-matter is convenient, and all references are given numbers used for reference in cross-indexing at the foot of each subsection. An author index is included.

The section "Special Physiology of Aviation and Conditions Simulating Flight" occupies nearly half the volume and is divided into twenty-three physiological divisions further subdivided.

The Bibliography is comprehensive and includes historical works and accounts of early balloon and mountain ascents where these contained any pertinent matter.

Research on the effects of altitude may be divided historically into two parts, from Robert Boyle to Paul Bert, and Paul Bert to the present. Robert Boyle subjected animals to low pressure and recorded seeing a bubble in the eye of a viper when exposed to low pressure, and so might be considered the discoverer of decompression sickness. Modern research dates from Bert, who laid the main foundations of modern knowledge in his great work "La Pression Barométrique", published in 1878.

The book is excellently produced, and the arrangement could well serve as a model of bibliography.

B. H. C. MATTHEWS.

PLANT VIRUSES AND VIRUS DISEASES*

By F. C. BAWDEN
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THE existence of viruses was first deduced from work done in 1892 on tobacco plants suffering from mosaic, and much of what we now know of these elusive entities has come from further work on this and a few other plant diseases. It is far from certain that this knowledge can safely be applied to the causes of the many diseases, affecting all kinds of animals, higher plants and bacteria, that are now attributed to viruses. These cover a wide range of clinical conditions, and we know for certain of only two features that they have in common ; their causes have neither been seen nor cultivated *in vitro*. If we wish, we can turn these negative features into what looks like a positive statement, by defining viruses as obligately parasitic pathogens too small to be resolved by microscopes using visible light. Indefinite as this is, it may still prove to be more precise than the facts warrant, for obligate parasitism is always postulated rather than proved, and serious attempts at cultivation have actually been made with very few viruses. Thus, when we speak of a virus disease, we usually mean merely an infectious disease with an invisible cause. Unless the resolving power of the microscope has some unsuspected significance in defining biological types, this obviously tells us nothing specific about the nature of viruses and might well cover a range of different entities.

This possibility seems increasingly likely when we try to generalize about plant virus diseases, for we find that no statements can be made about such features as symptoms, methods of infection, or distribution of virus in the host, to which there are no exceptions. This is far from conclusive, however, for what a virus does to a plant is as much a property of the plant as of the virus, and the same virus may produce very different effects in different plants. Also, although complete generalizations are impossible, there are some features shared by a number of different virus diseases, especially those met commonly in Nature.

The effects most frequently caused by viruses are a dwarfing of the host plants and an alteration of the colour and shape of the leaves. Instead of being uniformly dark green, the leaves may bear spots, rings or patches of light green, yellow or white, or they may be generally chlorotic without definite mottling. Deformation may show only as an alteration in the leaf outline, or the laminæ may be so reduced that the leaves consist of little but the main veins ; it may take the form of local hyperplasia, to give unusual outgrowths from the leaves or gall-like proliferations in stems. Symptoms tend to occur more generally over a whole plant than with most fungal or bacterial diseases, for in natural infections it is usual for viruses to spread through the vegetative parts of affected plants. In plants infected experimentally, however, symptoms are often restricted to local lesions, produced by the death of tissues around the entry point. Diagnosis from symptoms is by no means easy, for different viruses may cause almost identical symptoms in the same host, whereas the same virus may produce totally different clinical conditions in different hosts. A further complication is that many viruses are unstable and frequently change to give forms that produce different symptoms from those produced by the parent virus. To be recognized, a virus must cause changes in the appearance of some plants ; but it need not necessarily cause changes in all susceptible hosts. Indeed, the phenomenon of the carrier—an infected individual showing no symptoms—is common in plants, and such carriers can be of considerable importance as unsuspected sources of infection for intolerant species.

In many virus diseases, three distinct phases can be identified. As a result of virus multiplication at sites of infection, lesions first appear on inoculated leaves. After a few days, the virus passes to the phloem, through which it travels rapidly to distant parts of the plant. It seems to have no autonomous movement, but to travel along with the translocation stream of elaborated food materials, away from tissues actively engaged in photosynthesis and towards regions of active growth. It is because of this, and not because they resist infection, that leaves already fully developed at the time the plant becomes infected rarely show symptoms. Thus results of systemic infection appear on the young, actively growing leaves ; the later symptoms of this systemic phase often differ from those first produced, as the disease passes from an acute to a chronic stage. Often both stages are serious diseases ; in many potato varieties, for example, leaf-drop-streak is succeeded by severe mosaic. Occasionally, however, the chronic stage is extremely mild, such as in tobacco plants with ring-spot, which recover from an acute necrotic disease and afterwards show few or no symptoms. The virus is present in such plants, but in smaller quantities than during the acute stage. The sequence of three phases is common, but by no means general, and the same virus may give different sequences in different hosts. Potato virus Y, for example, gives local lesions only in one host, local lesions followed by systemic symptoms of two kinds in a second host, whereas in a third it gives no local lesions and systemic symptoms of only one kind.

In addition to altering the external appearance of plants, viruses also produce internal changes. Some of these are simply modifications of normal structures or tissues, such as reduction of the chloroplasts or necrosis of the phloem ; but the most characteristic involve the production of new kinds of intracellular inclusion bodies. These are not found in all virus diseases, but their formation appears to be specific to viruses, for similar bodies have not been found either in healthy plants or in those suffering from other kinds of disease. Different viruses give rise to different kinds of inclusion body, and produce them in varying numbers and in different tissues. The most general type is a vacuolar, amœboid-like body found in the cytoplasm, but crystalline and fibrous inclusions also occur in infections with a number of different viruses. At least two viruses give rise to crystalline inclusions in the nuclei. The precise nature of these bodies is still uncertain ; but we know that they contain virus, and their production can in part be simulated *in vitro*. It seems most likely that they are insoluble complexes produced by the viruses combining with some metabolic product of the diseased plants.

Symptomatology without proof of transmissibility is insufficient to assign a particular disease to the virus group, for similar kinds of symptoms can be caused by toxins, deficiencies of mineral nutrients

* Substance of two lectures at the Royal Institution delivered on November 21 and 28.

and aberrant genes. With hosts that are easily grafted, transmission by grafting is usually the first method tried; for once organic union is established, all viruses that cause systemic symptoms readily pass from infected scions into healthy stocks. Indeed, grafting is the only method of transmission known for many virus diseases, and it has almost become the critical test of a plant virus disease.

Infection occurs only through wounds, but wounds that permit one virus to enter may not permit another. Many viruses are readily transmitted by rubbing healthy leaves with sap from diseased plants, but others are not; some of both these types are transmitted by insects. Several different explanations can be offered for the failure of inoculation to transmit viruses that are readily transmitted by insects. First, some viruses may be able to establish themselves only in deep-seated tissues, such as the phloem, which are not penetrated by ordinary inoculation methods. Secondly, conditions in the expressed sap of some hosts may be such that the viruses are rapidly destroyed or rendered non-infective. Thirdly, the virus content of sap from some diseased plants may be below that required for infection. Thus, although failure to transmit by inoculation is often used as a specific character of a virus, clearly it may equally well be a reflexion of some property of the host.

Insects do not seem to act simply as mechanical carriers of viruses, for no insect vectors are known for the two viruses most easily transmitted by inoculation; and there appear to be specific relationships between insects and the viruses they transmit. Individual viruses are usually transmitted by only a few related species of insect and not by others, though these may have similar feeding habits and be vectors of other viruses. Vectors are usually insects with sucking mouth-parts; the most important are aphides, leaf-hoppers, white-fly and thrips. Two main types of behaviour in the insect have been distinguished. Vectors of one type of virus can infect healthy plants immediately after feeding for a short time on a diseased plant, and these usually cease to be infective within a few hours. After feeding on diseased plants, vectors of the other type cannot infect healthy plants for some time, which varies from minutes to days with different viruses, and such vectors remain infective for long periods, often for their whole lives. Some workers believe that viruses of the second type multiply in the insects. There is no obvious reason why they should not, and the theory would explain some of the now puzzling features of the behaviour of these viruses; but there is no conclusive evidence that insects ever contain more virus than they acquire while feeding on infected plants. Studies on the virus causing dwarf disease of rice supplies the best circumstantial evidence for multiplication. This virus is unique in being the only one known to pass from infective adults through eggs to their progeny. Progeny up to the seventh generation have once been found to be infective and from one infected egg the progeny have infected more than 1,000 plants. This is regarded by some workers as 'overwhelming' evidence for multiplication, as they consider that the quantity of virus in the original eggs could not have been enough to give all the infections. But is this so? If the virus multiplied in the insects to anything like the extent it does in plants, then there would be no reason why the progeny should not continue to be infective indefinitely, and infect as many plants as they feed on. We know nothing of the size of this virus, but if it is of the same order as other plant viruses the sizes of which are approximately known, then 1,000 particles would weigh less than 10^{-14} gm., and many times this quantity could surely be contained in a leaf-hopper's egg without difficulty.

Transmission of some viruses has been achieved by linking diseased and healthy plants with the parasite dodder (*Cuscuta* sp.). This novel method of transmission promises to be valuable in extending the host ranges of some viruses to plants more favourable for study than those in which the viruses occur naturally. One of the greatest differences between individual viruses lies in the numbers of different plants they can attack. Some are known to infect hundreds of plant species, belonging to many different families and orders; others have been transmitted to only a few closely related species. This difference may be apparent rather than real, for viruses transmissible only by grafting or by insects will normally have host ranges restricted to plants which can be intergrafted or which can act as food plants for the insect vector.

For more than forty years, work on plant viruses was largely concerned with symptoms, transmission and host ranges. It showed that viruses could multiply and alter, and produced few results conflicting with the generally accepted conclusion that they were small organisms, essentially similar to bacteria. There were opposers of this, usually from among those studying tobacco mosaic virus, but they could offer nothing definite to support their alternative views. The intensive study during the last ten years of the viruses *in vitro* has led to results that necessitate considerable modification of the earlier views. They do not, however, justify the sweeping conclusions implied by such facile phrases as 'lifeless molecules', which are increasingly applied to viruses.

What has been achieved is the successful application of the techniques of protein chemistry to the purification of a dozen or so viruses. This has shown us that the particles of these viruses are not organized cellularly like organisms, and that in many ways they resemble constituent parts of organisms rather than whole organisms. They can be obtained in forms chemically much simpler than bacteria, free from diffusible components, and with a much greater regularity of internal structure than is usual with organisms. The viruses so far purified have all been obtained in the same chemical form, as nucleoproteins. They all contain nucleic acid of the ribose type, but the proportion of nucleic acid to protein varies with the individual viruses. It is far too early to conclude that all plant viruses are essentially nucleoproteins; but we can say that it will be a major discovery if one is found to be anything else, for those already purified cover a diversity of types, some known to be insect-transmitted and others not. They range from potato virus Y, which denatures and loses infectivity within a few days, to tobacco mosaic virus, which remains stable for years. Stability as a native protein, however, is not always the same thing as stability as a virus; the infectivity of preparations of any of these viruses can be destroyed by some treatments that have no appreciable effects on the physical, chemical and serological properties.

The shape of the particle is responsible for some of the most striking differences between the properties of preparations of different viruses. Solutions of purified tobacco mosaic virus, and of potato viruses X and Y, show phenomena characteristic of greatly

elongated particles; they are anomalous in all their physical properties and are polydisperse. No true crystals have been prepared from these, but dilute solutions show anisotropy of flow strongly, and concentrated solutions are liquid crystalline. X-ray studies of solutions of tobacco mosaic virus have demonstrated a regularity of structure previously unsuspected in fluids, for the particles are arranged equidistant from one another so that the available space is filled uniformly. When mixed with their antisera, these rod-shaped virus particles precipitate almost immediately, giving bulky, fluffy precipitates resembling those produced by bacterial flagellar antigens.

Solutions of bushy stunt and tobacco necrosis viruses behave very differently and show none of the anomalous properties characteristic of elongated particles. By suitable treatments they can be induced to crystallize in forms characteristic of the individual virus. When mixed with their antisera, they precipitate more slowly than the rod-shaped viruses and, as might be expected with spherical particles, pack more closely to give dense, granular precipitates resembling those produced by somatic antigens.

What is the relationship between these isolated nucleoproteins, which in laboratory work behave much like preparations of other proteins, and the viruses as they occur in the plant? There is enough evidence now to show that these proteins are the viruses in the sense that they can initiate infection. Nevertheless, it would be premature to assume that, while active in the host plant, the viruses are chemically so simple as analysis of the purified preparations suggests. During the course of isolation, many materials are discarded as impurities; most of these are certainly constituents of the normal host, but some may well be specific products of virus activity. Any such are clearly not essential for infectivity; but if the virus were organized cellularly, they would be retained within a cell wall and would be accepted as integral parts of the virus, which would immediately look a much more complex body than does our naked protein particle.

In the absence of specific tests for any product of virus activity, we have no positive evidence for their occurrence in plants, but evidence from various sources suggests that purification may be altering the viruses. Purified preparations of tobacco mosaic virus, for example, contain particles about 15 mμ wide but varying in length from less than 100 mμ to more than 1,000 mμ. There is nothing to show that the greatly elongated particles occur in the plant, and much to suggest that they are produced by the linear aggregation of small particles during the course of preparation. By taking suitable precautions, solutions of tobacco mosaic virus can be made that show little or no anisotropy of flow and behave serologically more like somatic antigens; but these are unstable and readily change into anisotropic solutions with serological behaviour characteristic of flagellar antigens. This change seems to be connected with the removal of other material from the small nucleoprotein particles, which then join together end-to-end. The change in size and shape may explain the failure to produce true crystals of this virus *in vitro*, though they occur abundantly in infected plants.

We know also that the purified virus readily combines with other proteins such as trypsin and ribonuclease, and that these can be removed again without affecting infectivity. May not similar combinations occur within the host, and be responsible for converting this nucleoprotein into a functioning system capable of multiplication and of the activities of which the results are so obvious?

In addition to the changes produced by purification, there is other evidence that virus does occur in the plant in forms with different properties from those of the purified nucleoproteins. Until recently, all laboratory work on plant viruses was done with the sap that is expressed from macerated infected leaves. This was thought to contain all the virus in the plant, for washing the fibrous residues gives little extra virus. However, these residues actually contain as much virus as does the sap; but normally this is insoluble, probably because it is combined with other substances, and special treatments are needed to get it into solution. It is possible that this insoluble virus is the biologically active system, whereas that free in the sap may be merely excess virus functioning as a mobile source of infection for other cells. We know so little about the multiplication of viruses, and of their activities within the host, that at present we must suspend judgment. But it is probably safest to regard the nucleoproteins as the chemical minima—equivalent to reproductive organs or embryonic viruses—which develop into working entities only when placed in an environment containing the materials or enzyme systems they lack in their purified state.

CHEMISTRY AT THE OLDER UNIVERSITIES OF BRITAIN DURING THE EIGHTEENTH CENTURY

By ARCHIBALD CLOW
University of Aberdeen

IN 1814 Sir John Sinclair, president of the Board of Agriculture, wrote:

"At present there are a greater number of intelligent practical chemists in Scotland, in proportion to the population, than perhaps in any other country in the world" (J. Sinclair, "General Report", App. 2, p. 307).

In the light of this rather startling assertion, it is instructive to analyse the development of chemistry in the universities of Scotland during the preceding hundred years, and to compare it with developments farther south. In Great Britain there are five universities to consider: in England, Oxford and Cambridge; in Scotland, Edinburgh, Glasgow and Aberdeen. There was no profession of chemistry at St. Andrews until at a later date.

While alchemy yet held the field, the universities of Scotland remained aloof from the flux of gold and elixir making, but at the end of the seventeenth century the Surgeons' Incorporation in Edinburgh established a laboratory where apprentice apothecaries received a chemical training. The instigator of this pioneer development was Alexander Monteith. He was thus a contemporary of Sir Isaac Newton (1642–1727), who in Cambridge was still studying Boyle's method of gold-making during 1690–93, perhaps not without hope of practical application, since within a few years he was appointed Warden of the Mint.

In the early years of the eighteenth century, the Town Council of Edinburgh decided to appoint a

professor of chemistry in the Town's College, and in 1713 James Crawford was selected to fill the chair. Crawford did not achieve European reputation, but he was a product of the great Boerhaave school at Leyden, and his appointment to Edinburgh is significant. It inseminated Scotland with the finest seed of Continental chemistry, and it gave professorial status to a teacher of chemistry in advance of most other countries. The only other university with a like claim is Oxford. On cursory examination it appears that, as a centre of chemical activity, Oxford has indeed a better claim. This arises in the main from its association with the alchemistic Roger Bacon, but even R. T. Gunther points out that "it is a moot point whether Roger Bacon really made much impression on his contemporaries; if any, it was evanescent; and in the succeeding centuries Oxford savants continued to wander in a maze of arbitrary figments and partial inductions, in which experimental science found no place". (R. T. Gunther, "Early Science in Oxford", 7.)

In the middle of the seventeenth century, we find that for a time Oxford did indeed give hospitality to an evacuee, Robert Boyle (1627–91), one of the greatest of contemporary thinkers. For fourteen years from 1654 Boyle was at Oxford. While there, he became the centre of a small coterie of intellectuals who doubtless helped to bring about what J. U. Nef calls the first English industrial revolution. Boyle's influence was rather that of a patron experimentalist than a teacher; but he was responsible for introducing to Oxford its first regular teacher of practical chemistry. It was a long time, however, before the teaching of chemistry became continuous. In compensation for the paucity of chemical instruction, an important contribution to technics made by an Oxford B.C.L. may be mentioned.

"The mystery of salt-glazed stone ware was discovered by the ingenious John Dwight of Christ Church, who set up a manufacture at Fulham. . . . When and where John Dwight became acquainted with this use of salt is not known but in 1671 he took out a patent for his process, and in the same year the first specimens of salt-glazed ware were being manufactured at Fulham. Soon after 1688 similar ware was being produced at Burslem by the Dutchman Elers, and in 1700 in Nottingham". (R. T. Gunther, "Early Science in Oxford", 27.)

Oxford suffered from its proximity to London, and the removal of Boyle (as well as of other intellectuals which followed the more settled conditions of state established in 1660) did irreparable damage to its scientific life. The only man of science worthy of the name who remained was John Mayow, whose "De sal-nitro et spiritu nitro-aereo" heralded the later discovery of oxygen. But Mayow too left Oxford in 1675, and died in 1678.

"Thus closed the brief life of the greatest chemist whom Oxford has ever produced. His works, a century in advance of the times, were unappreciated during his life and were soon neglected, buried and forgotten under a thick pall woven in Germany by Stahl, out of a warp of genuine facts and a weft of false hypotheses". (R. T. Gunther, "Early Science in Oxford, 32.)

By one of the unfortunate accidents to which collegiate monasticisms are prone, Mayow was not an associate of Boyle; indeed they seem to have been mutually unaware of each other's work, and Mayow's contributions remained hidden for many years. Yet his manipulative skill substantiates his claim to be considered one of the founders of pneumatic chemistry. Thus while Oxford may claim an earlier contribution of chemistry applied to manufactures than can Edinburgh, its periods of seventeenth century brilliance were sporadic and contrast markedly with the continuity of chemical teaching undertaken at Edinburgh.

So far as continuity is concerned, albeit it was mediocre, Cambridge fared better than Oxford. In the early years of the eighteenth century, that is, contemporary with Crawford's professorship at Edinburgh, the title of honorary professor of chemistry at Cambridge was conferred on one J. F. Vagani (*c.* 1650–1713), a native of Verona. Of Vagani we know little, but he was probably the first chemist there to throw off the alchemical tradition. From records of purchases made to illustrate his lectures, it is highly probable that they were biased towards pharmaceutical ends. It is interesting to note that one of Vagani's students was Stephen Hales (1677–1761), whose researches on the chemical reactions of plants laid the foundation on which Francis Home of Edinburgh was able to build his "Principles of Vegetation".

Vagani was followed by John Waller, who lectured until 1718, and Waller in turn by John Mickleburgh, who brings us up to 1741.

By this time great changes had taken place in Scotland. In 1724, four fellows of the Royal College of Physicians announced that they had purchased a house in Edinburgh for a chemical laboratory, and indicated that they proposed to lecture extra-murally on chemistry and *materia medica*. Every one of them had studied at Leyden under the celebrated Boerhaave, and thus went to Edinburgh with the finest training that could be obtained at the time. They were Drs. John Rutherford (1695–1779), Andrew Plummer (*d.* 1756), John Innes (*d.* 1733) and Andrew St. Clair. After extra-mural teaching for a few years, they insinuated themselves into the University, which for a time boasted four "professors of chemistry". None of the quartet made revolutionary contributions to the advance of chemical theory or practice—Rutherford's son discovered nitrogen, it is true—but their significance in the history of technology and science lies not in their own contribution but in the pioneer foundations established by their students. Plummer particularly was the mentor of several founders of chemical industry, as well as of two of the greatest academic chemists Scotland, or for that matter any country, has produced, namely, William Cullen (1710–90) and Joseph Black (1728–99).

