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BIOLOGISTS IN WAR-TIME

'GUNS not butter' may have some merit as a war slogan but cannot be considered a good axiom for warfare. Without fats, with their high calorific value, there would not be the muscular energy to build more guns. In the strategical planning of modern wars the only sound precept is—guns *and* butter. Under conditions of emergency or unpreparedness it may be necessary to concentrate upon gun-production and neglect butter-making. This state of affairs existed in Great Britain before and during the early phases of the War, and the urgency of the demand for guns, tanks and aeroplanes, with all their associated equipment, laid great stress on the part the physicist, chemist, metallurgist and engineer could play in development and research.

Biologists during this first period of crisis well realized that they could not expect to be regarded as essential cogs in the war machine. They did, however, feel that as the first crisis passed, it would become evident that biologists had a contribution to make to the war effort. The Government was known to have recognized that the prolonged storage of reserves of grain and other foodstuffs is associated with special war problems concerned with mould, insect and rodent attack. These problems were already being investigated by biologists ; it seemed likely that there would be further storage problems to investigate or advise on. In other fields there were encouraging signs that biologists could do useful work. Much attention was being given to the problem of crop failures and the incidence of wire-worms in the soil. There was, therefore, the feeling that there would be more war-time agricultural problems, but there was, however, the sobering reflection that wire-worms in newly ploughed up grassland had been a problem in the War of 1914–18 and that during the intervening decades little had been done to solve it. This was emphasized by Sir John Russell at the Conference on Science and the War Effort organized by the Association of Scientific Workers (see NATURE of February 21, p. 208).

By the middle of 1940 biologists first began to realize that they could not be expected to be directed into war work of a biological nature without themselves taking active steps to inquire whether problems existed or whether their biological knowledge could be put to a useful purpose. Already there was a tendency, not only among intelligent laymen but also among other men of science, to assume that in this War there was no place for biology—apart from the medical aspects—except in a few narrow fields. This attitude was not surprising, for to the non-biologist the progress in many other sciences, compared to that in biology, appears like the race between the tortoise and the hare. After a year of war the contest seemed even more unequal, and the physical sciences had a start. From the outset engineers, physicists and chemists had been mobilized in large teams to work in such specialized fields as radio location, chemical warfare and aeroplane design. Biologists in general had not had the advantage of co-ordinated team work. They also had the further handicap that many biological developments cannot have the dramatic appeal of the technical developments which lead to the production of a new fighter or a more effective bomb. Misunderstandings, too, concerning the importance of certain branches of biological research and results have also played a serious part during this War, as emphasized by Dr. A. Walton at the Conference to which reference is made above.

Another aspect of this unequal expansion of non-biological and biological research led to a large demand, especially in radio location, for biologists with a knowledge of physics. As a result many biologists who saw no prospect of being employed in a biological capacity joined up as technicians in the Services. To other biologists, who by this time were convinced that there were biological problems to be solved, this progressive depletion of the ranks was more than disturbing. There was also the further cause for anxiety since objections were sometimes raised that biological research was essentially long-range and therefore could not be brought to a successful conclusion in war-time. In agriculture, many of the research stations had lost most of their research staff for advisory work under the county war executive committees. This policy had the advantage of capitalizing the knowledge of experts in advising farmers, but had the disadvantage that simultaneous research at a number of research stations, so essential if reliable results are to be obtained in a minimum time, was seriously handicapped for lack of facilities.

Although in agriculture this switching of biologists from research to advisory duties indicated an official recognition that research workers are capable of making constructive use of their research experience, in other spheres there seemed to be little realization that a large capital of knowledge existed among biologists which if properly employed would return a handsome dividend. Biologists were convinced that wherever biological material was used or stored in large quantities there must be biological problems connected with supply, preservation, provision of substitutes, etc. Some of the problems, it was felt, were capable of immediate solution or only required short-range research. What appeared to be lacking was the appreciation that the problems existed and the absence of any mechanism by which information could be obtained or contacts established.

This was the background in the early part of 1941 when the Association of Applied Biologists, the British Ecological Society and the Society for Experimental Biology agreed to form a joint committee to consider what was the role of biology in war-time, what parts biologists could most effectively play and how their services or biological knowledge could best be brought to bear both on the problems and on the prosecution of the War.

As announced on p. 238 of this issue, a committee —the Biology War Committee—has now been set up which is recognized by the Government and is linked to a new joint committee of the Department of Scientific and Industrial Research, the Medical Research Council and the Agricultural Research Council.