Of Plummer's industrialist pupils, John Roebuck (1718–94) is the most important. The manufacture of sulphuric acid was first carried out in England on what may be called an industrial scale when Dr. John Roebuck, in company with Samuel Garbett (1717–1805), established his works at Steelhouse Lane, Birmingham, in 1746. They set up a second works at Prestonpans in 1749, and with the profits gained in these very successful enterprises Roebuck went on to found Carron Iron Works in 1760, thus opening up for the first time the carboniferous deposits of central Scotland.

The establishment of sulphuric acid manufacture on an industrial scale in both England and Scotland by Roebuck and Garbett—and it must be remembered that Roebuck's interest in chemistry was derived from Plummer's teaching at Edinburgh—is of signal importance, since it almost immediately brought

about a revolution in the art of bleaching by the application of the researches of another Edinburgh professor, Francis Home (1719–1813), professor of *materia medica* in the University from 1768. The introduction of sulphuric acid at this early stage in the industrialization of bleaching was indeed a milestone in the long chain of contributions made by Scottish chemists to industrial development.

About the same time as Roebuck settled at Prestonpans, another student of Plummer began to engage in chemical manufacture. He was no other than the celebrated Scottish geologist, James Hutton (1726–97). Beckmann in his "History of Inventions and Discoveries" states:

"If I am not mistaken, the first real manufactories of sal ammoniac were established in Scotland; and the oldest of these, perhaps, was that erected by Dovin and Hutton at Edinburgh in 1756* and which, like many in England, manufactures this salt on a large scale" (4, 383).

Soot was the raw material used, and they continued to use it for many years until they began to buy crude sal ammoniac from a tar works that had been established in the interim by Lord Dundonald at Culross.

All this has to be set against "the mystery of salt-glazed stone ware", contributed by Oxford.

From Edinburgh the teaching of chemistry spread to the University of Glasgow. William Cullen (1710–90), having studied arts at Glasgow and medicine under Plummer at Edinburgh, was appointed a teacher of medicine at Glasgow in 1746. Stimulated by Plummer's teaching, he developed a dominant interest in chemistry, and in 1747 induced the University to establish the teaching of *Chemie*. In the same year, Cullen himself, and a John Carrick, were appointed lecturers in chemistry. Carrick however died in 1750, and Cullen was left to continue the course on his own.

Cullen's outlook on chemistry was severely practical, as has been that of all great Scottish chemists. At the beginning of his second course he printed and distributed "The Plan of a Course of Chemical Lectures and Experiments directed chiefly to the Improvement of Arts and Manufactures". He clearly recognized the importance of scientific chemistry and its application to industrial and agricultural development. While in Glasgow he devoted a considerable part of his time to industrial problems of the time, particularly to those subjects of which industry demanded a chemical investigation, for example, salt-boiling, bleaching, and alkali supply.

"He was a great master of the scientific branches of husbandry; a consummate botanist, and possessed a correct taste in the fine arts. In the year 1758, after finishing off chemistry, he delivered to a number of particular friends, and favourite pupils, more lectures on the subject of agriculture. In these few lectures, he, for the first time, laid open the true principle concerning the nature of soils, and the operations of manures." (A. Bower, "History of the University of Edinburgh", 2, 392.)

In 1751, Cullen was appointed professor of medicine and lecturer in chemistry at Glasgow, which posts he held until 1755, when he was appointed colleague and successor to Plummer, and moved to Edinburgh. In Glasgow he was succeeded by Joseph Black (1728–99), who followed him both at Glasgow (1756) and later at Edinburgh (1766).

* The correct date is a good deal earlier than that given by Beckmann.

If we compare the number of students reading chemistry at Edinburgh and at Cambridge at this period, it is likely that Edinburgh will be found to have the smaller number, but an expansion took place almost immediately. In the light of these figures we can sympathize with Davies, who wrote to Stephen Hales in 1759 lamenting that at Cambridge,

"Anatomy, botany, chemistry, and pharmacy have been but occasionally taught; when some person of superior talents has stayed up and has honoured the University by his first display of them, before his passage into the world". (R. Davies to S. Hales, 1759.)

In Scotland at this date Cullen was teaching in Edinburgh and Black in Glasgow.

In 1766 Black went to Edinburgh to succeed Cullen, who had been translated to another chair, and for thirty years he occupied the chair of chemistry during one of the great formative periods through which chemistry has gone, both in expansion on the theoretical side and in its application to industry. So great were Black's contributions to fundamental chemistry that one is apt to forget that he also kept in close touch with contemporary industrial developments, especially in a consultative capacity, and through personal contact with friends like Roebuck and Hutton. Of particular interest are his attempts, in collaboration with Roebuck and James Watt, to synthesize alkali; his connexion with the initial stages of Lord Dundonald's Tar Works. Problems concerning Cort's process for the production of malleable iron were referred to him. He advised on pottery problems. Specimens of ore and water from the lead mines at Wanlockhead and Leadhills were sent to him for analysis. His opinion was sought by the committee investigating Scottish distillery. He devised methods for the chemical assay of kelp.

What of chemistry in the 'older' universities? Mickleburgh, who had been appointed to Cambridge shortly before Cullen, went to Glasgow, gave way to John Hadley (1731–64), and he in turn to Richard Watson (1737–1816), afterwards Bishop of Llandaff, who was appointed professor of chemistry at Cambridge in 1764. At the time of his appointment it was said of Watson that "he knew nothing at all of chemistry, had never read a syllable on the subject, nor seen a single experiment in it". In two years the illustrious Black was to succeed Cullen at Edinburgh. Small wonder that the evolution of chemical science in the two countries was so different. Yet Watson was no idle churchman. He took his new appointment very seriously, and has related how at one period his conscience forced him to burn his chemical writings lest he be lost to the church altogether. Among other activities, he took steps to make the occupancy of the chemical chair more secure by persuading the Crown to make an annual grant of £100. It should be noted that initially there was no stipend attached to the chair that Cullen and Black occupied. They were remunerated by their students' fees and the takings of private medical practice.

Despite his inauspicious start, Watson was the first Cambridge chemist to evince any interest in the advance of industry based on exact chemistry knowledge which was taking place in various parts of the country. His "Essays", published in 1784–88, contain useful pictures of various industries, particularly on coal, lead and zinc, and his researches on charcoal production for gunpowder by closed distillation of

wood are known to have saved the Government large sums of money.

Black's influence was transmitted throughout the civilized world by the appointment of his students to influential positions in the academic and industrial world. Black was to Edinburgh what Boerhaave was to Leyden. Not only did his students, Robison, Irvine, Hope, Cleghorn, and Thomson, follow him in the lectureship at Glasgow and chair at Edinburgh, but also others founded chemical schools throughout the country and abroad. Ogilvie went to Aberdeen, Thomas Garnett to the Andersonian University of Glasgow, J. Morgan and B. Rush established the teaching of chemistry at Philadelphia. Wm. Henry, the Manchester chemist, studied under him, as did Sir Humphry Davy's brother John. Humphry regretted that it had not been his own good fortune to study under Black. But of particular importance here is that two of his students had a vitalizing effect on the lethargic schools at Cambridge and Oxford. Let us follow the evolution of Oxford after the death of Mayow referred to above.

Oxford chemistry did not recover easily from the loss of Mayow, although one might have expected the Ashmolean foundation to bring about a revival. A number of chemists followed in the laboratory established by Elias Ashmole (Robert Plot, Edward Hannes, John Freind, Richard Frewin), but none of them succeeded in establishing any sustained teaching or research school.

"The reason for this sterility was not far to seek. The Oxford contemporaries of Newton had not the enquiring mind; the most brilliant of her sons devoted their genius to other ends and developed their talents in other places; those who stayed behind were content to accept the statements of others without testing them for themselves, and to pass on to their students information acquired at second-hand. The business of teaching was set higher than the duty of research." (R. T. Gunther, "Early Science in Oxford", 53.)

Ashmole's inadequate foundation was, from the chemical point of view, a failure, and chemistry continued to lag behind other expanding sciences. No university professor was appointed, with the result that students who wanted to acquire some familiarity with the science had no one better to instruct them than the college fireman.

The only interesting outcome of the Ashmolean period is the association of John Wall (1708–76) of Merton with the foundation of the Worcester Porcelain Company (1751); but such a connexion cannot be considered adequate to compensate for the new low level to which Oxford intellectual life sank in the earlier part of the eighteenth century. Wall's connexion with Oxford was strengthened through his son, Martin Wall (1747–1824), delivering a course of lectures there from 1781 in the capacity of 'public reader in chemistry'.

The next development in the chemical history of Oxford was of great importance (corresponding as it did with the appointment of Smithson Tennant to Cambridge): it was the appointment of Thomas Beddoes (1760–1808), also a student of Joseph Black's at Edinburgh, to be reader in chemistry. Beddoes was only at Oxford from 1788 until 1793, but for a time at least chemical interests there were stirred up by his enthusiasm; and it is on record that such was the revitalized interest he created that attendance at his lectures exceeded anything known in the University since the thirteenth century. Here in Oxford was the vivid effect of Black's infectious personality re-enacted. Beddoes' short readership in chemistry was but a phase in his life, and the Pneumatic Institute at Bristol which he founded spread Black's influence in another direction. It cannot be said that Beddoes' short sojourn at Oxford led to any great chemical efflorescence; but in 1803 a professorship of chemistry was endowed, and with it the establishment of regular teaching, something that had been in progress in Edinburgh for almost a century.

Cambridge was rather more fortunate than Oxford with its chemists. Richard Watson was followed by Isaac Pennington, and he in 1793 by W. Farish, who like Cullen in Glasgow lectured on the "Application of Chemistry to the Arts and Manufactures of Britain". In Farish's lectures we see a swing-over to an appreciation of the important contribution that chemistry was making to the industrial revolution. They covered smelting metallic ores, the uses of coal, such industrial chemicals as sulphur, alum, salt, acids, and alkalis, the chemical arts of bleaching and preparing cloth, and the production of mordants, etc. This highly practical approach heralded the further break with tradition, namely, the appointment of a chemist trained in the Scottish schools to the Cambridge chair. In 1813 Smithson Tennant (1761–1815), who had been in Cambridge since 1782, was appointed to the vacant chair of chemistry.

By this time Scottish chemistry, nurtured in the faculties of medicine at Edinburgh and Glasgow, had achieved a European reputation, and so the successors of Cullen and Black had an assured flow of talented students out of all proportion to that which came to Tennant and Beddoes, despite the latter's popularity as a lecturer.

When Black went to Edinburgh from Glasgow, he was succeeded by J. Robinson (1739–1805), and he in turn by William Irvine (1743–87), both students of his own. Irvine died in 1787, and was succeeded by Thomas Charles Hope (1766–1844). Hope only occupied the chemistry lectureship for four years before transferring to the chair of medicine, but his interest in research and his ability as a teacher maintained the reputation of the Glasgow school built up by Cullen and Black, whom he ultimately followed at Edinburgh as well (1799). Hope added still another of the elements (strontium) to be discovered by Scotsmen. On his translation to medicine he was succeeded by Dr. Robert Cleghorn (1777–1821), who continued to lecture on chemistry until an independent chair was founded in 1818.

This was an era of great industrial development by the application of chemistry to the arts in Scotland, during which the link-up between industrialists and universities was further strengthened. One need only mention in passing the introduction of chlorine bleaching at Gordon Barron and Company's Woodside Works through the activities of Prof. Patrick Copland (1749–1822), professor of natural philosophy in Marischal College, Aberdeen; the production of the dyestuff *cudbear* and the development of turkey red dyeing by George and Charles Macintosh, the latter a student of Black; the patenting of bleach liquor and bleaching powder in the name of Charles Tennant of St. Rollox, and the general contribution made by the Tennant-Macintosh nexus in the way of heavy chemicals and ancillaries to the dyeing and finishing trades. Under Hope's influence the development of chemistry was rapid and of increasing economic importance. On account of his professional

contacts, Hope in some ways occupies a place of equal importance with Black, because increasing numbers of industrialists (for example, the Tennant and Macintosh group) were in a position to benefit by contact with chemistry in the universities of Scotland. The popularity of chemistry with all classes in Scotland became so great that Hope sometimes had five hundred students attending his lectures, and outside the University, interest was every bit as great. He continued to lecture until 1844, when he was succeeded by Dr. William Gregory as independent professor of chemistry, fully a quarter of a century later than the foundation of an independent chair in the more highly industrialized city of Glasgow.

LONDON'S WATER SUPPLY: SAFEGUARDING ITS PURITY IN PEACE AND WAR*

By LIEUT.-COLONEL E. F. W. MACKENZIE, O.B.E.
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History

NO story of London's water supply would be complete without some brief account of the historical background from which have emerged the methods of purification which now form our vital defences against the transmission of the germs of water-borne disease.

Prior to the fourteenth century, the citizens of London obtained their water from the River Thames and its tributary streams, or from springs and wells, which were plentiful, At that time the supply of water was a duty of the City Corporation, and it remained so until 1582, when a Dutchman named Peter Morrys was granted a 500-year lease at the nominal charge of 10*s*. per annum, with the right to supply water drawn from the River Thames by pumps driven by water-wheels set in one of the arches of the old London Bridge. This undertaking remained in the hands of the Morrys family until 1701, when it was transformed into a company, which also acquired the city conduits. Thus was the duty of supplying water to London relinquished by the constitutional authority and handed over to private enterprise.

The next incident of note was the construction of the New River, opened in 1613, to convey pure water from springs in Hertfordshire to the City. The success of the New River Company led to the granting of power by Parliament to other companies for the purpose of supplying water, and between the years 1669 and 1806 no fewer than seven such companies were promoted. At the time of their formation, those of the companies which drew water from the River Thames had their intakes in the tidal pool, which became increasingly polluted by the ordure of the City. This led to the succession of serious epidemics of cholera in London during the nineteenth century.

Meanwhile, however, two important measures had been taken: first, the introduction in 1826 of filtration through sand, and second, the passing of the Metropolis Water Act of 1852, which prohibited the abstraction of water from the River Thames below Teddington Weir and imposed, as a legal obligation, the filtration of all river-derived water and the covering of service reservoirs. The Metropolis Water Act of 1871 further contributed to the cause of purity by the appointment of an impartial water examiner who transmitted the reports of the analyst, at that time Prof. Frankland, to the Local Government Board, and who had other duties mainly inspectorial in nature. The water supply, however, continued to be the subject of public agitation, which culminated in the passing of the Metropolis Water Act of 1902, by which the Metropolitan Water Board was created to take over from the companies the duty of supplying water to London.

The Act of 1902 also placed upon the Board certain duties in connexion with laboratory examination designed to ensure the safety of the supply. This was the genesis of the present Water Examination Department, which came into being in November, 1905, with the appointment of Dr. (afterwards Sir) Alexander Houston as the first director. There were, thus, two persons whose duty it was to safeguard the purity of the supply: first, the water examiner, who was an officer of the Local Government Board; and second, the director of water examination, who was an officer of the Metropolitan Water Board. This state of affairs continued until the appointment of water examiner was abolished in 1921, and the duty of safeguarding the purity of the supply thus devolved entirely upon the director of water examination. In 1904 the Metropolitan Water Board finally took over the private companies, and the duty of supplying water to London was taken out of the hands of private enterprise and restored once more to the control of the representatives of the people, by whom it had been voluntarily surrendered more than two centuries before.

Methods of Purification

Until 1909, filtration through slow sand filters was relied upon for the purification of the river-derived water. Sir Alexander Houston, however, was responsible for the introduction of a number of revolutionary changes, chief among which were the regular use of water which had been purified by passage through a storage reservoir (1909), chlorination (1916), the use of primary mechanical filters antecedent to slow sand filtration (1923), and the use of ammonia as a means of reducing the tastes produced by chlorine alone.

The Metropolitan Water Board now comprises twelve filtration works and some sixty well stations. It supplies an area 575 square miles in extent containing more than 7,000,000 people. The water is supplied through a distribution system of pipes 8,000 miles in length. Approximately two-thirds of the water is derived from the River Thames, one-sixth from the River Lee, and one-sixth from deep wells sunk in the chalk.

The wells are usually of great depth and the water delivered from them is of excellent physical quality. For many years it was supplied without any treatment, but the increasing urbanization of the country districts around London and the excessive pumping which now takes place has led to a progressive deterioration in the quality of the water lying in the great chalk basin beneath London, and this has necessitated the chlorination of all well-derived water, but no other treatment is required.

The river waters, on the other hand, are heavily polluted and require somewhat elaborate purification.

* Substance of a discourse delivered at the Royal Institution on December 8.

Briefly, the methods employed at the outbreak of war in 1939 were storage followed by filtration and chlorination.

The high rate of filtration made possible by the use of primary filters had detracted from the efficiency of the slow sand filters and had made it necessary to reinforce the purification processes at some works by the use of chlorine both before and after filtration. This prefiltration treatment has conveniently been termed 'prechlorination'.

These methods enabled the production of water in keeping with the standards of bacterial purity widely accepted prior to 1939. In that year, however, a more stringent test of purity received authoritative approval, and immediate steps were necessary to bring the water to the desired level. This involved the use of prechlorination at all filtration works, together with certain adjustments designed to render it more efficient, and an increase in the terminal dose of chlorine to a point at which it is, at times, liable to give rise to some taste in the water when it reaches the consumer. With this exception the methods have proved successful, but it will be possible to surmount the difficulty of occasional chlorinous tastes without some sacrifice in purity only by the construction of large tanks in which the water may be retained, before delivering it to consumers, for a sufficient time to enable a small dose of chlorine to do the work for which, at present, a comparatively large dose is required. The construction of these contact tanks has not been possible during the War, but it has become the accepted policy of the Board that they shall be provided when circumstances permit.

Unfortunately, chlorinous tastes are not the only ones with which water engineers are afflicted. The river waters have tastes which vary from 'earthy' to 'mouldy'; the reservoirs are subject to algal growths which, by the liberation of essential oils, impart a wide range of tastes which have been variously described, the most common being 'aromatic', 'fishy' or 'grassy'. Until recently their complete elimination has been difficult or impossible, but the adaptation of activated carbon to waterworks practice has opened up a new field for the removal of tastes. Up to the present, it has necessarily been used by improvised methods, but it is hoped that it will be possible, after the War, so to adapt the works as to render this valuable treatment fully effective. The complete elimination of tastes from London's water supply should then be possible.

The Board has provided in the most ample manner for the statutory requirements in regard to water examination set forth in the Act of 1902. The Laboratories in Clerkenwell which were opened in 1938 are an outstanding example of a building which, in the highest degree, combines utility with architectural and artistic merit. Every unit in the building has been designed for a special purpose, and serves that purpose with the utmost efficiency. A high proportion of the staff have devoted most of their lives to particular aspects of water analysis.

The routine control of quality is exercised by the daily examination of samples representing the water in every stage of purification. Widespread random sampling from mains and consumers' taps throughout the area of supply is also practised, and enables the quality of the water as received by the consumer to be kept under frequent observation.

During the year 1943 no fewer than 38,351 such routine analyses were made. The work of the department is, however, by no means limited to this routine. Constant researches are made for improved methods, and special investigations are performed for the elucidation of purification and other problems which arise in connexion with the works. These have, in the past, contributed much to the sciences of water analysis and purification.

War and the Water Supply

Before describing the measures which were taken to protect the water supply of London against pollution from war damage, it would be well to picture what might have been the consequences had the water from one of the Board's major works become infected with the germs of typhoid, which is now, in England, the most to be feared of all the water-borne diseases.

Judged by past epidemics, it might be expected that infection of a filtration works of average size would, at the lowest estimate, result in 16,000 cases of typhoid with 1,600 deaths and the establishment of between 300 and 800 permanent carriers of the disease, as a reservoir of infection for the creation of further outbreaks. The significance of this can best be appreciated by comparison with the Croydon epidemic of 1937, in which there occurred 323 cases and 43 deaths. Many will recall the widespread feeling of alarm and the sense of insecurity which prevailed among those residing within the area affected by this comparatively minor outbreak.

It is easy to picture what might have been the effect upon the morale of the people of so disastrous an occurrence, and of the knowledge that each such incident would increase the probability of further similar epidemics.

High in the order of priority for protective measures came the necessity of ensuring that there should be no interruption in laboratory examination and control of the supply, for without this there would have been no means of assessing the need for, or the success of, other measures. Among the first steps to be taken were the selection of suitable buildings in comparatively safe areas and their conversion into alternative laboratories.

The use of prechlorination was extended to all filtration works. This increased the main lines of defence to four, namely, storage, prechlorination, filtration and terminal chlorination. This was undoubtedly the most important factor in preserving the purity of the water pumped from the works, for there was no occasion upon which less than two of these lines remained intact.

The residual chlorine in the water passed into supply was increased to the highest level consistent with the avoidance of serious cause for complaint. This provided against pollution through infiltration of ground-water into broken filtered-water channels and also gave some protection against failure to effect complete sterilization of fractured mains as a result of either physical difficulties, which often existed, or the fallibility of human nature. The value of residual chlorine has frequently been exaggerated; but its limitations were fully appreciated and its presence was not permitted to allow of any relaxation in the more positive methods of protection.

The possibility of the introduction of chemical poisons into the water either deliberately, or accidentally, as by the use of poison-gas bombs, was countered by a system of guards at works and reservoirs, and by arrangements for the immediate testing at the works for poisons should there be any suspicion that the water might have become contaminated.

It will be evident that chlorination was to play a most important part in our defensive measures, and it was necessary that every possible step should be taken to prevent any interruption in this vital process. Although chlorination had been in use by the Board for a considerable time, it was still regarded as somewhat subsidiary to the older purification processes. This view had now to be revised, for acts of war might at any moment have rendered it the most important or, indeed, the only barrier against the spread of water-borne diseases. A wholesale programme of reconstruction was planned and received the immediate approval of the Board. This work involved the detailed consideration and replanning of more than 150 chlorination points.

There could be no doubt that the most serious danger would be created by the fracture of water mains and sewers in close proximity in the streets, resulting in the admission of sewage to the water mains. Protection against the consequences of such incidents was provided by the rapid closure of valves by turncocks and by instructions to the effect that no main should be put back into supply after repair until it had been thoroughly flushed and disinfected by chlorine. Whenever possible, repaired and sterilized mains were examined bacteriologically before being restored to supply, but frequently they were so urgently required for fire fighting that this could not be justified. Sterilization by chlorine was, however, never omitted. Special mobile chlorinators were provided, and the whole of London was divided into areas to each of which some were allotted. Central control was maintained and a reserve of chlorinators was held directly under my orders, thus enabling relief to be provided in districts where the work was exceptionally heavy. Some difficulties in the rapid sterilization of mains were encountered in the early days, due chiefly to the almost complete disorganization of communications, but these were rapidly overcome and, as experience was gained, the whole organization worked smoothly and without avoidable delay.

It was not only necessary to conserve the purity of the supply. When the enemy resorted to fire raising, quantity became second only in importance to purity, for, without water, London might have been destroyed by fire. The greatest need for water frequently coincided with heavy damage to the works and consequent limitation of their output. Decisions had to be taken, often on the spur of the moment, as to the extent to which orthodox methods of purification might be discarded to increase production without imperilling the health of the people. There is no hard and fast line between a safe and an unsafe water, and such momentous decisions were not easily taken, for they had, of necessity, to be based upon personal opinion. It must be admitted that the taking of them was assisted by the gravity of the situation, and it can fortunately be said that there was no occasion when the quantity of pure water available was insufficient, nor was there, at any time, any deterioration in its bacterial purity. Local shortages there were, but these were due to the immense damage to the mains; and this was remedied with remarkable rapidity by the engineering staff of the Board.

Emergency Water Supplies

It was evident that, whatever precautions might be taken, interruption of piped supplies might occur. Arrangements were therefore made to deliver pure water by tank wagons and to provide emergency supplies if necessary from private wells and casual surface water sources. An organization was set up for the purification of these waters, which entailed the training in emergency purification methods of more than 7,000 volunteers.

During the aerial attacks on London, almost every conceivable form of damage which might have prejudiced the purity of the supply was inflicted upon the undertaking. Damage to sewers in the outskirts of London necessitated the discharge of untreated sewage into the rivers from which raw water supplies were drawn; reservoirs were cut off from the works by the destruction of aqueducts, thus necessitating the passing of unstored raw river water on to the filters; bombs fell into the filter beds and caused short-circuiting between the unfiltered and the filtered water channels. At times it was necessary to bypass the slow sand filters to enable the supply for fire-fighting to be maintained. Damage to the distribution system was particularly severe. In one night alone, more than five hundred mains were fractured and many became heavily charged with sewage.

All the necessary protective measures were, however, in readiness, and there is not a shred of evidence that the water supplied to London was at any time less safe than before the War. During the four years 1940–43, bacteriological analyses were performed on more than 50,000 samples drawn at the works, from mains pumping water into supply and throughout the distribution area, particularly in those localities where bomb damage had been heavy. Of these 50,000 samples, 99·3 per cent showed the absence of *Bact. coli* in 100 ml., that is to say, they conformed to the highest standard of bacterial purity despite the extensive damage which was frequently inflicted and the many opportunities for dangerous pollution which were created. This represents a purity, during these years of war, higher than ever before, and it has never been suggested by any health authority that a single case of disease occurred in London which might have been attributed to the water supply.

POLYMER-PLASTICIZER INTERACTION

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AT a meeting of the Plastics Group of the Society of Chemical Industry on November 17, 1944, polymer-plasticizer interaction was discussed. Owing to the wide range of the discussion and in view of topical interest in this subject, it has been thought desirable to present here a preliminary account which covers some of the points raised; more detailed papers will appear elsewhere. Some such general account seems to be overdue, as previous ideas on the subject would seem to need revision as a result of recent advances in polymer thermodynamics.

Though all thermoplastic polymers soften on heating, they also, in common with other organic compounds, tend to decompose as well. Moulding temperatures for any particular polymer must therefore be chosen to suppress this decomposition as much as possible. Some polymers can be moulded with negligible decomposition, whereas with others the temperatures required for moulding are too high,

even with the intelligent use of anti-oxidants. Historically, plasticizers were first used in a systematic manner in order that intractable polymers might be moulded at temperatures which were not too damaging; their purpose was to make the composition more plastic (or less viscous) at a given temperature, and the name 'plasticizer' perpetuates this property, though the fact that the elastic properties were also profoundly altered was soon realized.

To explain the action of plasticizers, it is necessary to start with the current picture of an amorphous polymer, which is that of a tangled collection of randomly kinked long-chain molecules; if certain reasonable assumptions are made about the flexibility of each chain, this qualitative picture can be transformed into a formal model in terms of which many of the observed properties of polymers can be described. These properties are a consequence of the tangled structure, and the effect of a plasticizer is presumably to break down or loosen this structure. Early theories of plasticizer action, allowing for this, suggested that the plasticizer acted as an internal 'ball-bearing' or lubricant by virtue of which neighbouring portions of polymer could slide over each other more rapidly. Though satisfying to the mechanically minded, the physical chemist finds such a picture inadequate; for, besides not accounting for the specificity of plasticizers, it completely avoids the crucial problem of how the plasticizer is distributed throughout the polymer. Is it a discrete physical mixture, like sand and sugar, or does dispersion on a molecular basis form a solution of plasticizer in polymer? In fact, any mechanical theory leaves for later consideration whether the 'ball-bearing' is a single molecule, a discrete visible droplet or anything between these two extremes.

It is now realized that plasticizers can belong to either of the above two main classes, these being termed non-solvent and solvent types respectively. Two of the oldest plasticizers for nitro-cellulose form conveniently contrasting examples of these. Castor oil is a non-solvent plasticizer and is dispersed in the nitrocellulose as discrete droplets which are sometimes visible with a microscope; many rubber 'extenders' also come into this category. As a result of internal diffusion in the plastic, droplets of non-solvent types will tend to coalesce to larger aggregates and ultimately to 'sweat out'. On the other hand, a typical solvent plasticizer for nitrocellulose is camphor; this is dispersed as single molecules forming a true solution, in contrast to the two-phase system of the non-solvent type. For the above reason, solvent plasticizers are preferable for nearly all purposes, and these are now discussed further; for the moment, only amorphous polymers and liquid plasticizers are considered.

Previously it has been hard to accept the idea of a plasticizer dissolving in a polymer owing to difficulties in visualizing the nature of the solution. To disperse a plasticizer molecularly, it was thought necessary to assume a specific attraction between the plasticizer molecule and the polymer unit, many references to a polymer-plasticizer complex being found in the literature. Similarly, the physical fact that raw rubber swells and finally dissolves in a solvent like benzene tends to reinforce the view that there is a relatively strong polymer-solvent interaction compared with the polymer-polymer one, and that this is the main factor influencing the swelling and solubility of macromolecules[1]. Recent work has tended to deny this view completely, and also explains why different plasticizers have different compatibilities with a specific polymer.

From the point of view of solubility, a polymer is now considered as a liquid; hence polymer-plasticizer (or polymer-solvent) mixing is exactly similar to that of two simple liquids and subject to the same thermodynamic principles. Two liquids will mix to form a single phase only if there is a decrease in Gibbs free energy as a result of the mixing process, that is, $\Delta G < 0$. Now, the decrease in Gibbs free energy can be split into the usual heat term and entropy of mixing term ($\Delta G = \Delta H - T\Delta S$). In the case of a polymer dissolving in a solvent, experiment reveals that ΔS is generally abnormally large and positive. The theoretical reason for this was first suggested by Meyer[2]; it is due to the fact that a long-chain molecule in solution can take up a large number of distinguishable configurations. This circumstance alone provides a large positive mixing entropy which is sufficient by itself to bring about solution. A polymer-plasticizer complex (and similarly a solute-solvent one in a liquid mixture) means that the heat term ΔH is strongly negative and therefore assists solution, instead of being positive as it is for most binary mixtures. ($\Delta H > 0$ corresponds to absorption of heat on mixing.) With polymers, the presence of a large positive ΔS means that it is unnecessary to invoke any specific interaction with the plasticizer to explain the solution process.

Meyer's original suggestion was formalized into a definite model the statistical thermodynamics of which were worked out independently and simultaneously by Flory[3], Huggins[4] and Miller[5] for the case $\Delta H = 0$ with essentially similar results; the more difficult extension for $\Delta H \neq 0$ has recently been made by Orr[6] and Guggenheim[7]. The results show that, provided ΔH is not too large, ΔS is almost independent of it. (A similar result holds for simple liquid mixtures and is the basis of Hildebrand's regular solution approximation[8].) It follows from this that if ΔH is strongly positive, then ΔG can also become positive; hence the mixture of polymer and plasticizer will separate into two phases. The theory predicts that one of these will be swollen polymer and the other almost completely pure plasticizer, in agreement with experiment[9]. Hence, at a given temperature, the composition of the swollen phase is the maximum amount of plasticizer which can be incorporated in a polymer for that specific pair of substances. Any excess plasticizer will 'sweat out' and form a separate phase similar to a non-solvent plasticizer. It seems possible that some of the evidence in the literature of polymer-plasticizer complexes containing 20–30 per cent plasticizer may be due to incompatibility effects of this type.

The problem has thus resolved itself into a measurement of the heats and entropies of mixing (or dilution) of a polymer with a plasticizer, with the emphasis on the heat term. In theory, relative vapour pressure measurements of the plasticizer in the polymer will give all the required information, provided a sufficiently large concentration and temperature range is covered. The heats involved are, however, small and therefore subject to large relative errors; even under the most favourable conditions, as in Gee and Treloar's classic study of the rubber-benzene system[10], the probable error in the estimated heat of dilution is $\pm$ 30 per cent[11]. In studying differences between plasticizers, which are usually chosen to have a very small vapour pressure, this line of attack is distinctly unpromising and will remain so until

pressures of 10^{-2}–10^{-4} mm. can be measured with as much precision as those of 10–100 mm. Direct measures of the equilibrium swelling or the total heat of mixing are difficult with 'hard' plastics, such as polyvinyl chloride, as equilibria are reached very slowly.

For the above reasons, it has been necessary to develop indirect methods of estimating ΔH, and even so, only relative results can be obtained. One method relies on the assumption that the polymer-plasticizer heat of mixing is proportional to the corresponding monomer-plasticizer heat effect. This is a pure assumption, though a reasonable working one, and can perhaps be checked later by heat of dilution measurements on a low polymer. The problem then becomes a measurement of the mixing heats between plasticizer and monomer, which is feasible experimentally; with vinyl polymers, the saturated monomer is preferred to avoid double-bond complications, for example, ethyl benzene for polystyrene. A second method uses the concentration variation (c) of the specific viscosity ($\eta_{sp.}$) of dilute solutions of polymers as a measure of ΔH. Alfrey, Bartovics and Mark[12] found that, for a given polymer, both the limiting value of $\eta_{sp.}/c$ at zero concentration and the slope of the $\eta_{sp.}/c - c$ line were dependent on the solvent; 'good' solvents increased both the slope and the intercept, whereas 'bad' ones depressed them. The concept of 'good' and 'bad' solvents can be formulated thermodynamically and a semi-quantitative treatment of the viscosity effects has been developed[13]. It is not often possible to measure dilute solution viscosities in pure plasticizers; to get over this difficulty, the polymer is dissolved in a mixture of 'indifferent' solvent ($\Delta H = 0$) and plasticizer. The slopes of the $\eta_{sp.}/c - c$ lines vary with the plasticizer used in the solvent mixture and are in agreement with their known plasticizing properties with a given polymer. Though the theory of the effect is admittedly not complete, the method is probably the most convenient one for estimating relative interactions. The light scattering of polymer solutions[14] is a possible means by which interactions might be studied in the future, but the technique is still in its infancy; the published results suggest a strong dependence of the effect on solvent type.

So far, discussion has been confined to polymers which are completely amorphous. For crystalline bodies, the total free energy change for the solution process can be split into two stages: (1) melting of the crystals to give an amorphous polymer ($\Delta G_{melt.}$); (2) mixing of the amorphous polymer with the plasticizer ($\Delta G_{mix.}$). For amorphous polymers, (2) is the only relevant process and the factors governing $\Delta G_{mix.}$ have already been discussed. If the melting range of the crystalline polymer is above room temperature, then $\Delta G_{melt.}$ will be positive; its magnitude will depend on the interval between room temperature and this range. For a high-melting polymer $\Delta G_{melt.}$ can become larger than $\Delta G_{mix.}$ for most solvents, in which case the crystals will be completely insoluble at low temperatures, as the total free energy change is positive. For this reason, crystalline polyethylene is insoluble in nearly all solvents at room temperature. As the temperature is raised, the melting range is reached in which there is an appreciable proportion of amorphous material in thermodynamic equilibrium with the crystalline regions[15]; this amorphous polymer is miscible with solvents, and the rapid increase in polyethylene solubility in nearly all solvents at about 70° C. is due to the operation of this factor. In such circumstances, a plasticizer will tend to dissolve in the amorphous regions and leave the crystalline ones untouched; only a strong relative attraction ($\Delta H < 0$) between plasticizer and polymer link will cause the crystalline regions to dissolve.

In the foregoing paragraphs, an attempt has been made to give the thermodynamic background of the polymer-plasticizer compatibility problem. Such an approach does not give any information at all about the relative plasticizing efficiency of two compatible substances—this is not surprising as it forms a completely unrelated problem. The efficiency of a plasticizer is now suspected to be almost completely determined by the size and shape of its molecule, but a large amount of accurate data is required before this problem can be pursued further.

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OBITUARIES

Dr. D. S. Raitt

THE tragic death of Douglas Raitt as the result of a motor accident on October 4, 1944, at the early age of forty-one, will be deeply regretted not only by marine biologists but also by the general public, for apart from his scientific attainments, he had wide cultural interests and was recognized as a successful broadcaster and as a composer of Scottish songs. In particular, fishery research has lost a most promising worker, who had a brilliant career before him.

Born in Aberdeen on January 18, 1903, Raitt was educated at Robert Gordon's College there and at the University of Aberdeen. In the University he came under the influence of Prof. J. Arthur Thomson, who undoubtedly did much to foster in him originality, precision in workmanship and a philosophic outlook on life. Immediately after graduating B.Sc. in 1926, he was appointed a probationer naturalist on the scientific staff of the Fishery Board for Scotland, being placed on the established grade two years later.

With the resumption of fishery research after the War of 1914–18, the study of haddock in all its aspects was delegated to Scotland by the International Council for the Exploration of the Sea, and Raitt joined the Scottish team of workers so ably led by Dr. A. Bowman, then scientific superintendent. His first contribution to the series of publications was on the fertility of the haddock. After the transference to Newfoundland of Dr. Harold Thompson, who had carried out brilliantly the survey of the haddock material and laid the foundation of the work, Dr. Raitt was given the task of continuing the research and producing forecasts of the future yield of the stocks, largely for the benefit of the fishermen and the trade. This led to the appearance of a series

of valuable papers published by the International Council and by the Fishery Board for Scotland (now the Fisheries Division, Scottish Home Department), two of the most important being "The Haddock Stocks of the North-East Atlantic" and "The Rate of Mortality in the North Sea Haddock Stocks"—the latter a definite contribution to the overfishing problem. These researches gained for him the D.Sc. in 1937. In 1939 he was appointed Buckland lecturer for 1940 to deliver a series of lectures on the haddock, but owing to the outbreak of war the lectures were never delivered.

Raitt was a fellow of the Linnean Society and of the Royal Society of Edinburgh and published papers on various aspects of marine life in different scientific journals. In one of these he described a new species of sandeel from British waters—no mean achievement these days.

He had a great gift for organizing and staging shows and was largely responsible for two fishery exhibits put up by the Fishery Board for Scotland at Aberdeen and Glasgow, which were designed to illustrate the practical aspect of marine research as carried out by a Government department. The film "Sea Food", a record of the methods carried out at sea by the research vessels and in the laboratory, was also supervised by him.

During the present War, he was seconded to the Ministry of Home Security and held important posts under the district commissioners at Inverness, Edinburgh and Dundee.

He is survived by Mrs. Raitt and two sons.

R. S. CLARK.

Prof. Gustav Cassel

PROF. GUSTAV CASSEL, whose death at the age of seventy-eight occurred on January 14, was beyond doubt one of the outstanding figures in economic science during the inter-war period. His authority was second only to that of Lord Keynes, and his advice was eagerly sought on many occasions by his own Government and by foreign Governments. He played an active part at many international monetary conferences during the 'twenties and early 'thirties, and was head of the Swedish delegation to the World Economic Conference of 1933.

Prof. Cassel occupied the chair of economics at the University of Stockholm, and his "Theory of Social Economy" will always rank with the outstanding works on economic theory. First and foremost, however, he was a monetary specialist. His most important contributions to economic science were in the monetary sphere. In particular, he secured his place in posterity as the leading theoretical expert on foreign exchange during the chaotic period that followed the War of 1914–18. In his "Money and Foreign Exchange After 1914", he put forward the theory that under a system of inconvertible paper currencies the exchange-rates tend to represent the ratio between the internal price levels of the countries concerned; in other words, they tend to adjust themselves towards what he called their "purchasing power parities". When he first sought to popularize this conception, it was regarded as almost revolutionary, and the time-honoured theory according to which exchange-rates are determined by the trade balance died hard. He lived to see, however, the general acceptance of his principle.

In the sphere of monetary policy, Cassel was strongly opposed to deflation, and advocated low interest-rates during the late 'twenties and early 'thirties. As a member of the Gold Committee of the League of Nations, and in his various writings, he was concerned by the inadequacy of the volume of monetary gold to meet the requirements of expanding world production and trade. In 1937, however, Prof. Cassel, together with many other theoretical and practical experts, was misled by an apparent excess of gold supplies into a panicky advocacy of a deflationary policy.

Prof. Cassel did not believe in the device, so popular among many economists, of seeking to impress his readers by indulging in obscurantism. His books and articles were written for the most part in simple, clear language, understandable to the intelligent layman at the same time as being inspiring to the expert.

P. EINZIG.

Mr. E. Rothbart

ERWIN ROTHBART, acting lecturer in economic statistics in the University of Cambridge, who was killed in action in Holland in December 1944, aged thirty-one, was a refugee from Nazi Germany and an expert in economic theory and statistics; he specialized in the theory of industrial fluctuations and economic development.

Despite his originality, he had published little: his reticence and his horror of superficiality were such that his best work is probably to be found in the mass of unpublished manuscripts which he left. Some of these may be rescued for publication, but undoubtedly the more subtle of his ideas were still locked away in his mind. The quality of his thought was revealed in the penetrating book-reviews which he contributed to the *Economic Journal*, and in the formal and informal discussions on economic theory in which he took part with the younger economists at Cambridge and London.

Rothbart first became prominent in discussions in 1936 at the meetings held regularly by Profs. Robbins and Hayek at the London School of Economics. There, with A. P. Lerner, O. Lange, N. Keldor and M. Kalecki, he found himself one of a small group championing the latest theories of J. M. Keynes with an almost religious insistence. Two years later he was appointed research assistant in economic statistics at Cambridge, and in 1940 he took over the teaching of economic statistics, and continued it until he insisted on joining the Army.

Rothbart was a fine example of that combination of opposites, which is the rule rather than the exception in the workings of the human mind. Quick in thought, he was slow in action and self-expression. A self-taught mathematician, he delighted in the subtler turns of economic theory; yet his awareness of practical and political realities was no less acute. A lover of liberal values, his political sympathies were nearer to the hard discipline of the communists: with an impish love of life, he combined a recklessness that courted death. He leaves a widow and young son.

D. G. CHAMPERNOWNE.

WE regret to announce the following deaths:

Dr. Alexander Duckham, chairman and governing director of Alexander Duckham and Co., Ltd., lubrication technologists, on February 1, aged sixty-seven.