The formation of this Committee is an important step forward and we heartily congratulate the three Societies on the important step they have achieved. Under the new procedure, biologists now have a channel through which problems, suggestions or ideas can be transmitted to the joint committee of the three Government research organizations and through it to other Government departments or the Services. Similarly, biological problems encountered by these departments or requests for information can be referred back to biologists through the same channel. In this connexion it is clear that if the proper contacts are to be established and the most correct information obtained, the Biology War Committee must be aware of the workers and their research in all the main biological fields. The course that the Committee has adopted initially of co-opting members so that each field of biology is represented by an expert is, in war-time, the only possible one if the Committee is from the outset to maintain the fullest liaison with biologists.

The setting up of this organization raises a number of important questions concerning the relationship of biology—excluding the medical aspects—to the community not only in war but also in peace. That such a committee should be in being only after two and a half years of war reflects the failure of the community as a whole to appreciate the true implications of biology and the part it has to play under war-time conditions. This lack of appreciation cannot be laid at a single door, but is brought about by a variety of causes. First, the teaching of biology in schools has had until recently scant attention compared to the teaching of physics and chemistry. Secondly, in the past the curricula of school biology have had a sterile academic flavour and little stress has been laid on how biology directly affects the lives of individuals. To-day in this respect there are indications of change, but the change has come too late to affect appreciably as yet the general outlook.

Furthermore, this welcome modification of biology from that of 'pure' botany plus zoology to the more comprehensive science of man and mankind is still being opposed by powerful authorities. Too many biologists still 'pride' themselves on their ignorance of the impact of their science on society.

Many biologists in our universities still look askance at applied biology. That outlook has had its repercussions right down to our primary schools, and herein, we think, lies the reason for the prevalent view, held even in many authoritative circles, that there can be very little *direct* use for biologists in our war effort. We welcome the founding of the Biology War Committee if only for the purpose of dispelling this out-of-date view; and, perhaps the Committee will succeed in rousing other biologists to their social responsibilities. The Committee might have another effect—that of counteracting the attempts of some biologists to disturb the relations of modern science to society under the pretext of preserving the freedom of scientific research. The establishment of this Committee in no way jeopardizes scientific freedom; rather will it help in the long run to preserve it.

The discoveries and technical developments of the non-biological sciences impinge directly on the life of every citizen, and it is literally brought home to him that science is affecting his way of life. That is seen especially in the sciences of physics, chemistry and engineering, in the development of radio, plastics and the motor-car. Because of his everyday familiarity with these products of science he learns to appreciate that each is a specialized field. He fully comprehends that a motor-car mechanic is not likely to be able to repair a radio set, and generally realizes that experts who design motor-cars do not design radio gramophones. But when it comes to biology, except perhaps in the farming community, biologists, when not classed as medic l men, anthropologists or psychologists, may be distinguished as botanists and zoologists but no further. The idea is still prevalent that the sciences of botany and zoology are not like physics and chemistry, that is, widespread in their many fields, but sciences in which all branches come within the orbit of a single individual. It is not uncommon to find that a botanist's primary function is regarded as his ability to identify and classify plants, and it comes as a shock to learn that botanists may be interested in other things for which a knowledge of the physical sciences and mathematics is essential. For this prevailing view the biologists have largely themselves to blame. Is there, for example, any science other than biology which has found it necessary to form a society to deal especially with its 'applied' aspects. Yet nearly forty years ago certain more progressive biologists felt the need for such a society and thus founded the Society of Applied Biologists, which deals with those aspects of biology which ought to come within the purview of any general biological society. This Society, together with others equally as progressive and broad-minded, is now the very one to turn its attention to the value of its work to the war effort.

This misunderstanding of the scope and breadth of the biological field is in peace and war a serious handicap to the appreciation of what are biological problems or to what extent biologists can help in solving them or even preventing them arising. This misunderstanding brings in its train other misapprehensions. It is not always realized that biologists are not all equally competent to deal with every biological problem, and that for a given problem there is likely to be one kind of biologist who can solve it most effectively. It is not appreciated that most biological problems are complex and may for their solution require the united efforts of several different specialists not all of whom may be biologists. Nor is it fully understood that biologists have an important contribution to make by suggesting preventive measures.

It is to be hoped that the Biology War Committee by its activities will demonstrate how wide are the fields that the biological sciences cover and so bring home the value of biological research and study. The approach must, however, be twofold and the community educated to adopt a more biological outlook. As Sir John Graham Kerr points out in his communication in NATURE of February 21, p. 221, the training of biologists is at the present time at a discount though the lessons of biological study may find their application in the design of the fuselage of fighter aircraft or the correct size of a Government department. Biologists, in this respect, are, as we have already pointed out, themselves much to blame. They have not sufficiently brought home to themselves or to the layman that, apart from food production and dietetics, biology is concerned in such diverse things as corrosion in condenser tubes, the purification of coal gas, the preservation of structures in bombed buildings and the fouling of ships' bottoms.