Prof. S. H. Gage, since 1908 emeritus professor of histology and embryology at Cornell University, on October 20, aged ninety-three.

NEWS and VIEWS

Sir John Orr, F.R.S., and the Rowett Research Institute

SIR JOHN BOYD ORR'S wide circle of friends will greatly regret that he has tendered his resignation from the directorship of the Rowett Research Institute after holding that post since its foundation, in which he took a prominent part. All over the world it is recognized that to a very large extent it was his vigorous and stimulating direction that made "the Rowett" one of the outstanding research centres where problems relating nutrition to agriculture have been studied. The wide recognition to-day that planned agriculture is the only sound foundation of a national nutrition policy is one direct result of Sir John's teaching and influence. His ideas prompted the appeal for "the marriage of agriculture and nutrition" that the Right Hon. Stanley Bruce made to the League of Nations on an historic occasion. They can be regarded, therefore, as having been prominently in the mind of those who called together the United Nations Conference on Food and Agriculture at Hot Springs, Virginia, in 1943. Whatever benefits to the world at large may ultimately be derived from what happened at that great conference will be related in the minds of many people with the views that have dominated everything that Sir John Orr has said or written during the past twenty years.

Sir John's own research has been mainly of protein and mineral metabolism: but he is perhaps best known to the general public for his published works on the nutritional needs of the people of Great Britain. A very deep impression was made just before the War by the appearance of his book "Food, Health and Income". The data presented in this work have been used by sociologists all over the world, but, perhaps, no more important use has been made of them than when they were adopted as a background against which a nutritional policy for Great Britain during the period of the War was planned. Among Sir John's many contributions to the advancement of nutritional science, there can be mentioned his establishment at the Rowett Research Institute of the Imperial Bureau of Nutrition and his foundation of *Nutrition Abstracts and Reviews*, of which he has been editor-in-chief since it first appeared. Sir John has been a member of many international and national committees dealing with matters concerning nutrition, more particularly in its relation to agriculture. He took an active part in the deliberations of the Technical Commission on Nutrition of the League of Nations and helped to draw up a number of the invaluable reports this Commission issued from Geneva. He served on the Advisory Committee on Nutrition of the Ministry of Health before the War and has since acted as chairman of the Scottish Scientific Advisory Committee. He was recently elected by a group of United States men of science to receive an award for the most outstanding work in the international field of nutrition. There will be everywhere an earnest hope that Sir John will continue to exert, perhaps in other spheres, the same powerful influence that he has exerted in the past. A great deal of enterprise and effort will be needed if the recommendations and resolutions of the Hot Springs Conference are to be implemented, even in the countries where the attitude of the politicians and the public is relatively enlightened towards social and economic problems. We understand that no successor at the Rowett Research Institute to Sir John Orr has yet been elected, but the post, which also carries with it the directorship of the Imperial Bureau of Nutrition and the editorship of *Nutrition Abstracts and Reviews*, will be advertised in due course.

Sir William Wright Smith

ON February 2, at the Royal Botanic Garden, Edinburgh, Sir William Wright Smith, King's Botanist in Scotland, regius keeper of the Royal Garden, and professor of botany in the University, was presented with a portrait of himself, on the occasion of his seventieth birthday. Sir John Stirling-Maxwell presided, and the presentation was made by the Earl of Stair. At the same ceremony a second portrait of Sir William was presented to the Botanic Garden by Sir John Fraser. The portraits, which were painted by Mr. Stanley Cursiter, were the gift of a large number of Sir William's botanical and horticultural colleagues and other friends. Sir William has been connected with Edinburgh since the beginning of his career. He was educated at the University, and was lecturer in botany there during 1902–7. In 1908 and 1909, he explored the vegetation of north-west Sikkim, and of the Tibet-Nepalese and the Sikkim-Chumbi frontiers, returning to Edinburgh in 1911 as assistant keeper of the Garden; in 1922 he was appointed regius keeper and professor at the University. Sir William's early explorations have given him a life-long interest in the mountain flora of India, Tibet and Nepal, and it is for his work on the classification and introduction of plants from these regions, notably primulas and rhododendrons, that he is best known to botanists and horticulturists all over the world. Under his genial direction, the great traditions of the Edinburgh Garden, and of the University Botanical Department, have been worthily upheld, and his many friends have welcomed this opportunity of showing him their admiration and affection.

Institution of Electrical Engineers: Faraday Medallist

THE Council of the Institution of Electrical Engineers has made the twenty-third award of the Faraday Medal to Dr. Clifford Copland Paterson, past-president, for the conspicuous services rendered by him in the advancement of electrical science, particularly in the field of electrical research. The Medal is awarded not more frequently than once a year, either for notable scientific or industrial achievement in electrical engineering or for conspicuous service rendered to the advancement of electrical science, without restriction as regards nationality, country of residence or membership of the Institution. On the staff of the National Physical Laboratory during 1903–19, Dr. Paterson took charge of the electrotechnical and photometric departments and was largely responsible for building up this section of the Laboratory. During the War of 1914–18 he participated in the inception and development of the Paterson-Walsh electrical height finder, which provided an automatic record of the heights of aircraft. He has been director of the Research Laboratory of the General Electric Company at Wembley from its inception, guiding the whole of its activities, which range from the heavy engineering field to electronics. His contribution to investigations into new methods

of artificial lighting and into electronic developments has been outstanding. During the present War he has collaborated with the Armed Forces and with the Supply Departments on many matters of outstanding national importance. Dr. Paterson was elected a fellow of the Royal Society in 1942.

Honorary Member

THE Council of the Institution of Electrical Engineers has elected Mr. John Somerville Highfield, past-president, to be an honorary member of the Institution. This distinction has been conferred upon him in appreciation of his distinguished work in the development of the science of the supply and application of electricity. Mr. Highfield became chief engineer to Stafford and St. Helens, Lancashire, after which he was appointed chief engineer and manager of the Metropolitan Electric Supply Company. As senior partner in the firm of Highfield and Roger Smith, he later acted as consulting engineer for the Central Electricity Board and several large electricity undertakings and industries in Great Britain and abroad. He also became a director of the London Power Company, the London Associated Undertakings, the Central London Electricity Ltd., etc. Mr. Highfield originated the closed-bar system for switchgear, discovered the reason for the failure of high-voltage alternator windings, and introduced to England the Thury system of high-voltage D.C. constant-current generation and distribution. During the War of 1914–18 he demonstrated at sea the use of shrouded hydroplanes for submarine detection.

Engineering and the Future

MR. FRANK PARFETT delivered his second presidential address before the Society of Engineers on February 5. He stated that there is a widespread and growing opinion among professional engineers in favour of federation of their institutions in order to have co-ordination of policy and joint representation on matters which are common to the whole profession. During the past year, two committees, the members of which came from various institutions, have been concerned with this matter, one of them dealing with national and the other with international federation. Very shortly proposals will be circulated to the councils of the bodies concerned for their consideration. Referring to the possibility of conscription after the War, Mr. Parfett said that the problem for peace-loving nations when war comes is to be ready and active with the minimum time-lag. The training of engineering personnel must therefore not be wasted. Young engineers in the future who are required to have army training should be sent to military engineering centres, and at the end of their term they should be entered on the National Register according to the sphere in which they could best serve the country in a national emergency. Factories, means of production, and personnel should be so organized that in the event of war we can immediately change from peace production to war production. Among post-war improvements suggested by Mr. Parfett was that of road and street lighting. He believes that main and secondary roads throughout Britain should be so illuminated that individual lighting on road vehicles would be unnecessary and merely reserved for minor roads. In all general utility articles, the public should accommodate itself to the acceptance of mass-produced articles from standardized designs. This would allow methods of production to be improved, which is the only way in which industry will be able to meet the higher wages now being demanded. Mass production does not mean the death of individual craftsmanship, for which there is still ample scope.

Conference on Science in War

THE Association of Scientific Workers is holding a Conference on "Science in Peace" on February 17–18 at the Caxton Hall, Westminster. The Conference is being organized in three sessions. The first, on Saturday afternoon, will deal with ways of securing an expanding economy and full employment. The problems of the effects of science on the productivity of labour will be introduced by a member of the Amalgamated Engineering Union, and specialists in each field will discuss in detail how science can be used to develop the basic British industries. The second session will deal with the more internal problems of science. The needs of fundamental research, and of applied science and technology will be dealt with, and papers will be read on the training of scientific workers, the organization and finance of science and information services. The third session will deal with the way in which science can affect the everyday life of each individual. The topics to be covered are research into consumer needs, health, food and agriculture, building and homes, and the place of science in culture. Tickets for the Conference (5*s*. for the three sessions, 2*s*. 6*d*. per session) and further information can be obtained from Mrs. B. Ryerson, Association of Scientific Workers, Hanover House, High Holborn, W.C.1.

Announcements

DR. HARLOW SHAPLEY, director of Harvard College Observatory, has been re-elected to the presidency of the Society of Sigma Xi, the American society for the encouragement of research in science. Dr. Frank B. Jewett, president of the National Academy of Sciences, has been elected member of the Society's national executive committee and Dr. M. C. K. Jones, of the Esso Research Laboratories, Elizabeth, N.J., is to be a new member of the national membership committee.

DR. B. A. KEEN, scientific adviser to the Middle East Supply Centre, has completed a second extensive tour of Middle East territories. He has now returned to England for further consultations, and the completion of his report on the problems of agricultural and rural development in that area.

DR. J. P. LAWRIE, senior press censor, Scientific and Technical Censorship, Ministry of Information, has, after five years service, resigned his appointment in order to take up new duties with the Royal Naval Scientific Service.

DR. HENRY DE LASZLO, managing director of L. Light and Co., Ltd., Old Bowry Laboratories, Wraysbury, fine chemical manufacturers, is visiting the United States, Mexico, Portugal and Spain. He will be making a survey of sources of organic intermediate chemicals manufactured in the United States, arranging for contracts for future delivery, and purchasing out of the way organic research chemicals not manufactured in Great Britain. Research workers and others requiring any particular substance are invited to communicate with Messrs. Light.

LETTERS TO THE EDITORS

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

Sub-crystalline Changes of Structure Accompanying Thermal Transitions in Rochelle Salt, and in Potassium Dihydrogen Orthophosphate

ACCORDING to the classical derivation of the Phase Rule, a transition *point* in a one-component system involves the co-existence of three independent phases, with the same thermodynamic potential of the component in three independent equations of state. For equilibria involving two solid and one vapour phase, the requirement that the phases shall be 'independent' can be given a definite interpretation in terms of crystal structure. For example, if we start with a single crystal of rhombic sulphur, and gradually heat it to above the transition point with monoclinic sulphur, the crystal may appear to preserve its external form, but X-ray photographs would show that actually it breaks up into a mass of smaller crystals of the monoclinic variety. These would give powder rings with the monoclinic spacings, insofar as they are independent of the original rhombic lattice. Corresponding with this rearrangement of the molecules to form a new crystal structure independent of the original arrangement, there are finite changes of volume and heat content at the transition temperature between the two crystal forms.

Thermodynamic transitions involving changes from one crystal structure to another at a transition *point* are said to be 'of the first kind'. We would prefer to call them 'discontinuous'. X-ray pictures illustrating the change from single crystal to powder have been published for the change from β- to α-resorcinol[1], and have also been observed[2] for the change of structure in KD_2PO_4.

An increasing number of examples is accumulating[3] of thermodynamic transitions 'of the second kind', which might be termed 'continuous'. In these, finite changes of volume and heat content at a transition *point* are replaced by 'anomalous maxima' on the expansion and specific heat curves, as if the phase change were spread out over a range of temperatures. Although some of these phenomena have been interpreted by statistical thermodynamics, it has not hitherto been clear how 'continuous' transitions affect crystal structure.

We have found in the case of Rochelle salt[4], and more recently in the case of potassium dihydrogen orthophosphate, that a break-up of the single crystal into sub-crystalline regions actually does occur over the transition range of temperatures. These sub-crystalline regions are, however, orientated in a definite relationship to the original crystal, and the discontinuities between them appear to be insufficient to lead to the formation of independent small crystals. Moreover, the break-up is reversible, in the sense that if the crystal is heated above the transition range again, X-ray pictures show that the sub-crystalline regions coalesce, and that the specimen again possesses all the properties of a single crystal. A single crystal broken up in this way may conveniently be called a 'hybrid'.

Up to the present, observations have been made by oscillating single crystals in a low-temperature chamber, at a series of temperatures, and making accurate determinations of changes in lattice spacings by means of a multiple-exposure spectrometer[5]. Changes in intensities of X-ray reflexions have also been observed, but will not be described here.

For Rochelle salt, over the transition range which begins at + 24·5° C. and terminates at about −20° C., no break-up of single crystals is observed when reflexions from planes parallel to the *c* axis are measured. But reflexions from planes parallel to the *a* axis and approximately bisecting the angles between the *b* and *c* axes are split into two components of slightly different Bragg angle. These two components are most clearly separated at about the middle of the transition range, around 0° C. Their presence shows that over the transition range a 'single' crystal of Rochelle salt is in reality made up of sub-crystalline units, which all have their *a* axis in common, and their *b* and *c* axes respectively equal, but which differ in that the angle between these two axes is either 90° 2′ or its supplement 89° 58′ apparently at random. These *b* and *c* axes coincide approximately in magnitude and direction with the *b* and *c* axes of orthorhombic Rochelle salt above 24·5° C.

It is noteworthy that if the 'single' crystal is cooled below about − 20° C., or is heated above 24·5° C., the split reflexions coalesce again. The change from a single crystal to a hybrid crystal with these sub-crystalline domains can be brought about repeatedly by the appropriate temperature changes, though there is some evidence that with repeated treatment the size of the sub-crystalline unit decreases.

In the case of potassium dihydrogen orthophosphate, when a single crystal is cooled below the transition threshold[6] at − 158° C., by dripping liquid oxygen (− 185° C.) or liquid nitrogen (− 195° C.) on to it, a corresponding break-up into sub-crystalline units is observed, which is even more striking in order of magnitude. When reflexions from planes parallel to the *a* or *b* axes are examined, the crystal appears to be 'single' down to the lowest temperatures so far observed. On the other hand, a break-up into sub-crystalline units is apparent at both − 185° C. and − 195° C. when the crystal is viewed normal to the *c* axis. The sub-crystalline units all have the *c* axis in common, and their *a* and *b* axes are equal to each other, and approximate in direction and magnitude to the original axes in the tetragonal form. But the angle between these two axes can be either 90° 27′ or its supplement 89° 33′, apparently at random. As with Rochelle salt, the change from single crystal to hybrid and back again can be brought about repeatedly by the appropriate temperature changes.

Further details of the structure of these hybrid single crystals are being examined. But even the experimental evidence so far obtained offers a suggestive picture of the structural basis of continuous thermodynamic transitions. It seems likely that when the change of crystal structure which would be involved in the appearance of a new phase is sufficiently small to be accommodated within the original lattice by a sub-crystalline break-up, the single crystal changes into a hybrid, instead of breaking up into new crystals. From the point of view of the Phase Rule, no new equation of state independent of the original structure can be formulated, so that there is no sharp transition point.

It is hoped that further experiments on these hybrid single crystals will throw light on the hysteresis which frequently accompanies continuous thermo-

The vacancies advertised in these columns are available only to applicants to whom the Employment of Women (Control of Engagement) Orders, 1942-3, do not apply.

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BRITISH ASSOCIATION OF CHEMISTS

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Applications, with the names and addresses of one or more referees, should be sent to the undersigned as soon as possible, and not later than March 1, 1945.

A. V. STEPHENS,
Professor of Aeronautics,
C/o, Scientific Research Liaison Office,
Australia House, Strand, W.C.2.

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County Offices, Chief Education Officer.
Chelmsford.

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dynamic transitions, and also on the formation of 'thermal mosaics' in crystals[7].

A. R. UBBELOHDE.
I. WOODWARD.

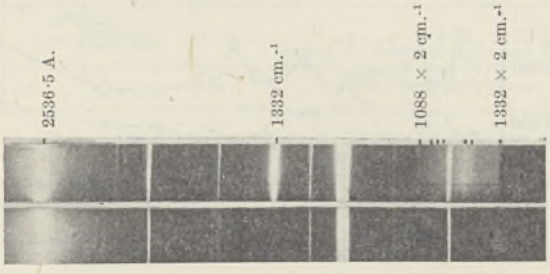
Davy Faraday Research Laboratory,
Royal Institution, 21 Albemarle Street,
London, W.1. Dec. 20.

[1] Robertson and Ubbelohde, *Proc. Roy. Soc.*, A, **167**, 138 (Figs. 5–7) (1938).
[2] Ubbelohde and Woodward, *Proc. Roy. Soc.*, A, **179**, 399 (1942).
[3] *Ann. Rep. Chem. Soc.*, **36**, 157 (1939); **37**, 167 (1940).
[4] In course of publication.
[5] Ubbelohde, *J. Sci. Instr.*, **16**, 155 (1939).
[6] Busch, *Helv. Phys. Acta*, 11, 269 (1938).
[7] Oldham and Ubbelohde, *Proc. Roy. Soc.*, A, **176**, 70 (1940).

Raman Spectrum of Diamond

THE new approach to the dynamics of crystal lattices made by Sir C. V. Raman[1] leads in the case of diamond to the result[2,3] that the atomic vibration spectrum of this crystal should exhibit *eight* distinct monochromatic frequencies. Of these, the highest frequency (1,332 cm.$^{-1}$ in spectroscopic units) corresponds to the triply degenerate vibration of the two Bravais lattices of the carbon atoms with respect to each other, this being *active* in the Raman effect. The other seven frequencies represent oscillations of the layers of carbon atoms parallel to the faces of the octahedron or the cube occurring normal or tangential to these planes with the phase reversed at each successive equivalent layer. All the seven modes of vibration of this description are *inactive* in the Raman effect as fundamentals. The *octaves* of these frequencies may, however, appear as frequency shifts in the Raman spectrum, though with intensities extremely small compared with that of the Raman line of frequency shift 1,332 cm.$^{-1}$. Besides the octaves, various combinations of these frequencies may also appear in the Raman spectrum.

The new lattice dynamics thus predicts that besides the frequency shift of 1,332 cm.$^{-1}$ corresponding to the so-called principal or fundamental oscillation, numerous other frequency shifts appearing as sharply defined lines should manifest themselves in intensely exposed Raman spectra of diamond. This result has been strikingly confirmed in an investigation already reported by me[4]. Since then, I have recorded spectra of much greater intensity and much better resolved, with the aid of a large quartz spectrograph and an exceptionally large plate of diamond of the ultra-violet transparent type recently acquired by Sir C. V. Raman. Under the conditions employed and using the 2536·5 A. resonance radiation from a water-cooled magnet-deflected mercury arc in quartz as the exciter, the Raman line with frequency shift of 1,332 cm.$^{-1}$ is recorded with an exposure of only two minutes. With an exposure of 72 hours, a satisfactory picture showing what may be designated as the Raman spectrum of the *second order* is obtained. This is the upper spectrum shown in the reproduction herewith, the lower spectrum being that of the mercury arc recorded with comparable intensity. It will be noticed that a whole series of discrete Raman lines appear in the former, which stand out on a feebler background evidently made up of unresolved combinational frequency shifts.



Above, RAMAN SPECTRUM OF DIAMOND; below, MERCURY SPECTRUM.

R. S. KRISHNAN.

Physics Department,
Indian Institute of Science,
Bangalore. Dec. 12.

[1] Raman, C. V., *Proc. Ind. Acad. Sci.*, A, **18**, 237 (1943).
[2] Bhagavantam, S., *Proc. Ind. Acad. Sci.*, A, **18**, 251 (1943).
[3] Chelam, E. V., *Proc. Ind. Acad. Sci.*, A, **18**, 334 (1943).
[4] Krishnan, R. S., *Proc. Ind. Acad. Sci.*, A, **19**, 216 (1944).

Shear Modes in Piezo-electric Crystal Plates

WHILE investigating the diffraction patterns produced by ultrasonic waves set up in a liquid medium by piezo-electric crystal plates, we made the following observations. Besides the usual thickness longitudinal mode, diffraction patterns corresponding to thickness transverse or shear modes have been observed occasionally. We find that the appearance of such patterns is facilitated when the crystal plates are either silvered in patches only, or so prepared that there is a deviation from the normal cut. When irregular silvering is adopted, not only do the odd harmonics of the shear modes make their appearance, but also the even harmonics of all the modes begin to show up. Such results have been observed by us in differently oriented plates of quartz and tourmaline and used for determining the elastic constants corresponding to the shear modes of these crystals. Details of these investigations are being published elsewhere.

These observations mean that particular shear modes cause longitudinal strains in the crystal plates resulting in corresponding longitudinal ultrasonic waves in the liquid. The phenomenon is presumably connected with the coupling between the longitudinal and shear modes produced either by the finite size of the plate or the cut of the plate, being such that the modes themselves are inherently coupled.