Now, the foundation of the Biology War Committee, with its strong membership, will be able to change all this. We heartily endorse its being and terms of reference, and feel it is an occasion for congratulation that the Government views it with favour, since this Committee can, and must, do much, not only for our war effort, but also for biology itself. It can raise the science of biology from the slough of complacency in which it has wallowed for decades to the firm road on which it rightly belongs—that of a science not only of cultural value but also of inestimable practical value to the progress and evolution of civilization, a value on a par with that at present attached to the sciences of medicine, engineering, physics and chemistry.

PURITY AND FINE MEASUREMENT

Chemical Species (La Notion d'espèce en chimie)
By Prof. Jean Timmermans. Translated from the revised French manuscript by Prof. Ralph E. Oesper. Pp. ix+177. (London: Macmillan and Co., Ltd., 1941.) 18s. net.

PROF. TIMMERMANS of the University of Brussels and director of the International Bureau of Physico-Chemical Standards, was the author of a book entitled "La Notion d'Espèce en Chimie" which was published in Paris in 1928. This most useful little book has now been amplified to bring it up to date and very satisfactorily translated into English by Dr. Oesper with the title "Chemical Species". The book deals with these questions: What is a chemical species and how can a given physical-chemical system be defined without ambiguity? How should a substance be refined so that it accords with such definition of purity? And what precautions are necessary in the precise measurements of its constants? There is much available data in the literature, good, bad and indifferent, but it is not always easy to know which are the best values to choose. Richter, the editor of "Beilstein", in an article on how that great handbook was compiled wrote, "the principles to be followed in such critical choices have been laid down by Timmermans": they will be found in Part IV of "Chemical Species".

The book is not a treatise; it is a statement of principles, illustrated by many interesting notes. Much of it is the common knowledge of chemists, indeed their stock in trade; but it would be difficult to find in any other publication the principles which govern the preparation of a pure substance and the conditions for the determination of its constants with known accuracy collected together and set out in a reasoned way so completely. It would be well that chemists should take note, so that the data which are being continually fed into the literature might carry greater 'weight' in the final assessment of the value of any measured constant.

The author, in discussing the nature of a chemical species, points out that in a thermodynamic system, the phase rule defines the distinction between aggregates, solutions, compounds and elements ; for example, change of pressure will alter the composition of a constant boiling (azeotropic) mixture and show that it is not a compound. From the thermodynamic point of view, the mixture of *d* and *l*- camphoroximes behaves as a single component; mixture is only established by change of configuration as shown by change of rotatory power. Timmermans directs attention to the difference in the position of the maxima in the freezing-point curves and the position of the maxima in curves of specific electrical conductivities for certain alloys and therefore the difficulty of attributing such maxima to the formation of compounds. So much light has been thrown on these questions by X-ray methods and by the studies of Hume-Rothery and others, that it would seem that the characterization of chemical species by X-ray analysis might have received some mention. Such methods have played a part in determining whether the red and yellow forms of lead oxide, cadmium sulphide, etc., are merely polymorphs or whether the difference is structural, and might have been referred to in the interesting discussion in Chapter 5 on polymorphism and isomerism.

There would not be much doubt about the characterization of a crystal of sodium chloride (chlorine isotope 35) as a distinct chemical species, but polymeric mixtures like water in dynamic equilibrium are not so unequivocably definable, and the majority of 'pure' materials do not consist of identical molecules but have to be considered as an intimate tautomeric mixture of different species of molecules. The system can only be completely characterized if the values of the factors determining the equilibrium are known. Such factors, for example, may be photochemical, as in the case of anthracene and dianthracene.

Timmermans discusses in Part I how a chemical system should be defined without ambiguity, and that leads to the question in Part II, how to prepare pure materials which permit the preparation of all possible chemical systems. He praises the work of the English chemists Perkin and Young in the extreme care taken as to the purity of the substances of which the rotatory magnetic power or stoichiometric properties were being determined. The purity required depends on the nature of the constant which is being determined ; for example, a very small fraction of air in carbon dioxide will affect the critical pressure, but it might have little effect on other properties. There are many tricks in the preparation of pure organic substances ; thus the solubility of secondary butyl alcohol in water is very sensitive to presence of alcohols or ketones, so in making a pure specimen for solubility determination it is no use trying to reduce methyl ethyl ketone, for the reduction is never complete enough. Saponification of a non-volatile ester is also of little use because traces of alcohol remain from the alcoholic potash employed ; by preparing the crystallizable monoester of the dibasic acid (phthalic acid), however, the danger of any contaminant during saponification is avoided. There are tricks, but many pitfalls, too ; for example, the boiling point of benzene (b.p. 80·2°) is only changed 0·01° C. by 4 per cent of heptane (b.p. 98·4°). The use of adsorbents, drying agents, etc., the determination of constants as criteria of purity, the discussion of errors, the weighting of observations, units and the selection of standards of reference are some of the matters with which Parts II and III are concerned.

It is perhaps not generally realized how many stages are involved in the determination of temperature by means of a mercury thermometer. First there is the observation on the particular thermometer used with the determination errors of the particular observation (stem correction, etc.), then there is the comparison of the thermometer against a standardized thermometer, those are two stages. The standardized thermometer has to be compared from time to time with a thermometer used in the comparison baths at the standardizing laboratory ; these would be standardized from time to time against a thermometer which is referred to the platinum resistance thermometer, which in turn has been referred to the gas scale by the elaborate investigations which have established the scale. Those are at least three further stages.

One of the most useful parts of the book is Part IV where examples will be found of determinations of physical constants and the weight to be given to such determinations. On p. 164 reference is made to the measurements of the critical point of benzene as an illustration of the weights to give to values found in the literature : the remark that the value measured by Sajotchewsky in 1879 which differs by nearly three per cent from the accepted value was 'very good', no doubt means that it was of passable accuracy at the early date when the measurements were made, but it would be a poor determination these days.

The final chapter refers to the great need for the provision of pure materials and the careful determination of their constants. Have not so many of the great advances in the past (for example, the discovery of argon) been made in this very quest ?

A. C. EGERTON.

THE LIFE AND WORK OF WILLIAM HALLOCK PARK

The Man Who Lived for To-morrow

A Biography of William Hallock Park, M.D. By Wade W. Oliver. Pp. 507. (New York : E. P. Dutton and Co., Inc., 1941.) 3.75 dollars.

ROBERT BRIDGES in a letter to Mrs. Henry Bradley, commented in these terms on his memoir of her late husband, the philologist, and one who had been for long his close friend : "It is a first essential in such literary portraits to write something which will engage the reader's attention." When judged by this test, Dr. Wade Oliver's book is a notable success, for it gives the reader a firm and attractive outline of the life and character of Dr. William Hallock Park and of the setting in which his work was done. Dr. Park was for many years director of the research laboratory of the Board of Health in New York City, and he held a well-earned reputation as an exponent of the first rank in the field of applied immunology. Not only so, but he also gained the respect and affection of the leaders of medical research throughout the world, as is witnessed by the generous tributes that were paid to him on the occasion, in October 1936, of the opening of the new laboratories in New York which were rightly named after him.

Park was truly a citizen of New York. Here, on December 30, 1863, he was born, and after a career at school and college which gave no promise of his mature qualities, he led a life of unremitting service in the application of microbiological knowledge to the control of epidemic diseases. He died on April 6, 1939, when he had passed his seventy-fifth year. As a medical student and also as a young graduate, he came under the influence of Dr. T. M. Prudden who, in 1885, was the first to teach medical bacteriology in New York City, and it was to him that Park owed his final choice of a vocation. For a time, however, his gropings in other directions showed him to be irresponsive to his real calling. Thus he graduated in medicine from a desire to become a medical missionary in Persia and in this way to maintain a family tradition. A post-graduate training in Vienna included courses in diseases of the nose and throat and in gynæcology. On his return to New York he was undecided whether to specialize in one or the other of these subjects, until a letter he wrote remained unanswered and in a moment of pique he resolved to practise as a throat specialist, and so became interested in the clinical features of diphtheria and the existing views on its bacteriology. Prudden, who had reached the wrong conclusion that Streptococcus and not the Klebs-Loeffler bacillus was the causal agent of the disease, offered to Park laboratory facilities to investigate the problem in his spare time, with the result that after two years work the etiological role of the diphtheria bacillus was clearly established.

Park's association with the Department of Health of New York City began in 1893 with his appointment to the modest post of inspector and diagnostician of diphtheria. His interest in the prevention and treatment of diphtheria by means of specific immunizing reagents never waned: he was indeed a pioneer in organizing the prophylactic immunization of children on a large scale. Thus, on May 15, 1933, when nearly seventy years old, he immunized, with due ceremony, the millionth New York City child against the disease; and his colleague, Bela Schick, the originator of the Schick test, performed the same service for the first child of the second million.

Park entered the stage of his life's work at an opportune and fortunate time, because then the new science of bacteriology was becoming more and more linked with the ancient study of epidemics, and was helping to free it from the baseless and often fantastic ideas of the older writers. The story which Dr. Oliver so skilfully unfolds of the epidemic history of New York is an enlightening one. In 1892 there were 241 cases of typhus fever with 45 deaths, and 10 cases of epidemic cholera with 9 deaths. In 1904 and 1905 a heavy epidemic of cerebrospinal fever and pneumonia fell upon the city; and in the latter year 3,585 cases of typhoid fever were reported in the Greater City of New York. Again, in the five months from June 1 to November 15, 1916, 8,928 cases of poliomyelitis were notified in this area. The pandemic year of 1918 accounted for 10,886 deaths from influenza and 9,722 deaths from pneumonia in the period September 15–November 16 in New York City. Park's ardour in fighting these plagues never cooled, and he published numerous papers on his investigations, either alone or jointly with his colleagues, who regarded him as an inspiring chief and who served him well.