S. BHAGAVANTAM.
D. SURYANARAYANA.

Department of Physics,
Andhra University. Dec. 18.

Significance of Power-Law Relations in Rheology

IN studying the relationship between shear stress (S), strain (σ) and time (t) in the deformations of certain bodies showing complex properties, many rheologists have effectively used Nutting's equation[1], which is now usually written

$$\psi = S^{\beta}\sigma^{-1}t^{k} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

From the point of view of physics, this has been regarded as an empirical equation and its meaning has been difficult to envisage. Exponential equations can be pictured in terms of dash-pots and springs, but power-laws lead to no such simple models.

We would suggest that a helpful way of viewing the matter, at any rate for the most usual type of experiment in which the test-piece is strained at constant stress, lies in the relationship between power-laws and fractional differentials.

Scott Blair and Coppen[2], acknowledging the help

of Dr. P. White, have discussed the significance of a fractionally differentiated form of the Nutting equation in connexion with psycho-physical experiments, and write

$$\frac{\partial^n \sigma}{\partial t^n} = \frac{\Gamma(k+1)}{\Gamma(k-n+1)} \cdot t^{k-n} \psi^{-1} S, \quad \ldots \quad (2)$$

where $0 < n < 1$, and $\beta = 1$. This equation has also been noted by Whitehead[3] to be the differential equation with fractional coefficients which represents a power-law.

Scott Blair[3] has proposed, as an alternative to the Nutting treatment, that complex materials be considered to show a property intermediate between shear modulus and viscosity which is, like them, independent of stress, and defined as follows:

$$\chi = S \div \frac{\partial^m \sigma}{\partial t^m} \quad \ldots \quad (3)$$

We had not hitherto realized, however, that if the same exponent is used throughout in the above equations, so that $k = n = m$, equations (2) and (3) become identical, since $\frac{\Gamma(k+1)}{\Gamma(1)}$ is a pure number.

Since the property χ would seem to be more easily envisaged than are the Nutting constants, the identity of equations (2) and (3) clarifies our understanding of the rheological behaviour of complex bodies. At constant stress, $\partial^k \sigma / \partial t^k$ is constant; k being, as usual, zero for Hookean solids and unity for Newtonian fluids. The physical and psycho-physical significance of k has already been widely studied[4].

Where $\beta \neq 0$ and S is not constant, another fractional differentiation is, of course, required; though the treatment here is not so fully understood, nor perhaps so soundly based, as that for k. We are indebted to Dr. S. Whitehead for directing our attention to the fact that the treatment here proposed must be regarded in a sense as limiting equation (1). We had already appreciated[5] that equation (1) is only applicable in its integral form under specified conditions of stress and strain, and we are at present engaged in investigating its limitations experimentally.

Nutting's equation implies that for constant stress, for example, the rate of deformation at any time is proportional to the average rate from the start of the experiment up to that time; and further, the strain and time described as zero are arbitrary, since strain is now taken to include non-recoverable deformations and any figure may be defined as zero strain.

For materials for which $f(S,\sigma,t)$ is dependent on the history of the experiment, a direct plot of strain or time would therefore be misleading, since it would wrongly imply that a change of zero could be made in strain or time without altering $f(S,\sigma,t)$. The log-log plot does not suffer from this defect.

Maxwell[6] appears to have foreseen something of this when he suggested that relaxation times of complex materials might well be a function of stress.

J. E. CAFFYN.
G. W. SCOTT BLAIR.
National Institute for Research in Dairying,
Shinfield, Nr. Reading. Nov. 21.

[1] Nutting, P. G., *J. Amer. Soc. Testing Mats.*, **21**, 1162 (1921); *J. Franklin Inst.*, **191**, 679 (1921).
[2] Scott Blair, G. W., and Coppen, F. M. V., *Amer. J. Psychol.*, **56**, 234 (1943).
[3] Electrical Research Association, June 1944.
[4] Scott Blair, G. W., *J. Sci. Instr.*, **21**, 80 (1944) (in which a bibliography of papers on the use of Nutting's equation is included).
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[6] Maxwell, J. C., *Phil. Mag.*, **35**, 129 (1868).

Freezing Point of Artificially Induced Bovine Mammary Secretions

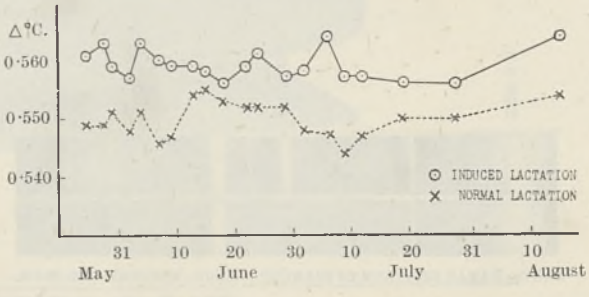
IT is well known that the freezing point depression of milk is remarkably constant for cows in normal lactation[1]. Folley and Malpress[2] have described experiments in which maiden Shorthorn heifers were treated by implantation of diethylstilbœstrol and hexœstrol, and they were good enough to supply me with samples of secretions from one animal in the former and two in the latter experiment. Three control animals from the same herd were also tested. Lactation in the three virgin heifers was initiated on May 21, 1942, following the administration of hormones on May 11, 1942. One heifer received, during the treatment, 2·40 gm. of diethylstilbœstrol, the second and third, 5·01 gm. and 2·54 gm. of hexœstrol tablets. The heifers were milked once a day until they yielded 5 lb. of milk, when they were milked twice a day. The maximum daily yield was 22 lb. from one heifer and 17 lb. and 14 lb. from the others.

For the determination of freezing-point depression, an improved model of the apparatus described by Temple[3] was used which provides results in very close agreement with the Hortvet[4] method. Following Elsdon and Stubbs'[5] suggestion, the thermometers, which were of the Hortvet type, were standardized by using three sucrose solutions. All samples were taken from individual animals and refrigerated soon after milking. Morning and evening milk was collected separately and tested within 18 hours of milking, mostly within a much shorter period. The freezing point depressions of 119 morning and 103 evening samples were determined; 59 morning and 46 evening samples being from the treated animals.

The average freezing-point depressions for each date are plotted in the accompanying graph. From this it will be seen that all the results indicate a persistently higher freezing-point depression for the secretions from the treated animals. The general mean for the maiden animals is 0·559° C., compared with the 0·550° C. for that of the other cows, and this difference is highly significant. Separate evaluation of freezing-point depressions for morning and evening samples also showed significant differences. The average values were, for normal secretions, morning 0·547° C., evening 0·553° C.; and for induced lactation, morning 0·558° C., evening 0·560° C.

For their normal cows, Aschaffenburg and Veinoglou[6] have shown that during the summer the freezing-point depression is persistently higher for evening than for morning milk, whereas in winter the reverse is true. Induced secretions were examined only during the summer, but likewise showed significantly higher freezing-point depressions for evening samples.

There were differences in the age and parity of the animals from which these samples were taken,



but Aschaffenburg and Veinoglou[6] and many others have shown that there is no correlation between these factors and the freezing-point depression of the milk.

Summarizing, it appears that, although only three treated and three control animals were studied, the persistently high freezing-point depression for the former constitutes a sufficiently unusual phenomenon to justify the hope that more extended experiments will be undertaken.

B. C. VEINOGLOU.
National Institute for Research in Dairying,
Shinfield,
Nr. Reading.
Dec. 4.

[1] Elsdon, G. D., Walker, G. H., "Richmond's Dairy Chemistry" (London: Griffin and Co., 1942).
[2] Folley, S. J., and Malpress, F. H., *J. Endocrinol.*, **4**, 1 (1944).
[3] Temple, P. L., *Analyst*, **62**, 709 (1937).
[4] Hortvet, J., *J. Indust. Eng. Chem.*, **13**, 198 (1921).
[5] Elsdon, G. D., and Stubbs, J. R., *Analyst*, **61**, 198 (1936).
[6] Aschaffenburg, R., and Veinoglou, B. C., *J. Dairy Res.*, **13**, 267 (1944).

Accuracy of Mineral Frequency Analysis of Sediments

IN view of the important paper by Rittenhouse and Bertholf[1] on the relative efficiency of gravity and centrifuge separation as applied to sedimentary analysis, it seems desirable to present the results of certain critical examinations of sedimentary technique carried out by us during the last five years. Though originally begun merely with the intention of assessing the relative merits of certain grain-counting methods (preliminary to the computation of mineral frequencies), our work has led us to a wider study of several of the sources of error likely to operate during laboratory treatment of sediments.

Basically, our method consists of applying the various 'treatments' (for example, counting methods, washing, boiling in hydrochloric acid), and certain combinations of them, to series of replicate random analyses made from thoroughly mixed ('homogenized'[2]) sediment. Statistical manipulation of the resulting data by analysis of variance in the ordinary way then permits recognition and partition of the treatment effects. Only gravity separation (in bromoform, by Milner's method[3]) has so far been employed. Throughout, the specific and varietal frequencies have been expressed as number-percentages.

We utilize two counting methods. In both, the minimum acceptable number of allogenic grains per 'light' or 'heavy' subsample count is fixed at 1,000—for obvious statistical reasons. Purely authigenic mineral species are not counted. This also applies to the micas, chlorites and similar flaky minerals having densities close to the borderline between 'heavy' and 'light' (s.g. 2·9), for their errors of separation are found to be unsystematic, unpredictable and disproportionately large. In the first method (P.S.W.), central transects (totalling 1,000 allogenic grains or more) are counted across each of the several slides ('heavy' or 'light') made from gravity separation in bulk. In the second (P.A.), '1,000-grain' subsamples representing complete separations are counted entirely. Each method has been utilized in making replicate series of number-percentage determinations (at least five per series) from certain coarse-, medium- and fine-grained sediments.

On testing the frequencies in each replicate series for homogeneity[2] with χ^2, we obtained no evidence leading us to suspect that the fluctuations of specific and varietal frequency within *the coarse-grained and medium-grained sediments* were usually due to any factor other than random sampling ($P > 0{\cdot}05$ in most cases). The sensitive additive χ^2 test (ref. 4, p. 79) showed that this was also probably true of each 'coarse' replicate series as a whole ($P > 0{\cdot}05$ in most cases). On the other hand, when applied to

Allogenic heavy mineral species	Coarse-grained*				Medium-grained**			
	Weighted arith. mean %	k_s†	χ^2	P lies between	Weighted arith. mean %	k_s†	χ^2	P lies between
Black Iron Ore + Leucoxene	53·82	1·2	6·039	0·20 and 0·10	40·99	1·5	9·039	0·10 and 0·05
Zircon	25·24	1·0	4·355	0·50 and 0·30	15·70	2·2	19·709	< 0·01
Tourmaline	7·20	1·2	5·748	0·30 and 0·20	17·69	2·6	26·885	< 0·01
Rutile	6·46	0·9	3·025	0·70 and 0·50	10·74	2·1	16·959	< 0·01
Kyanite	3·58	1·2	5·574	0·30 and 0·20	1·67	1·2	6·077	0·20 and 0·10
Staurolite	2·44	1·1	4·870	0·50 and 0·30	1·80	1·3	6·292	0·20 and 0·10
Sillimanite	0·62	0·6	1·677	0·80 and 0·70	0·31	0·9	3·379	0·50 and 0·30
Sphene					0·45	1·4	8·191	0·10 and 0·05
Monazite								
Garnet					3·55	1·5	9·464	0·10 and 0·05
Epidote	0·64††	1·1	4·438	0·50 and 0·30	3·49	3·8	56·817	< 0·01
Brookite					0·55	1·4	7·714	0·20 and 0·10
Anatase					0·34	0·7	1·809	0·80 and 0·70
Andalusite								
Topaz					0·19††	1·2	6·035	0·20 and 0·10
Glaucophane								
Corundum								
Zoisite					2·52	1·6	9·902	< 0·05
$\frac{\sqrt{2(\Sigma\chi^2)}}{-\sqrt{2\Sigma(c-1)}-1}$	= + 0·52, giving P between 0·61 and 0·60.				= + 8·87, giving $P < 10^{-9}$.			
	First counting method.				First counting method.			

* Coarse sand from Bagshot Sand (Eocene), Longlands W. pit, 1 m. E.S.E. Brading station, I.o.W.
** Medium-grained sand from base of Upper Headon Beds (Oligocene), Headon Hill, I.o.W.
† k_s = the ratio total standard error/standard error of random sampling[2].
†† Pooled, because expected numbers of grains in sub-samples were less than 5 (ref. 4, pp. 22 and 82).

Our thanks are due to Dr. J. C. P. Miller for his time-saving observation that, when k is calculated from weighted frequencies, and c is the number of analyses per replicate series, $\chi^2 = (c-1)k^2$.

the *fine-grained sediments*, the tests led us to conclude that the variations were *not* usually attributable to random sampling alone ($P < 0{\cdot}05$ for the majority of species, varieties, and replicate series). Some results for the heavy mineral suites from two of the three main sedimentary grades examined are summarized in the accompanying table. Varietal analyses have been omitted for reasons of space.

Systematic errors (investigated by analysis of variance) were found to be serious only for certain flaky minerals in the fine-grained sediments. No practical justification was obtained for the theoretical criticisms frequently levelled at the transect (and 'random field') method of counting. It is clear that, providing the specific and varietal size-differences are not excessive (a condition hydrodynamically assured for the allogenic suites of most sediments), their adverse influences upon frequency estimations obtained by the transect method are not sufficiently great to warrant consideration.

The main conclusions arising from our work may be summarized as follows: For most allogenic minerals but certain flaky species (micas, chlorites, etc.) within coarse- and medium-grained sediments, the errors of number-frequency due to normal laboratory treatment are sufficiently small to be neglected, providing that the container of the separating apparatus is large enough for adequate dispersal of the grains. For fine-grained sediments, the laboratory errors are liable to be considerable for all allogenic species. A method for estimating and manipulating the latter has been given before[2].

We are thus able to offer independent confirmation of certain of the results obtained by Rittenhouse and Bertholf.

Transect errors are mainly of theoretical interest, and will be dealt with elsewhere.

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Transmission by Insects of a Plant Virus Complex

STUDIES on the rosette disease of tobacco, first recorded by Wickens[1], have shown that the disease is a complex one consisting of two undescribed viruses for which the names 'vein-distorting' and 'mottle' viruses respectively have been suggested[2]. The vein-distorting virus cannot be transmitted mechanically but is dependent upon an aphis vector; whereas the mottle virus is easily transmitted by sap-inoculation. Investigation of the insect-relationships of these two viruses[3] has revealed some interesting facts. All attempts to transmit the mottle virus by aphis vectors have failed, *unless it is accompanied in the plant by the vein-distorting virus.*

The following description of one experiment out of many will make this clear. A tobacco plant, var. White Burley, infected with the mottle virus, was colonized with the aphis *Myzus persicæ* Sulz.; after the requisite feeding period the aphides were removed and colonized on twelve young tobacco seedlings, 5–10 aphides per plant. All twelve plants remained healthy. The mottled tobacco plant, from which these negative attempts at transmission were made, was then infected by means of aphides with the second component, the vein-distorting virus. In due course the plant developed the rosette virus complex. This plant was then again colonized with virus-free aphides and, after the requisite feeding period, these aphides were transferred to twelve tobacco seedlings, but using only one aphis per seedling as compared with 5–10 in the previous experiment. Nine out of the twelve seedlings developed the rosette disease, which of course contains the mottle virus as one of its components. Having once picked up the mottle virus together with the vein-distorting virus, the aphis retains them both for long periods and in successive transfers at twenty-four hour intervals can infect twenty consecutive plants without recourse to a fresh source of virus. Having once become infected with the virus complex the aphis, in a series of transfers, sometimes transmits the mottle virus alone, sometimes the vein-distorting virus alone and sometimes the whole complex. A somewhat similar phenomenon has been recorded in the aphis transmission of two potato viruses[4], but a clear-cut obligate relationship such as this between two viruses so unlike as the components of the rosette complex seems to be of unusual interest.

The most likely explanation of the phenomenon is a quantitative one; namely, that there is more mottle virus in a plant infected with the complex disease than in a plant containing this virus alone. But dilution tests made from the complex and from the mottle virus respectively do not appear to support this explanation.

It is possible that this phenomenon has a wider application in Nature than has been realized, and it may be discovered that other plant viruses which are apparently not insect-borne when alone can be so transmitted when in the presence of another virus.

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Selective Power in Virus Transmission Exhibited by an Aphis

THE selective power exhibited by insects in their transmission of different plant viruses is a subject of considerable interest since it is calculated to throw some light on the relationship between virus and insect. Such selective transmission has been studied by Hoggan[1], Kenneth Smith[2] and Kassanis[3], while Black[4] has shown that there are two strains of potato yellow dwarf virus, each with its specific insect vector.

So far as I am aware, however, the selection of one virus by a particular aphis species, out of a complex of two closely similar aphis- and sap-transmissible viruses, is a new phenomenon. This has been demonstrated in my experiments on the aphis-transmission of viruses affecting cruciferous plants.

The viruses in question were those of cabbage black ringspot (*Brassica Virus* 1) and cauliflower mosaic (*Brassica Virus* 3).

The aphides *Myzus persicæ* Sulz. and *Brevicoryne brassicæ* Linn., when colonized on cauliflower seedlings infected with these two viruses, transmit both, but the aphis *Myzus ornatus* Laing, similarly colonized, picks out the cauliflower mosaic virus, leaving the black ringspot virus behind.

A full account of this work is being published elsewhere.

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Fluorine in Fish Pastes

THOUGH there is much interest at the moment in the fluorine constituent of the diet, appreciation of the benefits to be obtained from certain types of food which have recently been shown to contain fluorine is by no means new. For example, in ancient Tibet the most cherished gifts received by a new mother consisted of powdered yak bone and pigs' knuckles. In India a diet of rice and fish, from which the bones were discarded, commonly resulted in calcium deficiency, which was remedied when small whole fish were included in the diet. In Deaf Smith Country, Texas, where the fluorine content of the water is 2·2–2·7 parts per million, the inhabitants are reputed to have a very low dental caries susceptibility. In a recent investigation on the Tyneside, it has been suggested that the delayed onset of caries in a section of the population may be due to the fact that the water supply to this particular area contains 1·4 parts of fluorine per million. The Eskimos of the Pacific northwest have superb teeth and their diet includes soft fish bones which contain 250 parts per million of fluorine.

Before the War, some fish-paste manufacturers obtained their supplies from fresh fish, others from canned fish; there is reason to believe that some manufacturers now use macerated whole fish in the mix, and it was therefore thought that fish pastes might contain much available fluorine. We recently requested the Chemistry Division of the Royal Aircraft Establishment to analyse five well-known brands of fish paste; the following report was received:

Type of paste	Calcium % Ca./wt.	Phosphorus % PO_4/wt.	Fluorine parts/million (wt.)
1. Bloater	0·17	0·39	5·8
2. Salmon, shrimp, etc.	0·22	0·43	8·2
3. Smoked herring ..	0·12	0·25	3·1
4. Salmon and shrimp ..	0·23	0·37	8·0
5. Salmon and shrimp..	0·26	0·34	8·9

"The method employed consisted of ashing the paste at dull red heat (600° C.), followed by extraction of the ash with dilute hydrochloric acid. Calcium was determined by precipitation as oxalate followed by back-titration with standard hydrochloric acid. Phosphorus was estimated by precipitation as magnesium ammonium phosphate which was then ignited to magnesium pyrophosphate. Fluorine was determined in accordance with the method described in the *Analyst* of August 1944, page 243: (Steam distillation of the ashed paste with perchloric acid in the presence of silicates and subsequent comparison of the distillate with a standard fluoride solution, using an Alizarin Red/Thorium lake as the colour indication)."

It has been suggested that the diet should contain 1 milligram of fluorine per day for the prevention of caries; this is provided by 2½ quarts of milk, or three grams of beef bone, or a quarter of a pound of fish paste (types 4 or 5).

We feel, then, that there are probably few people who would go to the extreme of taking daily 1⅛ jars of fish paste in the hope of avoiding the necessity of conservative dental treatment, and suggest that they seek a more suitable source of fluorine.

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Effect of Methionine upon Nitrogen Losses in the Urine following Severe Burns

IT has been known for some time that, following fractures, there are losses of nitrogen in the urine as urea[1]. Lately, in this laboratory, similar losses have been found after severe burns in animals, the loss being quite appreciable even ten days after burning[2]; these changes also occur in patients[3]. The nitrogen loss in rats can be largely eliminated by increase of protein in the diet from 14 to 22 per cent[4]; this figure including some 4 per cent of yeast protein[4], the remainder being casein. In man it has proved necessary in some patients to give up to 2,000 gm. of protein per week to stem the nitrogen loss[3].

In attempting to explain this loss, one hypothesis to be considered is that the tissue proteins are being raided by the body to provide one, or perhaps a few, amino-acids especially needed for repair of the burned area; the increased nitrogen elimination would then be due to the excretion as urea of nitrogen from the unwanted amino-acids in the raided protein molecules. We have obtained evidence in support of this hypothesis. In controlled experiments with rats, 1 per cent *dl*-methionine prevented the loss after burns as effectively as the increased protein in the experiments referred to above. This improvement was not obtained with alanine or with an *ad hoc* amino-acid mixture.