A chapter is devoted to the problem of the contamination of milk and to Park's efforts to bring about improvements. Reference, too, is made to the discovery of the first case of a chronic typhoid carrier in the United States. Dr. George Soper, who tracked down the house epidemics for which this carrier was responsible, has contributed elsewhere a most interesting account of his inquiries ("The Curious Career of Typhoid Mary", *Bull. New York Acad. Med.*, 15, 698–712; 1939); the bacteriological work was carried out in Park's laboratory. The sinister figure of Mary Mallon, who was either wholly heartless, or utterly ignorant, or was compounded of both these defects of mind and character, is likely to remain an historic warning of the tragedies which may follow the uncontrolled activities of a potential focus of infection in the person of a typhoid carrier.

Park's success as a director of research was founded on his simple and, at times, almost naive nature, and was supported by his patience, his perseverance and the integrity of his aims; he had a constant desire to help his fellows. He possessed a quiet sense of humour and revealed firmness of mind when the need arose.

The author of this book manifests an apt and pleasing choice in the quotations with which he prefaces the chapters. The portrait of his subject is worthily drawn, and the publishers also deserve praise for their part in the production of the volume.

MAN'S ACHIEVEMENT OF FLIGHT

The Birth of Flight

An Anthology by Hartley Kemball Cook. Pp. 204+8 plates. (London: George Allen and Unwin, Ltd., 1941.) 7*s*. 6*d*. net.

ON December 17, 1903, the Wright Brothers made that flight of twelve seconds in a power-driven controlled heavier-than-air machine which was to change the history of the world. The aeroplane has, in a few flying years, altered the outlook of every man and woman in the world, whether he or she has ever flown or not, or even seen an aeroplane. When this War ends, it will alter it still more, for flying will be as much of the world's everyday experience as travelling by rail or steamer is now.

"The Birth of Flight" is a pleasant corrective to the present attitude towards flying. It begins with the mythology and ends with the accomplishment of flight. Much is from contemporary records. In 1670 we find Francesco Lana, who considered air-exhausted spheres acting as balloons, writing, "God would never allow such a machine to be successful since it would create many disturbances in the Civil and Political Government of Mankind." Its success has certainly borne out Lana's prediction, though his theology was wrong. The Royal Society, not long after its foundation, made some inquiries into the art of flying and drew from Dr. Henry Stubbe the caustic comment that "flying was an art in which they have not as much as effected the most facile part of the attempt, which is to break their necks".

In 1783 Montgolfier flew his first hot-air balloon, quickly followed by the hydrogen balloon. For the first time man left the ground and Walpole wrote, "all our views are directed to the air." In a few years France and England had become balloon-minded.

In 1785 Dr. Jefferies crossed the Channel in a balloon, causing as much sensation as Blériot did well over a century later. In the 1860's Coxwell and Glaisher reached a height of seven miles. The balloon played its part in the American Civil War, in the Franco-Prussian War, and a balloon section was formed in the British Army, out of which ultimately came the Royal Flying Corps and the Royal Air Force. Now we have the balloon barrage.

The author only briefly touches upon the coming of the aeroplane. There is, however, one quotation worth recording, from an interview given by Rudolph Martin, a German: "In a world war Germany would have to spend millions on motor airships and a similar amount on aeroplanes to transport 350,000 men in half an hour during the night from Calais to Dover. Even to-day the landing of a large German army in England is a mere matter of money." As the author comments: "there is not enough money in the world for the purpose."

The author has chosen here and there from the story of the conquest of the air, and has chosen well. This is a book for quiet reflection, for those moments when the alarm has not been sounded, for a consideration of those words of Camille Flammarion, in the nineteenth century, writing of the coming conquest of the air: "There would no longer be frontiers but only peoples, and the whole earth will be a Confederation of United States, living in might and liberty."

J. L. PRITCHARD.

A FRENCHMAN'S VIEW OF PREHISTORY

Prehistory

By Prof. A. Vayson de Pradenne. Authorized translation by Ernest F. Row. Pp. 239. (London: The Scientific Book Club, 1941.) 2*s*. 6*d*.

ORIGINALLY trained as an engineer, M. Vayson de Pradenne was a well-to-do Frenchman with a château in Vaucluse and a flat in Paris, a married man with children. Towards the beginning of the present War tragedy overwhelmed the family. With the exception of one child, all were killed in the Paris flat by fumes from a defective stove. Vayson had for long been interested in prehistory, especially in the earlier periods. He bought up nearly the whole of the vast collection of Lower Palæolithic tools so carefully got together by the late V. Commont from the gravels of the Somme in the Amiens district, and had intended, later in life, to write for it a *catalogue raisonné*. He brought to bear on the whole subject a keen and trenchant mind and was only too happy to differ from the accepted or orthodox views. His common-sense outlook on a problem was often like a breath of fresh air in a crowded room—even if to some of the inmates it may have been felt as a draught! He was accorded the titles of "Directeur à l'école des Hautes Études" and "Professeur à l'école d'Anthropologie", and he also had a finger in the pie at the Trocadero, but none of these posts involved any regular teaching or research. Indeed, Vayson always remained the amateur—with the merits and faults of his kind.