These facts, supporting the hypothesis advanced, at the same time make unnecessary the view that the nitrogen losses in burns are due to toxæmia, as they can be explained upon the basis of deficiency. Now that tannic acid has been discontinued, due to the finding that it may damage the liver in human patients[5] as well as in animals[6], there is little evidence that thermal burns *per se* cause liver damage[7]; hence it does not seem that the effect obtained with the methionine is due to its well-known action upon the fat metabolism[8]. The reason for the call for methionine is not yet clear, nor the physiological steps involved; but the experiments, the full details of which will be presented elsewhere, suggest

that methionine might benefit patients suffering from burns, at the stage where the appetite is too poor to allow of ingestion of adequate protein. An amount of 5 gm. per diem has been tolerated[9].

Our thanks are due to the Medical Research Council for grants in aid of this research and to the Ministry of Supply for synthetic methionine and other amino-acids which made the work possible; to J. Jenkins for technical assistance and to colleagues in the Department for advice. The work has been carried out under the ægis of the Burns Sub-Committee of the War Wounds Committee, Medical Research Council.

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Colour Phenomena in Ultra-Violet Vision

THE note by N. I. Pinegin[1] suggests a more detailed discussion of the relation between the threshold intensities for scotopic and photopic vision in the ultra-violet. The threshold ratio T_p/T_s is a measure of the intensity range, often misleadingly called 'photochromatic interval', in which the visual impression is free from the specific colour sensation. This intensity range is a marked function of the position of the illuminated area on the retina, the threshold ratio increasing with increasing angle from the fovea. Since Pinegin's note does not contain any data for this angle, it is difficult to draw definite conclusions from his results.

It seems, therefore, a suitable opportunity to record the results of measurements of the threshold ratio at 365 mμ for four normal observers and one observer with an aphakic eye. These measurements were made in 1938 in connexion with investigations on the photosensitivity of visual purple and scotopic vision in the ultra-violet[2]. The threshold ratio was determined using the same observers and apparatus as described in the previous paper for the measurement of the absolute scotopic sensitivity at 365 mμ. The procedure adopted was as follows:

After 5 minutes dark adaptation, monochromatic light flashes of about 1 second duration were viewed with the parafoveal region of the retina 10° temporal or nasal for right or left eye of the observer respectively. (The illuminated retinal area was a circular patch subtending an angle of 2·6° in the normal eyes and 4·4° in the aphakic eye.) The intensity of the flashes was twice slowly decreased and increased, and the relative intensities noted which corresponded to the disappearance and reappearance of the 'colourless' (scotopic) visual sensation and the colour (photopic) sensation. This procedure occupied 8–11 minutes. The mean values for the threshold ratio arrived at from the observations and shown in the table correspond, therefore, to a mean dark adaptation of 9–10·5 minutes. Little variation of T_p/T_s with dark adaptation was found after the first 5 minutes.

Observer	Age	Eye		$\log_{10}(T_p/T_s)$
G.C.	27	R		1·8
C.F.G.	34	R		2·3
R.J.L.	42	L		2·3
E.E.S.	27	L		2·1
A.G.G.	26	L	*aphakic*	I. 2·7 II. 4·7

Assuming that Pinegin's results for the absolute photopic threshold were obtained in a comparable retinal region, we get in conjunction with the results from our earlier paper for the absolute scotopic sensitivity a value $\log_{10} T_p/T_s = 2·6$ at 365 mμ, in fair agreement with the above table. A value of $\log_{10} T_p/T_s = 2·7$ at 546 mμ derived from the same sources could be compared with the measurements of Wentworth[3] which gave values of $\log_{10} T_p/T_s = 1·8$ and 2·2 at 522 and 582 mμ respectively and at a retinal position of 10° from the fovea.

The most remarkable result of our measurements is the appearance of a clearly defined second chromatic threshold in the aphakic eye. An increase of the light intensity in the region of violet sensation above the first threshold led to a point where there is a sharp transition from violet to a distinctive blue sensation. Furthermore, this transition point could be reproduced quite accurately irrespective of whether it was approached from above or below. The value of two logarithmic units derived from the table above represents, therefore, a definite quantitative measure for the intensity interval between the blue-violet point and the violet-colourless point. It seems likely that this effect is a consequence of the extremely high sensitivity of the aphakic eye in the ultra-violet, where it is found to be of the same order as in the visible part of the spectrum. The sensitivity of normal eyes was found to be about 10,000 times smaller at 365 mμ than at 546 mμ. As this difference is due to the presence in normal eyes of an absorbing substance in front of the perceptive organs of the retina rather than to a difference of the process of vision in the two spectral regions, it is to be assumed that the complex colour vision is also present in normal eyes but not clearly observable at usually available light intensities. An indication of its presence in normal eyes may be found in the widely diverging descriptions of the subjective colour of ultra-violet light, ranging from distinctive blue to distinctive violet[4], and in the frequent observation of Goodeve (private communication) that a strong mercury arc viewed through a deep violet filter which passes only the 405 mμ line, looked quite blue.

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Colour Vision of the Fovea Centralis

MR. E. N. WILLMER[1] in his communication in *Nature* of May 11, 1944, and in his recent address to the Colour Group of the Physical Society (November 22), directed attention to the defective colour vision of the fovea centralis in the normal eye. He pointed out that the colour confusions which occur there are similar to those experienced by blue-blinds (tritanopes).

It is worth recalling that fifty years ago König[2] came to a like conclusion from careful experiments on his own and other normal eyes. Describing his work, he wrote: "I have established the complete dichromatism of my fovea by setting up foveal colour matches between mixtures of 650 mμ and 475 mμ on the one hand and all intermediate spectral regions on the other. The matching field could, it is true, be held in the fovea for only a few seconds, frequently for only a fraction of a second". Despite these fixation difficulties, König was able to determine the green and red *Elementarempfindungen* for the central fovea, and to compare them with the corresponding curves for a larger matching field extending into the truly trichromatic region of the macular retina. He explained the observations in terms of his theory of the function of visual purple and visual yellow. The theory has not thrived and the experimental result just described may in consequence have attracted less attention than it deserves. Parsons[3] refers to it, however.

It may be well to emphasize that partial or complete blue-blindness of the fovea centralis can be accommodated in various visual theories (König's own, for example) and does not provide a crucial test of Mr. Willmer's.

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DR. M. H. PIRENNE'S experiment on colour vision[1] seems to be designed to test Willmer's suggestion that the rods are the end organs which are mainly responsible for the sensations of blue and violet[2], and he reaches the conclusion that "The rods are not necessary for colour vision".

Such a conclusion from this experiment (much of the substance of which is reported by Parsons and attributed to Gotch[3]) is scarcely justified. Pirenne's curve for violet light shows three 'coloured' points, two of which, those at 0·75° and at 0·34°, would, on the basis of Østerberg's data, fall on rod-containing retina. The third, if accurately focused, seems to utilize a portion of retina free from rods, but when astigmatic and other aberrations are considered, even this test area might fall partly on the rods. Thus the 10′ test field used could have stimulated the rods; and therefore if one postulates, as Willmer has done, that the rods are mainly concerned in blue and violet vision, the results given are not contradictory to this suggestion.

Objects which subtend a much smaller visual angle than 10′ would seem to be necessary if evidence is to be collected on this point. One such object is used in the following experiment, which demonstrates the blue blindness of the fovea noticed by König (ref. 3, p. 84). A hole 2 mm. in diameter was drilled in sheet metal and covered with an Ilford spectrum-violet filter which is stated to transmit only wave-lengths less than 4800 A. This was then viewed at a distance of 10 metres with a strong source of illumination behind it. By looking directly at the violet spot and so using foveal vision, the small star-like image could be made to disappear entirely; whereas on viewing the test object with the para-foveal retina the deep violet colour could be easily seen. If the Ilford spectrum-red filter was used (stated to pass only wave-lengths greater than 6200 A.), no such disappearance with foveal and re-appearance with parafoveal vision was possible at any distance. The test object subtends an angle of about 1′ at the eye in this experiment, but owing to aberrations of the optical system the area of the retina illuminated in practice would be larger than that calculated from this visual angle; it is probably not larger than the size of the rod-free area given by Østerberg.

The blue blindness of foveal vision can be explained in two ways:

1. The test object disappears because its image lies wholly within the rod-free area of the retina, and thus according to Willmer's theory on receptors which are insensitive to violet light.

2. At the fovea there is an increase in the amount of macular pigmentation sufficient to absorb the whole of the violet light.

This latter view is commonly held; but it does not agree with anatomical data, which show that the amount of yellow macular pigment present is actually less at the fovea than it is at other parts of the central area, and it may possibly be absent altogether[4]. The absence of violet receptors in this region would explain the above result quite as well.

It might be argued that if indeed there was a violet-blind foveal area, one would see a dark spot at the centre of an evenly illuminated violet field. In fact, one would not see such an area, because one would disregard it in the same way that one disregards the shadows on the retina of the retinal vessels. Hence it appears that the rods may be necessary for colour vision and may be, as Willmer postulates, the receptors most concerned with violet vision.

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[1] Pirenne, M. H., *Nature*, **154**, 741 (1944).
[2] Willmer, E. N., *Nature*, **151**, 213 and 632 (1943).
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MR. L. C. THOMSON says in his communication that my experiments[1] on colour vision in the dark-adapted eye do not prove that the rods are not necessary for colour vision, because in all cases the blue light of the test field might (according to Mr. Thomson) have stimulated rods—which in Willmer's[2] theory are supposed to be mainly responsible for blue and violet vision. It must first be pointed out that my statement "The rods are not necessary for colour vision" is not a general conclusion, as it may appear to be in the quotation given by Thomson. Its context

shows that it refers to the particular conditions of the experiments. It continues: "on the contrary, where they [the rods] are present in fair numbers, the colour of the violet test field vanishes"[1]. This fact is not explained, or even mentioned, by Thomson. It was, however, the crux of the argument.

It is by no means certain that, as suggested by Thomson, the rods were always stimulated to some extent in the experiments mentioned. As can easily be verified, the use of a 2 mm. artificial pupil reduces in a striking way the effects of aberrations of the eye in what concerns the spread of the retinal image. Since the experiments were made at threshold intensities, moreover, it is likely that only the brighter, central part of the retinal image of the field was actually able to stimulate receptors. The field, when seen, appeared as a well-defined small area. All this makes it improbable that rods were stimulated when the field was presented at an angle of 0·15°. (When the light falls in an area containing a certain number of rods, as presumably happens at an angle of 0·75°, the cone threshold may still be lower than the rod threshold if the field is small and if spatial summation in the rods takes place only to a small extent.)

Even if it were proved, however, that the rods were always stimulated in the experiments discussed here (using the dark-adapted eye), the fact remains that the presence of many rods, as observed in parafoveal vision, is highly *detrimental* to blue or violet vision. In brief, therefore, when an area containing many rods is stimulated with violet light, no colour is seen; and, in some of the cases where colour is seen, it is at least doubtful whether any rods are stimulated. These observations seem incompatible with Thomson's suggestion that the reason why (in my experiments) the light appeared coloured by foveal vision is that it spread on to parts of the retina which contain rods. (The mechanisms active under conditions other than dark adaptation are not discussed either in the original paper or here.)

The bibliography given in my paper makes it clear that there was no claim to originality in the type of experiments described. They were made largely because Wentworth[3] in her extensive investigation (which is more recent than Gotch's[4] and more like my experiments) had found in the central area of the retina an achromatic threshold which is lower than the chromatic threshold. This was not found in my experiments. Differences in the conditions used account in a general way for the different results obtained.

Thomson reports an experiment from which he seems to conclude that the rod-free area is completely insensitive to violet light, and which he gives as a demonstration of the 'blue blindness' of the fovea noticed by König[5]. What König meant by this 'blue blindness' is a certain type of dichromatism "which, however", in König's words[6], "by no means implies complete insensitivity to light of short wave-length". This is obvious, for König states[7] that he was able to match all spectral colours with mixtures of 650 mμ and 475 mμ in the fovea, which implies that the latter wave-length is able to stimulate the fovea—and that the stimulation it produces can be confined to the receptors contained in this area. It is therefore possible that, if the violet light used by Thomson was not seen by foveal vision, it was because the light was not sufficiently bright.

According to König's data, the centre of the fovea is dichromatic. According to Young's theory, it is therefore a two-receptor system. But it is not clear whether the more peripheral parts of the rod-free area, in which the cones are somewhat different anatomically from those of the "bouquet central"[8], are dichromatic or trichromatic. It seems that the discussion cannot be carried much further before this point is settled.

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[2] Willmer, E. N., *Nature*, **151**, 213 and 632 (1943); **153**, 774 (1944).
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Persistence of Vision

PERSISTENCE of vision is the basis of the cinematograph, but there is no persistence of vision when the eye is moved in ordinary circumstances. If there were, reading would be a difficult and slow process. This is explained by the double function of the ocular muscles, the decomposed photochemical products being pressed in the direction in which the eye moves, beyond the fixation point.

There is another form of persistence of vision, namely, that of positive after-images. If a strip of white paper 3 in. × ¼ in. be placed on a sheet of black cardboard, in a good light, and viewed for the shortest possible time, and the eyes then closed and covered with the hands, a clear-cut positive after-image of the paper will be seen, which will gradually fade away without becoming negative. If the eyes, being closed and the positive after-image clearly visible, are moved to the right, the whole after-image will appear to move to the right, past the fixation point. It will also bulge towards the right. These observations can be explained on the view that the photochemical stimulus in vision is in liquid form, and that the cones are stimulated indirectly by these products, and not by the direct action of light.

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Classification and Nomenclature of Animal Behaviour

MODERN work on the simpler forms of behaviour has gained much from the classification of reactions into different types. Its main achievement has been the separation of kineses from taxes. Fraenkel and Gunn have brought order into the subject with their book, "The Orientation of Animals: Kineses, Taxes and Compass Reactions" (1940). The following criticism is offered in the belief that they have provided a valuable point of departure for future work.

The authors argue (p. 22) that "When activity results from high intensity or concentration of the stimulus, it should be called *high kinesis*, and when it results from low intensity or concentration, it should be called *low kinesis*". There is a weakness

here that the authors do not deny. If a kinesis is called 'high', the obvious implication is that it is a stronger reaction than one that is 'low'. It requires a mental effort to recall that 'high' is used quite differently here, to indicate the *sense* or '*sign*' of the reaction with reference to the stimulus. The authors fear that even greater confusion would arise if high kinesis were called positive kinesis. A positive photo-kinesis, for example, can lead to aggregation in darkness, but this has been called photo-negative behaviour in the past. The argument raises a wider issue than mere nomenclature.

Besides clearing up confusion among reactions, recent work has helped to clear up another source of past confusion. It has sharpened the distinction between the two planes on which behaviour may be studied. On one plane are the reactions of single animals to single stimuli, the *units* of behaviour. On a higher plane are the spatial re-distributions and average activity changes that appear when a mass of animals is subjected to a whole system of stimuli. It follows that the laws governing the units of behaviour must underlie, but cannot be the same as, the laws governing organized systems of those units.

Fraenkel and Gunn do not make that qualitative distinction. For example, they describe aggregation in temperature gradients and average activity changes under continuously changing temperature. They show how such behaviour has been misinterpreted in the past by treating it as equivalent to simple reactions or metabolic effects. Having gone so far, they return to the analysis of reactions, as if mass behaviour comprised no more than a sum of reactions. They do not emphasize that mass behaviour has its own laws requiring separate study. Instead, they argue that a term like 'photo-negative' should be applicable to kineses, taxes and aggregations alike. This confuses different reactions; and more, it confuses phenomena on two different planes.

The following usage is suggested as more logical. Terms like 'photo-negative' should be confined to the description of taxes. This removes any justification for the awkward expression 'high kinesis'. The sense or 'sign' of reactions should be indicated uniformly, using positive and negative for both kineses and taxes. Thus, in the case of a positive kinesis, strengthening the stimulus leads to greater activity, and weakening it leads to weaker activity. In a gradient of stimulation intensity, aggregation would occur where the stimulus was *weakest*. In the case of a positive taxis, of course, aggregation occurs where the stimulus is strongest. In a negative kinesis, strengthening the stimulus leads to weaker activity, and weakening it to greater activity. In a gradient, aggregation will be where the stimulus is *strongest*. In a negative taxis, aggregation occurs where the stimulus is weakest. This usage should serve to sharpen the distinctions drawn above and so help to consolidate the gains of recent years.

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WHEN writing the book referred to by Dr. Kennedy, we were frequently faced with the alternatives of inventing new technical terms (which are often resented by the people who have to learn and use them) and of using common words in newly defined ways (which causes trouble because it is difficult to exclude from one's mind some of the common implications of the words). If Dr. Kennedy, who has studied insect activity and aggregation in the field as much as anyone, has to make an excessive mental effort with 'low' and 'high' kinesis, then the generality of zoologists must find them very troublesome indeed. The advantage of the broad use of 'photo-negative' is that it can be applied to a particular reaction before one knows which type of behaviour is responsible for aggregation in the dark. 'Positive' photo-kinesis can lead to photo-negative behaviour. If this awkwardness of signs is generally thought to be preferable to the ambiguity to which Dr. Kennedy directs attention, then let us use his modification of the system of terms.

Dr. Fraenkel and I did not write much about the plane of integrated behaviour, because we set out to deal with the elements of behaviour; apart from learning in Arthropods, reviewed by Thorpe[1], I doubt if enough is yet known to justify a treatise on invertebrate behaviour in this plane. We were, however, able to deal with some aggregations in which animals react to components of a situation other than each other.

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[1] Thorpe, W. H., *Brit. J. Psychol.*, 33, 220; 34, 20 (1943).

Moulting Fluid of Woodlice

IT is well known that in insects during the process of moulting or ecdysis there is a thin plasma or moulting fluid. "When the epidermal cells separate from the old cuticle and begin to secrete the new, the space between the two cuticles is occupied by a thin plasma. In the later stages of moulting this space is filled by an abundant fluid, the moulting or ecdysial fluid, first clearly demonstrated by Newport. There can be little doubt that much of this fluid, which extends also throughout the tracheal system, arises by exudation from the epidermal cells; indeed, this has sometimes been its sole source. But the epidermis of the majority of insects contains numerous glands which become active only at the time of moulting and certainly contribute to the secretion of the fluid."[1]

In the literature on moulting of the woodlice, I have failed to find in the writings of Schobl, Friedrich, Leichmann, Němec, Schönichen, Herold and others any reference to such a fluid in these isopods. Recently, I have had occasion to mount small pieces of the exuviæ recently cast by *Armadillidium vulgare* (Latr.), and I noticed that they seemed to adhere to the glass slide; on being removed a faint film was noticeable where each fragment had been. In longitudinal sections of *Porcellio dilatatus* Brandt, the epidermal cells show numerous glandular ones, which, I suggest, may give rise to this plasma. Moreover, in the space between the cuticle and the epiderm there is a uniform, thin streak of non-cellular matter. This was present in all the sections. Finally, exuviæ, in alcohol, show a thin glistening substance on their inner side.

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[1] Wigglesworth, "Principles of Insect Physiology" ,25 (1939).

RESEARCH ITEMS

Corpora Lutea in Elasmobranchs

THE fact that in an elasmobranch, *Myliobatus bovina*, after ovulation the follicle is transformed into a glandular organ was first reported by Giocomini in 1896. The formation and structure of this gland in another batoid, *Rhinobatus granulatus*, investigated more fully and by more modern methods, have been recorded by Miss Mary Samuel (*Proc. Indian Acad. Sci.*, **18**; 1943). This gland, a corpus luteum, is formed after the discharge of the ovum in a manner homologous with that in mammals. The luteal cells are derived from the follicular epithelium and invading cells of the theca interna, while intrusions of the theca externa accompanied by blood vessels without either the hypertrophy or luteinization of its cells provides a framework. It is noted that the fully formed corpus closely resembles that of a mammal, but it also has its own distinctive characteristics. In the mammal the corpus luteum is associated with the maintenance of pregnancy, a typical mammalian function, and its presence in an active state appears to suppress ovulation. Miss Samuel, however, records that each ovary in *Rhinobatus* may contain twenty corpora lutea in all stages of development, from newly formed to fully active glands and also maturing follicles.

Primitive Fishes

THE late Dr. Bashford Dean left an extensive and valuable collection of notes and drawings on various groups of the archaic fishes. The American Museum of Natural History very wisely decided to have this material worked up in the form of a Bashford Dean Memorial Volume, which they entrusted to the able editorship of Dr. E. W. Gudger. The last two articles of this work are just to hand, and the extent of the whole work has rendered it necessary to bind the volume in two parts. Article 7, by E. W. Gudger, is on the breeding habits, reproductive organs and external embryonic development of *Chlamydoselachus* and is illustrated by six plates and 33 text-figures. Article 8, by B. G. Smith, is on the natural history of the heterodontid sharks and the external development of *Heterodontus japonicus* and is illustrated by seven plates and 69 text-figures. The plentiful material for both these memoirs was obtained in Japan by Bashford Dean, and both contain valuable contributions to our knowledge of these rather rare, primitive sharks. The two authors and the American Museum are to be congratulated on rescuing this work from oblivion, and it is a pleasure, in these times of stress, to handle publications that are presented in such a splendid form.

Serious Fungoid Disease of British Mackerel

NORA G. SPROSTON describes the occurrence and life-history of *Ichthyosporidium Hoferi* in mackerel mainly landed at the Plymouth Laboratory (*J. Mar. Biol. Assoc.*, 26, 72; 1944). The presence of the fungus, which has been recorded from trout in Germany, from herring in the Gulf of Maine, and which probably occurs in several British marine fishes, is indicated in the mackerel by necrosis of the viscera with the production of a thick brown fluid, but there are no external signs of lesion or colour change. In the four years 1940–43, when samples were examined, the incidence of the disease continued to be high; and since it renders the fish inedible, the suggestion is made that mackerel should be split and gutted and at once packed in dry crushed ice. The opening of the fish would at once reveal badly infected individuals, which should be discarded. The main part of the paper describes the profuse forms developed by *I. Hoferi* in the mackerel —chlamydospores, conidia, hyphal bodies, branched conidiophores, clavate sporangia, hyphal fusions, spores produced by hyphal fusions—several of which are described for the first time. There is no need for an intermediate host, and infection in the sea may be spread by living individuals or from dead fish or discarded offal of diseased individuals. Within an individual an infection spreads throughout the viscera by way of the blood stream; kidney and spleen eventually breaking down and blood vessels becoming toughened by the hyphæ and nodules of the parasite. Only in very advanced cases do the body muscles degenerate.

Management of Honey-bee Colonies

CIRCULAR No. 702 of the U.S. Department of Agriculture by C. L. Farrar deals with the above subject in the northern States. It is pointed out that the trend in bee-keeping practice in recent years has been to increase the number of hives in an apiary, with less attention to individual colonies. As a consequence, most commercial apiaries are reporting average yields of about one third of those obtained from maximum-producing colonies. The most effective way to lower production costs is through increased colony yields. This Circular gives the necessary information on practices that will give the best returns from each colony. In the northern States the equivalent of not less than two ten-frame hive bodies should be used to house the colony during the winter, and not less than five during the active season. The colony to be overwintered should have a gross weight of not less than 130 lb. and consist of a laying queen along with 8–10 lb. of bees that emerged after August 20. The hive should contain not less than 40 lb. of honey in the upper body with pollen and some empty cells in the centre, and 20–30 lb. of honey and as much pollen as conditions allow in the lower body. The development of colonies inadequately provisioned with pollen can be increased by feeding pollen supplemented with 75 per cent of soy-bean flour. The bee-keeper should be familiar with the sources of pollen and nectar in his locality. During the active season the object is to build maximum populations for the honey flow and maintain them throughout the season. The most populous colonies produce not only the most honey per hive but also the most honey per bee. Brood-rearing is the basis of colony development and the maintenance of maximum populations during the flow.

Yew Scale

G. FOX WILSON has described the life-history and control of the yew scale, *Lecanium corni-crudum* (*J. Roy. Hort. Soc.*, 69, Pt. 8; Aug. 1944). The pest is at present distributed in the south of England. Attacks are mainly recognized by the presence of black honey dew on the shoots—black because of the presence of sooty mould fungi. Eggs of the pest are laid towards the end of June and hatch during the summer. Overwintering is usually accomplished as second-stage larvæ. Dispersal of the insects may be effected by birds, wasps and bluebottles, in addition to the distribution of infected plants. Control has been accomplished by spraying with a nicotine and white oil emulsion in autumn and spring.

Genetics of Cultivated Species of Cucurbitaceæ

C. F. POOLE (*J. Hered.*, 35, 122 ; 1944) summarizes the genetics of the water melon, cucumber and musk melon since 1937, when a summary was published in the Year Book of the U.S. Department of Agriculture. Upwards of twenty-five different qualitative characters and thirteen genes associated with fruit weight have been isolated in the water melon, and seventeen characters in the cucumber are now determined to be controlled by genes. Resistance to disease, sex determination and chlorosis are also partly analysed in respect of their inheritance. Of particular interest is the evidence that transmission of a mottling of leaves and fruit is by means of plastids and that an abnormal plastid may be carried by the pollen tube. Linkage relationships and gene interactions are also reported.

Barley-Rye Hybrids

R. A. Brink, D. C. Cooper and L. E. Ausherman (*J. Hered.*, 35, 67 ; 1944) report the results of some interesting experiments in wide hybridization. The hybrid embryos of *Hordeum jubatum* with *Secale cereale* cease growth at 6–13 days after fertilization if they are left on the mother plant. If the embryos are dissected and grown in artificial media, they continue growth ; 33 out of 81 such embryos developed in an undifferentiated manner, but one developed normally and was grown to maturity. These experiments confirm the view that infertility frequently arises from the non-functioning of the hybrid endosperm and not from the hybrid zygotes. The cytological examination of this unique hybrid indicates little homology between the chromosomes and the parents, but the characters of the hybrid were intermediate between those of the parents.

Statistics of Field Experiments

E. A. Cornish (*Bull.* 175), and I. E. Phipps, A. T. Pugsley, S. R. Hockley and E. A. Cornish (*Bull.* 176, *Aust. Coun. Sci. and Indus. Research*) have extended the statistical analysis of Fisher and Yates to lattice squares and cubic lattice designs, and suggest methods for use where the data are incomplete.

Synthetic Morphine Substitutes

EXPERIENCE with synthetic œstrogens has proved useful in the search for synthetic substitutes for morphine. The naturally occurring œstrogens all have the same basic ring structure ; they differ only in the position of double bonds and polar groups. So the discovery of synthetic œstrogens such as stilbœstrol which did not possess this ring structure but which nevertheless were chemically related, though somewhat distantly, to the natural œstrogens, was somewhat surprising and opened up new fields of investigation. E. C. Dodds, W. Lawson and P. C. Williams (*Proc. Roy. Soc.*, B, 132, 119 ; 1944), realizing that morphine has a phenanthrene ring structure somewhat similar to that of œstrone, have synthesized a number of analogues which bear roughly the same chemical relationship to morphine as does stilbœstrol to œstrone, and have shown that some of these analogues have morphine-like properties. The synthetic substances investigated were diphenylethylamine and seventeen other related compounds. From the clinical point of view the most promising one discovered was β-hydroxy-αβ-diphenyl ethylamine, which, in 4-hourly doses of 200–400 mgm. by mouth, relieved pain as effectively as morphine. It remains to be seen whether such substances are habit-forming.

Possibility of Aurora in a Comet's Tail

JULIE M. VINTER-HANSEN has written in the *Publications of the Astronomical Society of the Pacific* on some interesting comet observations, and a short account of the article is given in *Sky and Telescope* of June. On March 29, 1943, Arend, at Uccle, noticed a nebulous object of 13th magnitude with a nucleus, and the object was close to Comet Whipple (1942*g*). Subsequent measurements showed that its position must have coincided with that of the comet on March 28·5. Dr. Brunner-Hagger, at Zurich, had photographs of the comet on nearby dates, and these confirmed the view that the nebulous object had split off from the comet on March 28·5. On March 27·8 a fine aurora was seen, and from the time which elapsed between the appearance of the aurora and formation of the cometary object, it was deduced that the solar corpuscles, which are responsible for the auroras, travelled at the rate of one astronomical unit in about 30 hours. It is known from other evidence that this is near their speed, and the presumption that corpuscles shot off from the sun were responsible for disintegrating a portion of the comet's tail is strong. Other photographs of the comet showing similar spots were available for the period February 28–March 4, and solar eruptions had occurred during February 26–28 ; similar velocities were found for the corpuscles during this period. The view advocated may throw some light on the erratic behaviour of some comets in their sudden variations in magnitude.

Problem of U Cephei

IT has long been known that insuperable discrepancies exist between the orbit of the eclipsing binary U Cephei determined from its light curve and the orbit found spectroscopically from its velocity curve, if both curves are interpreted in the standard way. O. Struve now gives the results of a new determination of the velocity curve made with the 82-in. McDonald reflector (*Astrophys. J.*, 99, 222 ; 1944). It is confirmed that, unlike the light curve, the velocity curve is distinctly unsymmetrical, by an amount which no permissible adjustment of the elements can remove. Struve tentatively suggests that this asymmetry may be produced by absorption in a stream of gas which starts from the *B*8 star and passes along the following side of the *G*2 star, to return toward the *B*8 star along the preceding side of the *G*2 star. Superposed lines of H and Ca II which appear in the partial phases seem to need the postulation of just such a stream. The following paper (*ibid.*, p. 239) by Kopal, however, points out that several eclipsing binaries besides U Cephei have conflicting spectroscopic and photometric orbits. *Ad hoc* interpretations of each are obviously undesirable. A more general explanation is needed which will attribute the distortion of the velocity curve to some peculiar distribution and motion of matter in the outer envelopes of the stars where the density is so low that the distribution of mass remains unaffected and no dynamical perturbations are produced. So long as the matter remains in doubt, the conventional spectroscopic elements should be regarded as parameters which do not readily admit of dynamical interpretation. It is disturbing to realize that ordinary (that is, non-eclipsing) spectroscopic binaries may have their orbital elements vitiated in the same way without any indication that they are so vitiated.

ANTIBACTERIAL ACTIVITY OF AMŒBÆ

THOSE who have followed the development of research on penicillin and other substances which inhibit the multiplication of micro-organisms will be familiar with Sir Alexander Fleming's discovery (*Brit. J. Exp. Path.*, 10, 226; 1929), that staphylococci cultured on a plate culture failed to develop around an accidental contamination with the mould *Penicillium notatum* and were undergoing lysis, and that culture fluid taken from a culture of *P. notatum* would, even when it was diluted 500–800 times, completely inhibit the growth of the staphylococci. This work, together with other work on penicillin and on various antibacterial substances derived from bacteria and moulds (for example, pyocyanine, gramicidin, actinomycin, aspergillic acid, helvolic acid, etc.), has been summarized in the *British Medical Bulletin* (2, No. 1; 1944) and elsewhere. In bacterial cultures in which these antibacterial substances are present, areas in which the bacteria fail to develop appear, which are sometimes called 'clearance areas'. Protozoologists have been aware for many years that certain amœbæ ingest readily certain kinds of bacteria, yeasts and similar organisms, and that they can, under appropriate cultural conditions, produce in bacterial cultures 'clearance areas' somewhat similar to those produced by the antibacterial products of moulds and bacteria. The two phenomena are, however, quite distinct. While the amœbæ clear parts of the cultures by actively ingesting the bacteria, the antibacterial products of moulds and bacteria clear them by inhibition of the bacterial growth, or even by lysis of the bacteria.

The clearance of certain areas of bacterial cultures by the active ingestion of the bacteria by an amœba has been studied by Sir Aldo Castellani (*J. Trop. Med. and Hyg.*, 33, 160, 188, 221, 237; 1930, and 34, 83; 1931). Castellani first recorded his study of the active ingestion of the pink yeast, *Cryptococcus pararoseus* (Cast.), by an amœba which was named *Hartmanella castellanii* by M. Douglas (*J. Trop. Med. and Hyg.*, 33, 258; 1930). Castellani found that pure cultures of this amœba could be obtained by growing it on glucose-agar smeared with the dead yeast. He also found that, when amœbæ were inoculated on to a growth of certain bacteria, after a few days a zone of clearing of the culture appeared which radiated from the point of inoculation of the amœba. Such zones of clearing appeared in cultures of *B. typhosus*, *B. paratyphosus* A and B, *B. dysenteriæ* Shiga and Flexner Y, some strains of *Vibrio paracholeræ* and *B. pestis*, but not on cultures of *B. proteus*, *B. pyocyaneus*, *B. morgani*, *Brucella melitensis*, some strains of *Vibrio choleræ* and all strains of *Staphylococcus* that were tried. The amœbæ sometimes cleared bacteria (for example, *B. coli*), while at other times they did not. Douglas (*loc. cit.*) confirmed these results in the main.

C. E. van Rooyen (*J. Trop. Med. and Hyg.*, 35, 118 and 259; 1932) studied further this activity of *H. castellanii*. He found that the clearing of the yeast and bacteria by the amœba is due to ingestion of these organisms, and not to a diffusible lysin produced by the amœbæ or to a change in the *p*H of the culture; nor is the action of the amœbæ related to the action of bacteriophage. The rate of destruction of the bacterial culture is proportional to the thickness of the bacterial growth. Van Rooyen confirmed Castellani's discovery that the amœba eats certain bacteria only and will not eat others. He gives a list of about fifty micro-organisms which he subjected to the action of the amœba, most of which are important pathogenic organisms affecting man and domestic animals. The amœbæ were able to destroy, after four days of aerobic incubation at 26-30° C., *B. typhosus*, *B. coli*, *V. choleræ*, *V. paracholeræ*, *B. dysenteriæ* Shiga, *B. pullorum*, *B. suisepticus*, *B. pertussis*, *D. crassus*, *Micrococcus catarrhalis*, *Gonococcus* and *Meningococcus*. To a lesser degree they devoured *B. aertrycke*, *B. suipestifer*, *Streptococcus hæmolyticus*, *B. subtilis* and some strains of *Pneumococcus Type* II. The following were not affected by the amœba: *V. choleræ*, *B. morgani*, *B. fœcalis alkaligenes*, *B. dysenteriæ* Flexner, *Staphylococcus*, *Pneumococcus Type* III, *B. hoffmanni*, *B. xerosis*, *B. pseudotuberculosis ovis* (Preisz), *B. anthracis*, *B. mycoides*, *B. mesentericus*, *B. abortus* (Bang), *B. melitensis*, *B. tuberculosis* (human, bovine, porcine and fish strains and R and S variants of the same), *B. leprœ* (Brinkenhoff) and *B. salmonicida*.

Staphylococcus aureus was used to investigate why the amœba did not eat certain organisms. It was found that substances produced by the staphylococci inhibited the growth and multiplication of the amœbæ. Cultures of *S. aureus* killed and washed in saline were readily eaten by the amœba. The amœba also readily ate killed and washed cultures of the following other organisms which they did not eat when they were alive: *B. proteus* X 19, *B. morgani* No. 1, *B. pyocyaneus*, *B. anthracis*, *B. anthracoides*, *B. leprœ* (Brinkenhoff) and other organisms. The amœba ate mixed cultures provided that these contained either only susceptible bacteria or resistant ones rendered susceptible by killing and washing. The age of the bacterial culture did not make any difference to the ingestion of them by the amœba. The amœba is a strict aerobe and cannot multiply in anaerobic cultures. It will be noted that the bacteria which the amœba will or will not eat do not fall within the Gram-positive and Gram-negative classifications.

Filtrates of saline washings derived from bacteria which the amœbæ will not eat were unable to inhibit the growth and multiplication of the amœba; they did this only when they were combined with the living bacteria and were being produced by them. In certain circumstances the appearances produced by the amœbæ in cultures of bacteria which they will eat resemble closely those produced by bacteriophage action and may be indistinguishable from those of bacteriophage actions except by examination under the low-power microscope. The amœba is extremely resistant to emetine hydrochloride for long periods of time, and prolonged exposure (2 hr. at 75 cm. distance) to X-rays does not kill it.

It seems likely that further work with this species of amœba, and with other species also, would increas the biological interest of these results. It might throw light on the metabolism of amœbæ in general; and, when we remember that the phagocyte is an amœba actively engaged in ingesting and destroying bacteria which are causing disease, the effects of bacterial products on amœbæ in bacterial cultures are perhaps worth further study. It may be argued that studies of a free-living amœba in the presence of pathogenic and other bacteria in artificial cultures will not be applicable to the phagocyte confronted with pathogenic bacteria in the body, but this remains to be proved. At any rate, the work of Castellani

and van Rooyen provides the basis for studies of this kind. While we have, in penicillin and alllied substances, instances of the action of metabolic products of an organism on bacteria, we have, in the work here recorded, an instance of the reverse process—the action of bacterial metabolic products on an amœba physiologically equivalent to the phagocyte, which is not, fortunately for us, affected by penicillin. The work of Castellani and van Rooyen brings us, in fact, nearer to the work on the opsonins and similar substances—work which aimed at rendering the invading bacteria more palatable to the amœbic phagocyte, or at any rate aimed at helping the ingestion of these bacteria by the phagocyte. It is possible that the work done with *H. Castellanii* might be best developed with this idea as its basis.

G. LAPAGE.

AGRICULTURAL SAMPLE SURVEYS

THE need for adequate statistics relating to our agricultural resources and requirements must have become obvious to all during the last few years, for the urgency of war problems has served to direct attention to the inadequacy of peace-time data and also to the methods of rapidly filling the deficiencies. Complete and reliable censuses are often impracticable and always make great demands on both time and skilled labour. Where the need is more for a quick and reasonably accurate estimate of crop acreage, yield, or whatever it may be, sample surveys will generally offer a better method of obtaining the data. These set out to arrive at an estimate of the whole from the collection of a limited sample of representative parts. The dangers of such an approach are as clear as the advantages, and only by conducting the sample surveys along sound statistical lines can biased or distorted estimates be avoided and a measure of the reliability of the estimate be secured.

The need for information of the kind given by sample surveys is, of course, confined neither to Great Britain nor to war-time. In 1937 a statutory body called the Indian Central Jute Committee initiated, as one of its first tasks, a five-year scheme for obtaining improved estimates of the area under jute in Bengal. After some hesitation it was decided to use the sample survey method, the earlier years being devoted to small exploratory surveys, with a complete survey of about 60,000 square miles in 1941. It was laid down that the final estimate of area under jute should have a margin of error not exceeding 5 per cent, that it should be ready early in the jute season and that the cost should not be excessive. P. C. Mahalanobis, who was statistical adviser to the scheme, has now published an account of the methods, both organizational and statistical, by means of which the task was successfully accomplished. The final estimate was within 2·8 per cent of an independent official estimate based on census data; it was ready a week or so before the latest useful date; it cost only about £8,500 as against £110,000 for a complete census. In view of this the Jute Census Committee recommend the adoption of sample surveying to the Indian Government.

Mahalanobis' paper ("On Large-scale Sample Surveys", *Phil. Trans. Roy. Soc.*, B, 231, 329–451) is divided into three parts. Part 1 describes the way in which the problem arose, outlines the method of approach and discusses production and mapping surveys in addition to those concerning acreage. Part 2 is a mathematical treatment of the statistical theory of various methods of sample surveying. The concepts and principles are dealt with mainly in the abstract, but the results of model sampling experiments are also used. Part 3 concerns the application of this theory to crop area estimation, especially the jute survey of Bengal. The experimental results are summarized and numerical examples worked out.

The fields under jute in Bengal vary much in size, and furthermore any field may be only partly devoted to this crop. It was therefore decided to take as the sampling unit areas, termed grids, of a definite size, like four or twenty acres. The proportion of the land given to jute in each grid was ascertained, and by combining these proportions from all the grids, which were randomly located over the jute-growing area, an estimate of the jute acreage was obtained. Both the precision and the cost of this estimate depend on the area of each grid and the number of grids (that is, density per square mile) surveyed. Now for any given cost, the larger each grid is, the smaller is the density that can be used. The problem is then to adjust grid size, and with it density, so as to maximize the precision of the final estimate. The alternative procedure, which though discussed was not used for the jute survey, is to adjust grid size and number to minimize the cost for a given level of precision.

Two functions, relating cost and precision (variance) to grid size and number, were set up. The constants which they contained were estimated empirically from the data of the early exploratory surveys and by their aid the final survey was planned. The cost function was found to involve consideration of time necessary for enumerating the jute areas within each grid (which depends on grid size but not on density), of time necessary for journeying from grid to grid (which depends on density but not size), of miscellaneous time (independent of both size and density) and of time needed in the statistical laboratory. The precision, or variance, function was found to involve a parameter which took into account the correlation of cropping on adjacent fields. It was also shown that precision varied with proportion of land under jute in the grid, so that the adjustment of grid size and density best for one proportion would not be best for another. For this reason the area to be surveyed finally was divided into zones of more or less homogeneous proportions of jute land, and the best grid sizes and densities found for each zone separately.

Linked pairs of sub-samples, at constant distances apart but randomly orientated, were used to give the standard error of the final estimate. These were always surveyed by different groups of enumerators and at different times, so as to prevent collusion.

The laboratory methods of organizing the survey and randomizing the grids are described in detail, as are the kinds of errors arising from untrained and even dishonest enumerational labour. The means used to adjust the work to the very varied speeds of the enumerators are also mentioned. In the discussion of the planning of sample surveys it is emphasized that surveys of the kind undertaken are progressive. Each one adds to the information relating to the cost and precision functions and their changes with zone and time. So each enables a better survey to be planned for the next occasion. Finally, a detailed account of the work of others on survey sampling is appended.