Though the reviewer, himself a 'professional', is one of those who feel strongly that the professional owes a great part of his knowledge of prehistory to the amateur—that the former marches forward to a great extent on the trail blazed by the latter—yet none the less there is usually a lack of co-ordination to be felt in the work of the amateur, a criticism from which Vayson cannot wholly escape. His book is an epitome of himself and gives an excellent general, world-wide survey of his subject extending into Neolithic times. It will not be of much use to the specialist, but for the general reader it will help to stimulate interest in a fascinating branch of study and will serve as an undetailed book of reference to those who, having already a little information, would like a summary of what is known of prehistoric times in this or that part of the world. After all, Vayson was in the inner ring of prehistorians, he knew his subject and was not merely a scissors and paste hack hired by a publisher to write a work on prehistory for the benefit of the latter's pocket!

Part 1 deals with definitions, scope, method, interpretation of data and technology; Part 2 with classifications and chronology in western Europe, and Part 3 with prehistoric material from various parts of the world. There are a number of simple and convenient tables and the illustrations are adequate. There is no bibliography. In the original French version a short, exclusively French, bibliography was appended; it is perhaps a pity that the translator, who has done his work well, did not see fit to add a list of references suitable for English readers and which would have enabled them to delve deeper into the subject had they desired to do so as a result of the stimulus of Vayson's work. Knowing the book in French, one was at first inclined to wonder whether a translation was really called for: whether the book would really fill a gap in English libraries. On the whole it was, however, probably worth while, and one is glad in any event that Vayson's name should be thus perpetuated on our bookshelves.

M. C. BURKITT.

PHYSICAL GEOGRAPHY

The Earth and its Resources

A Modern Physical Geography. By Prof. Vernor C. Finch, Prof. Glenn T. Trewartha and M. H. Shearer. Pp. x+634. (New York and London: McGraw-Hill Book Co., Inc., 1941.) 17*s*.

FROM the title of this book one might expect to find a treatment on fresh lines, with the selection and arrangement of its matter governed by the relevance of the materials to human needs. It is in fact a sound text-book on quite conventional lines. Its relation to a previous work is stated in the preface (p. vi) as "The content and organization have come largely from *Elements of Geography*, a college text by two of the authors. The third author . . . with the background of . . . teaching experience in secondary schools . . . has restated the subject matter for use at that level of instruction." It is added that the accompanying laboratory manual provides sufficient work for two semesters, with a hint that possibly it may be confined to one semester. The book could be used for the physical geography of the last year of the high school; it assumes a good elementary knowledge of the home country (United States of America).

The treatment is deductive in that general principles are simply stated, results are drawn from them and these are then illustrated by particular facts, used as examples. For instance, Chapter 11, p. 288, starts "Imagine that Fig. 186 represents a plain about 500 miles long. A glacier, centred at *A*, slowly pushes its way southward. . . ." To a reader familiar with the subject it is clear that this hypothetical case is a very simplified version of the glaciation of either North America or Europe. The deductions as to the features of glaciated land are similarly begun from diagrams of theoretical, imagined surfaces which are equally recognizable as generalizations (for example, Figs. 190, 201, 205), each of which is followed by specific examples of the feature discussed.

This kind of treatment may well give the student the impression that our knowledge of the phenomena is obtained from such hypotheses. In fact, as the authors well know, what happened was (1) the observation and record of a large number of facts, (2) comparison and classification of these, (3) the development of possible hypotheses for the generalization and 'explanation' of the facts, (4) the testing of the hypotheses by further observation and survey and, where possible, experiment; and only those hypotheses which have survived this testing are now accepted. While it is true that no text-book can set these steps out in full over the whole of its fields, it would be better to follow this order, so as to leave the student with the knowledge that a scientific hypothesis, or theory, is to be judged by the success with which it fits both the facts it was devised to 'explain' and any other relevant facts which may come to light later. Hence if a better hypothesis is devised, the less satisfactory one will be abandoned. Physical geography contains abundant material for such study; and, as a teaching method, it may well be preferred to the presentation of present-day hypotheses as if they are unquestioned truths.

The book is very well illustrated and produced. Although it is a first edition, the present reviewer has noticed no important error or misprint. The appendixes are, for the most part, useful; but Appendix D, "The Interpretation of Maps", does not add anything to what the pupil should know before reaching this stage; while the seven maps which conclude the volume are quite inadequate to illustrate the work although their presence may be regarded as suggesting that the reader does not need an atlas. C. B. FAWCETT.

THE SOCIOLOGY OF MEDICINE

Medicine and Mankind

By Arnold Sorsby. Pp. 256 + 16 plates. (London: Faber and Faber, Ltd., 1941.) 12*s*. 6*d*. net.

ADVANCES in medicine and the allied sciences have of recent years been very great; it is no exaggeration to say that the change both actual and potential in only one generation of time has been exceptional. This increase in degree and direction of knowledge cannot be wholly regarded as technical detail interesting to the practising physician in the conduct of his profession; it comes, on the contrary, directly within the concern of the intelligent man in the street, for the prevention and treatment of disease are matters which impinge immediately upon his personal life. Immunization against diphtheria may be cited as an example; it is now an established method, easy in performance and satisfactory in practice, yet it has been carried out in Great Britain on a scale which can only be described as petty compared with the extent practised in the United States. The difference is almost entirely due to the respective efficiencies of the campaigns bringing the matter to the notice of the general public.

It is not only advantageous in his private life that the ordinary man should have some knowledge of these developments, it is important from a social point of view; public opinion has to be educated to appreciate the trend and movement of work which steadily intrudes more and more into all aspects of human life. For good or ill, man has become a community animal. Civilization brings manifold consequences; it means responsibility as well as advantages. Measures for communal welfare are not brought to efficiency by being placed upon the statute book, but by the active support of an educated public.

Books, therefore, which seek to bring anyone of matriculation standard in touch with the current findings, purposes and possibilities of modern science deserve an important place. "Medicine and Mankind" is of this type. Dr. Sorsby succeeds admirably in the picture he gives of present-day medicine. He is greatly aided in this task by the adoption of a simple straightforward design which allows balance and proportion in the subject-matter.

Dr. Sorsby, opening with an account largely historical of the relations between health and disease and the theses which have governed our attitude at different stages of progress, goes on to a statement of health from the physical point of view. He then proceeds to discuss in sequence the ill-formed body, the ill-balanced body, the abused body and the assaulted body. This enables him to set out in due proportion such diverse subjects as experimental embryology, endocrinology, dietetics, including the accessory food factors, the new dye-stuff medication, allergy and bacteriology. Treatment equally has its place and is conveniently divided into chapters on individual and collective measures.

Under the heading "Social Achievements and Frustrations" Dr. Sorsby takes up various aspects of morbidity, mortality, environment, nutrition, occupation and related factors of importance; this is perhaps his best chapter. While he directs attention to the persistence of many preventable conditions, he avoids polemics on the culpable aspect, for there are, of course, environmental conditions to-day the continuation of which infers ignorance or inattention on the part of responsible public opinion.

Dr. Sorsby is obviously interested in the special problems appertaining to nature and nurture. He therefore allows himself rather more space on questions of genetics and environment, but as these subjects represent an active fringe of medicine, active in its justifiable inferences as well as its real or immediate effects, this latitude cannot be regarded as altogether out of place. He omits all reference to psychological matters. This depends apparently upon a question of space, but the omission in a sense enhances the value of the book. For it allows a restriction of the contents to purely physical aspects which certainly offer enough to think about.

"Medicine and Mankind" is a competent and careful study. It should be of use to the practising physician anxious to keep in touch with the broad lines of thinking medicine; it should also be of use to the man in the street with sufficient intelligence to appreciate his limitations. There are numerous illustrations which have been happily chosen to simplify the subject-matter and enhance its application. JOSEPH GEOGHEGAN.

THE BIOLOGY WAR COMMITTEE

DURING 1940 meetings were held at which the place of the biological sciences and the role of biologists in war-time were discussed by the British Ecological Society, the Association of Applied Biologists and the Society for Experimental Biology. As a result of these discussions each of the societies appointed a sub-committee to review the position and make further inquiries as to the nature of war problems. Early in 1941 the sub-committee of the societies agreed to form a joint committee and after several meetings formulated a number of proposals as to the best means of mobilizing both biologists and biological knowledge in the national interest.

In May, the Committee of the three societies was informed that the Agricultural Research Council jointly with the Department of Scientific and Industrial Research and the Medical Research Council had considered the proposals as to the application of biological knowledge to problems arising out of war-time conditions and the fuller use of the services of biologists during the War. They had, therefore, created a small Joint Committee to carry out "the declared policy of H.M. Government to make the fullest possible use of science to assist the Nation in its war activities" and to confer "on the further steps which might usefully be taken to carry this policy into effect so far as the biological sciences are concerned". The Joint Committee invited representatives of the Committee of the three societies, as being an organized body dealing with this matter, to meet them, and the following scheme was put before this Joint Committee.