IDENTIFICATION OF TIMBERS

TWO leaflets have recently been issued under the auspices of the Department of Scientific and Industrial Research by the Forest Products Research Laboratory, Princes Risborough: No. 34 on "The Identification of Timbers", and No. 37 on "Selecting Ash by Inspection" (H.M. Stationery Office, London, 1944). Leaflet No. 34 is intended to be a rough-and-ready guide to the identification of timbers. Its aim is the temporary one of taking the place of more elaborate text-books and official publications at present practically unobtainable. As is well known, owing to the demands of the War, a number of new or at least unfamiliar timbers are being used in Great Britain, and it is at times difficult for those wishing to do so to identify timbers the use of which is permitted by official specifications. General appearance, colour, texture, weight and smell, etc., are useful aids to the *cognoscenti*, but more reliable tests are based on the characteristic structural features remaining unaltered under conditions which may entirely change the external appearance of the timber. The object of the leaflet is to describe and illustrate the principal structural features of wood and to explain how they can be used in identifying timbers in the factory or office with the minimum of apparatus and little or no previous experience.

The text first describes the type of sample required and its method of preparation for examination, the sample being of a size convenient for handling. Details are then given of the structure of wood, heartwood and sapwood, softwoods (conifers) and hardwoods, structural features of softwoods and the same of the hardwoods. Two tables descriptive of the chief features of certain species are given, one for softwoods (larch, Scots pine, Douglas, western hemlock, spruce, noble fir, western red cedar and Parana pine) and the other for hardwoods (oak, sweet chestnut, ash, elm, beech, birch and sycamore). Two plates show photographs of cross-sections (magnified × 10) of some of the species mentioned above.

Leaflet No. 37 deals with ash. The importance of ash depends largely on its mechanical properties, notably its toughness, high resistance to shock and the readiness with which it can be shaped by steam and bending. These properties are subject to considerable variation, and it is of importance to users that they should be able to judge the quality of the timber whether in the standing tree or in the conversion thereof. Mechanical testing has various inherent and obvious drawbacks, and in any event involves standardized equipment and the waste of a considerable proportion of the timber. It is therefore usually necessary to fall back on visual methods of inspection and other simple tests.

The investigations dealt with in the leaflet are made on English ash (*Fraxinus excelsior*), but generally they cover other kinds of ash commonly imported into the United Kingdom, the *Fraxinus excelsior* of Continental Europe and the American ash (various species of *Fraxinus*). Shippers usually allude to American ash as 'tough ash' or 'cabinet ash'. The better grades of the former are suitable, or at any rate are used, for many of the same purposes as well-grown English ash, and are widely used in normal times by the automobile and agricultural implement industries, most of the good ash in Britain having been hunted down and felled during the War of 1914–18.

The leaflet discusses this matter of selection of ash by inspection under the headings of factors affecting the quality of the timber, structure as an indication of quality, density, selection of standing timber and winter- and summer-felled timber. As regards selection, trees with a good length of straight clean bole, clear of side-branches and with a well-developed healthy crown at least one half the height of the tree should be chosen. Such trees are likely to be found in fairly open woodland and are preferable to those grown in crowded woods. The prejudice against summer-felled timbers, the writer says, is mainly due to the fact that weather conditions in summer favour the occurrence of splits and checks owing to too rapid drying. Provided that the logs can be taken to some spot where they can be stored in sheltered conditions, there is no need to discriminate between spring- or summer-felled timber.

AMERICAN INDIAN STUDIES

BULLETIN 136 of the Bureau of American Ethnology (Smithsonian Institution) consists of a further series of six anthropological papers dealing with the American Indian and allied subjects.

Miss Frances Densmore contributes two more papers to her long series on the music of the American Indian. The first deals with the Indians of British Columbia, and is noteworthy as being her first work in Canada. Unlike most of her earlier papers, it deals with many tribes, being the product of a visit to a hopping centre where Indians from widely separated localities were at work. The music of the Choctaw Indians of Mississippi is the subject of the second paper. The investigation was prompted by the discovery that the songs of the Yuma Indians have a definite form, consisting of several periods recurring in regular order, a peculiarity which was afterwards found to occur sporadically among the Pueblos and the Seminoles, and as far away as the Tule Indians of San Blas, Panama. This characteristic, described as "period formation", was found also among the Choctaw, particularly in songs inferred to be ancient. Numerous songs were recorded.

A paper entitled "Ethnological Data concerning 100 Yucatan Plants", by Morris Steggarda, is based on a collection made by him in the area occupied by the Maya Indians, near Chichen Itza. Having eliminated 125 species, about which the available data had already been published, he prepared this interesting report on the remaining 100, so the information in it is entirely new. The body of the report consists of a catalogue, arranged in alphabetical order of scientific names, giving notes on the uses to which each is put by the Maya. This is followed by an alphabetical list of Maya names, and the paper ends with a discussion of the uses of the plants. Some are used, of course, for the production of various necessary objects and ornaments, but the greatest number are employed in the treatment of disease. It appears that the yerbateros, or herb doctors, have little scientific knowledge on which to base their treatments, which are in consequence a mixture of folklore, superstition and herbal concoctions, of which few have any curative value.

Steggarda is also responsible for "A Description of 30 Towns in Yucatan, Mexico", consisting of topographical and historical notes on a selection of the towns and villages he has visited. This should be useful to intending travellers in the area. (The first

line of the description of the village of Mani is repeated at the beginning of that of Mama, where it does not make sense.)

Julian H. Steward's paper on Western Shoshoni myths does not claim to be exhaustive, but is published because information from this tribe in Nevada and eastern California was lacking. The tales are not new for the most part, and local variation is their most interesting feature, so as many different versions of the same tale as possible were collected. The title of one version of the "Theft of the Pine Nuts" story is misprinted "The Origin of People".

The last paper, by Leslie A. White, on new material from Acoma, is a supplement to his book on the Acoma Indians, and contains miscellaneous information obtained since its publication. It includes a brief autobiographical sketch of an Indian, preceded by some particularly interesting notes on the mentality of the Pueblos, which sum up the difficulty of obtaining such material in the words "The autobiography of a Pueblo Indian is about as personal as the life story of an automobile tire"!

G. H. S. BUSHNELL.

THE STONE AGE IN SOUTH AUSTRALIA

A COLLECTION of reprints, all concerned with the stone age industries from certain parts of Australia, has been received*. The novel feature is the collaboration of Mr. H. V. V. Noone, who is an expert in the typology and technology of the older stone age industries of western Europe, and who therefore ensures that descriptive terms used there shall not be employed to describe something different in the Antipodes. This is very important, as heretofore there has always been the danger that, for example, Australian tools described as burins might not really be burins at all. There are few good collections of European stone age industries in Australia, and Australian prehistorians have had to judge solely from pictures—never a very safe proceeding. If any kind of comparative work is to be done, a proper use of sensible descriptive terms universally is a *sine qua non*. It is to be hoped, then, that the above collaborators will extend their activities to other parts of Australia as well.

Similar needs engender similar tools to deal with these needs, and it does not follow that there is any cultural connexion between distantly separated areas because more or less similar tools occur in both. The same may be said of simple industries. Where the industries in the two areas, however, are complex and contain a number of specialized tools, the situation is altered and some cultural connexion may be postulated. Hence the necessity for accurate descriptions and descriptive terms.

In studying Australian stone age collections, one notes the not infrequent occurrence of pigmy types —triangles, crescents, tiny round scrapers, and the like. As these resemble not a little types occurring in the mesolithic industries of western Europe, prehistorians have been tempted to make close comparisons. Actually great care must be taken, because the occurrence of a pigmy industry denotes nothing more than the development of the composite tool—one in which the haft is made of some suitable material such as wood and the 'business' parts of flint and suchlike substances. Such composite tools could be, and were, developed at different times in different parts of the world. Their development probably depended largely on the incoming of climatic conditions allowing of the growth of soft-wood forests. The pigmy industries of Central India would seem to date from a century B.C. to the tenth century A.D., and those from Ceylon are also not very ancient. It must not be assumed, then, that the pigmy element in the Australian stone age industries is necessarily very old. Once the idea of the composite tool had been adopted, only a few types of pigmy artefact would be suitable—a chipped circle or square would be useless. The only form typical of the western European mesolithic cultures, and not occurring in India, Ceylon or Australia, seems to be the micro-burin.

Students interested in the comparative typology and technology of stone age industries should take note of the above-mentioned excellent descriptive articles.

M. C. BURKITT.

* Campbell, T. D., and Noone, H. V. V., "South Australian Microlithic Stone Implements", *Rec. S. Aust. Mus.*, **7**, No. 3 (May 30, 1943). Campbell, T. D., and Noone, H. V. V., "Some Aboriginal Camp Sites in the Woakwine Range Region of the South-East of South Australia", *Rec. S. Aust. Mus.*, **7**, No. 4 (Nov. 30, 1943). Noone, H. V. V., "Some Aboriginal Stone Implements of Western Australia", *Rec. S. Aust. Mus.*, **7**, No. 3 (May 30, 1943). Noone, H. V. V., "Australia: Material Culture", *Mankind*, **3**, No. 5 (Dec. 1943).

EFFECT OF HORMONES ON PLANT DEVELOPMENT

SINCE it has been shown that synthetic growth substances will supply the stimulus necessary for continued ovary development and hence fruit development in a number of plants, various attempts have been made to increase 'fruit set' in both 'fruits' and vegetables by the use of hormones. These attempts have met with varying degrees of success. L. Greene (*Proc. Amer. Soc. Hort. Sci.*, **42**, 149; 1943) finds that a number of growth substances applied as sprays, lanoline pastes and injections to Starking apples failed to increase fruit set.

Similar negative results are reported by C. S. Pomeroy and W. W. Aldrich (*ibid.*, **42**, 146; 1943), using naphthyl acetic acid on orange and grape fruit, although with the marsh grape fruit used, pollen of other grape fruit varieties did increase the set of fruit; on the other hand, R. H. Roberts and B. E. Struckmeyer (*ibid.*, **44**, 417; 1944) found that aqueous solution of β-naphthoxy acetic acid and 2 : 4 dichlorophenoxy propionic acid sprayed on to tomato flowers induced fruit setting. *O*. chlorophenoxy propionic acid was less effective but both phenoxy-acids caused distortion of the foliage.

The use of the sprays did not prevent fruit or flower abscission due to virus or to nutritional deficiencies. Favourable results from the use of these sprays were also found with pumpkins, outdoor cucumbers, egg-plant and *Nicandra physaloides* but not with apple (nine varieties), greenhouse cucumbers, peppers, potatoes, or strawberries.

In field experiments of a similar nature carried out by A. E. Murneek, S. H. Wittwer and D. D. Hemphill (*ibid.*, **44**, 428; 1944) on snap beans, using naphthylacetamide and naphthoxyacetic acid applied as a spray to the plants every second or fifth day, increases in fruit yields were obtained in hot years, but decreases in cold years, emphasizing the importance of environmental conditions in determining the nature and extent of response to the treatments.

FORTHCOMING EVENTS

*(Meeting marked with an asterisk * is open to the public)*

Saturday, February 10

ROYAL PHOTOGRAPHIC SOCIETY (SCIENTIFIC AND TECHNICAL GROUP) (at 16 Princes Gate, London, S.W.7), at 3 p.m.—Mr. J. Yarwood: "The Deposition of Metal Films—their Application to Colour Photography".

Monday, February 12

FARMERS' CLUB (at the Royal Empire Society, Craven Street, Strand, London, W.C.2), at 2.30 p.m.—The Earl De La Warr: "British Agriculture and World Conditions".

ROYAL GEOGRAPHICAL SOCIETY (at Kensington Gore, London, S.W.7), at 8 p.m.—Mr. C. Hope Gill: "The People and Country of Ethiopia" (with Kodachrome Film).

Tuesday, February 13

ROYAL ANTHROPOLOGICAL INSTITUTE (at 21 Bedford Square, London, W.C.1), at 1.30 p.m.—Prof. V. Minorsky: "The Tribes of Western Persia".

CHEMICAL ENGINEERING GROUP (SOCIETY OF CHEMICAL INDUSTRY) (joint meeting with the INSTITUTION OF CHEMICAL ENGINEERS) (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 2 p.m.—Mr. J. Watson Napier: "Ammonia Synthesis from Coke Oven Gas".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Sir George Dyson: "The Origin and Development of Early Musical Forms", (1) "The Elizabethan Period".

INSTITUTION OF CIVIL ENGINEERS (MARITIME ENGINEERING DIVISION) (at Great George Street, Westminster, London, S.W.1), at 5.30 p.m.—Mr. A. L. Harvey: "Two New Quays at Tyne Dock, South Shields".

ILLUMINATING ENGINEERING SOCIETY (at the Institution of Mechanical Engineers, Storey's Gate, Westminster, London, S.W.1), at 6 p.m.—Discussion on the Report of the D.S.I.R. Committee on "The Lighting of Buildings" (Post-War Building Studies, No. 12) (to be introduced by Dr. C. C. Paterson, F.R.S.).

ROYAL PHOTOGRAPHIC SOCIETY (SCIENTIFIC AND TECHNICAL GROUP) (at 16 Princes Gate, London, S.W.7), at 6 p.m.—Mr. Stuart Williamson: "Trends in Air-Camera Design".

INSTITUTION OF ELECTRICAL ENGINEERS (LONDON STUDENTS' SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 7 p.m.—Dr. W. Wilson "The Cathode Ray Tube and its Applications".

Wednesday, February 14

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 3 p.m.—Prof. Owen Thomas Jones, F.R.S., and Prof. William John Pugh: "The Complex Intrusion of Welfield Rocks near Builth Wells, Radnorshire"; Dr. Frank Dixey: "The Relation of the Main Peneplain of Central Africa to Sediments of Lower Miocene Age".

ROYAL COLLEGE OF SURGEONS OF ENGLAND (at Lincoln's Inn Fields, London, W.C.2), at 3.30 p.m.—Prof. G. Grey Turner: "The Hunterian Museum, Yesterday and To-morrow" (Hunterian Oration).

INSTITUTION OF ELECTRICAL ENGINEERS (TRANSMISSION SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. R. C. Hatton and Dr. J. McCombe: "The Operation, Maintenance and Testing of Overhead Lines and Associated Outdoor Equipment on A.C. Systems".

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 5.30 p.m.—Wing-Commander T. R. Cave-Browne-Cave: "Camouflage for the Concealment of Civil Factories".

BRITISH ASSOCIATION OF CHEMISTS (at Caxton Hall, Westminster, London, S.W.1), at 6.30 p.m.—Prof. Harold Laski: "The Place of the Scientist in Post-War Administration".*

Thursday, February 15

CHEMICAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 2.30 p.m.—Mr. L. C. Bateman, Dr. E. D. Hughes and Prof. C. K. Ingold, F.R.S.: "Molecular Compounds between Amines and Sulphur Dioxide, a Comment on Jander's Theory of Ionic Reactions in Sulphur Dioxide"; Mr. S. H. Harper: "Experiments on the Synthesis of the Pyrethrins, Part 1, Synthesis of Chrysanthemum Monocarboxylic Acid": Mr. N. Barton, Mr. G. L. Buchanan, Prof. J. W. Cook, F.R.S., Mr. W. Graham and Mr. J. D. Loudon: "Studies on the Chemical Constitution of Colchicine".

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Prof. James Gray, F.R.S.: "Locomotory Mechanisms in Vertebrate Animals", (3) "Nervous Control of Movement".

ROYAL AERONAUTICAL SOCIETY (at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1), at 6.30 p.m.—Mr. J. Wright: "Aircraft Wheels and Brakes".

Friday, February 16

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 5 p.m.—Prof. F. Wood Jones, F.R.S.: "The Mammalian Toilet".

INSTITUTION OF ELECTRICAL ENGINEERS (MEASUREMENTS SECTION) (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. D. J. Desmond: "The Economic Utilization of Modern Permanent Magnets".

INSTITUTION OF MECHANICAL ENGINEERS (at Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Annual General Meeting. Dr. J. Lockwood Taylor: "The Variable-pitch Marine Propeller".

Saturday, February 17

INSTITUTE OF PHYSICS (LONDON AND HOME COUNTIES' BRANCH) (at the Royal Institution, Albemarle Street, London, W.1), at 2.30 p.m.—Dr. R. E. Slade: "The Organisation of Research in Industry".

Saturday, February 17—Sunday, February 18

ASSOCIATION OF SCIENTIFIC WORKERS (at Caxton Hall, Westminster, London, S.W.1)—Conference on "Science in Peace".

Saturday, February 17

At 2.15 p.m.—"Science and Production" (Chairman: Prof. P. M. S. Blackett, F.R.S.).

Sunday, February 18

At 10 a.m.—"The Future Development of Science" (Chairman: Sir Robert Watson-Watt, F.R.S.).

At 2.30 p.m.—"Science in Everyday Life" (Chairman: Prof. H. Levy).

APPOINTMENTS VACANT

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

CHIEF LECTURER IN THE MINING DEPARTMENT—The Registrar, Technical College, Sunderland (February 16).

TEACHER OF MECHANICAL ENGINEERING SUBJECTS for the Day School of Engineering and Part-time Day and Evening Classes—The Principal, Hendon Technical College, The Burroughs, Hendon, London, N.W.4 (February 17).

LECTURER (full-time) IN CHEMISTRY in the Science Department, and an ASSISTANT MASTER or MISTRESS to teach MATHEMATICS to School Certificate standard, with some Elementary Physics or Geography, in the Technical High School attached to the College—The Clerk to the Governors, South-East Essex Technical College and School of Art, Longbridge Road, Dagenham (February 17).

ENGINEERS (temporary) for the Public Works Department of the Government of Nigeria—The Ministry of Labour and National Service, Appointments Department, Central (T. and S.) Register, Room 5/17, Sardinia Street, Kingsway, London, W.C.2 (quoting Reference No. E.1212.A) (February 20).

PSYCHOLOGISTS (two or more, temporary)—The Secretary, Civil Service Commission, 6 Burlington Gardens, London, W.1 (February 21).

LECTURER IN BOTANY—The Secretary and Registrar, The University, Bristol (February 23).

ASSISTANT MASTER (full-time) to teach MECHANICAL ENGINEERING to Higher National Certificate standard and ALLIED SUBJECTS in the York Technical College—The Chief Education Officer, Education Offices, Clifford Street, York (February 24).

SENIOR LECTURER IN AERONAUTICS in the University of Sydney—Prof. A. V. Stephens, c/o Scientific Research Liaison Office, Australia House, Strand, London, W.C.2 (March 1).

ENGINEER to take charge of all engineering and associated services in large old-established works in North Midlands area—The Ministry of Labour and National Service, Appointments Department, Central (T. and S.) Register, Room 5/17, Sardinia Street, Kingsway, London, W.C.2 (quoting Reference No. C.2453.XA) (March 6).

LECTURER IN METALLURGY—The Registrar, King's College, Durham (March 10).

CHAIR OF ANATOMY in the University of Ceylon—The Secretary, Universities Bureau of the British Empire, c/o University College, Gower Street, London, W.C.1 (March 19).

PROFESSOR OF PHILOSOPHY, tenable in the Durham Division of the University—The Registrar, University, 46 North Bailey, Durham (March 31).

ASSISTANT KEEPER to be responsible for the GEOLOGICAL COLLECTIONS in the Manchester Museum—The Registrar, The University, Manchester 13 (April 24).

LABORATORY ASSISTANT (skilled) FOR PHYSIOLOGY DEPARTMENT—The Vice-Dean, St. Bartholomew's Hospital Medical College, at Queen's College, Cambridge.

SECRETARY AND TECHNICAL EDITOR—The Secretary of the Technical Section, Paper Makers' Association, Melbourne House, Aldwych, London, W.C.2.

PSYCHOLOGIST (part-time) for the Child Guidance Clinic—The School Medical Officer, Public Health Department, Stour Street, Canterbury.

RESEARCH ASSISTANT, with medical qualification, or degree in a biological subject, in the Department of Chemotherapy—The Laboratory Secretary, Liverpool School of Tropical Medicine, Pembroke Place, Liverpool 3.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

London Shellac Research Bureau. Lac Derivatives as Substitutes for Alkyd Resins. By Dr. B. S. Gidvani and N. R. Kamath. Pp. 8. (London: London Shellac Research Bureau, 1944.) [241

Ministry of Health: Nurses Salaries Committee. Mental Nurses Sub-Committee Further Recommendations. Mental Nurses S.C. Notes No. 1. Pp. 4. (London: H.M. Stationery Office, 1945.) 1*d.* net. [251

Parliamentary and Scientific Committee. Annual Report, 1944. Pp. 19. (London: Parliamentary and Scientific Committee, 1945.) [251

Catalogue

Books of All Ages. (Catalogue No. 674.) Pp. 70. (London: Francis Edwards, Ltd., 1945.)