It was proposed that, *vis-à-vis* the Joint Committee of the Government research organizations, the Committee of the three societies should be expanded into a larger committee representative of all the main aspects of biology and that this larger committee—the Biology War Committee—should co-operate with the Government Committee in the following ways:

(1) Act as a clearing-house through which problems or ideas coming from biologists can be brought to the notice of the Joint Committee of the Government and through it transmitted to the appropriate Government Councils or Departments.

(2) Provide a channel by which the Government can refer problems to biologists.

(3) Direct the attention of the Government to any results of current biological research which the Committee considers to be apposite to the war effort.

(4) Prepare reports or memoranda on any aspect of the War which is essentially biological or which has biological implications and to submit such reports to the Joint Committee of the Government research organizations.

In connexion with this scheme the Joint Committee of the Government research organizations wishes to stress that the proposed machinery is to supplement the existing contacts with biologists and in no way seeks to disturb the connexions already established. Heads of university departments, research institutes or research stations who are already in touch with the Department of Scientific and Industrial Research, the Medical Research Council or the Agricultural Research Council would, therefore, continue to communicate direct with the Council or Councils in regard to their recognized field or fields of investigation. Similarly, biologists who are undertaking specific investigations for Government Departments or the research organizations should continue to deal with the Department or Council concerned in regard to those researches in which they are already engaged. With these provisos, the Government Joint Committee is most anxious that, to avoid undesirable complications, biologists shall submit all proposals or suggestions to the Government only through the Biology War Committee.

The original Committee of the three societies, in accordance with the suggestion put forward by the Joint Committee of the Government that the representation of the biological sciences should be as wide as possible, has given careful consideration as to how the original committee should be modified with a view to adequate representation of subjects. It was felt that, while the Committee should cover through its membership the main fields of biology, medicine should not be included, and that the representation should be weighted in favour of those fields which are not highly specialized and in which workers are not already closely linked with the three Government research organizations.

Members of the Committee of the three societies considered that, since at the outset, the work, scope and future of the Committee could not be foreseen, it would be unwise to limit the membership or to draw up a fixed constitution. They were of the opinion that the number of members should not be so great as to impede, especially under war-time conditions, both its efficiency and ability to make rapid decisions, but they also thought that while primarily the representation should be concerned with the main divisions of the biological sciences, yet there should also be some liaison with biological societies. After careful consideration the original committee co-opted fifteen additional biologists, expert in particular fields of biology. Afterwards the Committee approached the biological societies on the question of further liaison. The Committee suggested that, as a preliminary working mechanism for such liaison, biologists, under the exigencies of war-time and in view of the need for the Committee to function as soon as possible, would for the time being accept the arrangement that each biological society would nominate from the existing twenty-three members of the Committee a representative of the Society who would put before the Committee any views that the Society wishes to express and also report to the Society on the activities of the Committee; but here again it must be emphasized that the representation aimed at is that of subjects and not of societies as such.

It is clear that the whole question of the constitution of the Biology War Committee and its relationship with biologists must involve considerable time and discussion with the societies. In view of the fact that a number of biological societies have already offered support to the scheme the Biology War Committee has decided that it should make this preliminary announcement of the formation of the Committee and its personnel to date. It would like again to stress that the composition and numbers are provisional, and it is envisaged that alterations or additions will be made as circumstances arise. The Committee is, so far, constituted as follows:

† * G. E. Blackman (secretary), Department of Botany, Imperial College of Science and Technology, London;

* Prof. W. B. Brierley, Department of Agricultural Botany, University of Reading;

† * Prof. P. A. Buxton (chairman), Department of Medical Entomology, London School of Hygiene;
Prof. H. G. Champion, Imperial Forestry Institute, Oxford;
Prof. A. C. Chibnall, F.R.S., Department of Biochemistry, Imperial College of Science and Technology, London;
Dr. E. M. Crowther, Chemistry Department, Rothamsted Experimental Station, Harpenden;
Dr. C. Elton, Bureau of Animal Populations, University of Oxford;
* Dr. W. P. K. Findlay, Forest Products Research Laboratory, Princes Risborough;
† * Dr. H. Godwin, Botany School, University of Cambridge;
Dr. J. Hammond, F.R.S., Animal Nutrition Research Institute, Cambridge;
Prof. A. C. Hardy, F.R.S., Department of Biology and Oceanography, University College, Hull;
Prof. J. S. Huxley, F.R.S., Zoological Society, London;
† Dr. H. Martin, Long Ashton Research Station, Bristol;
Dr. A. T. R. Mattick, National Institute for Research in Dairying, Shinfield, near Reading;
* Prof. J. W. Munro, Department of Zoology and Applied Entomology, Imperial College of Science and Technology, London;
Dr. J. Needham, F.R.S., School of Biochemistry, University of Cambridge;
Dr. C. F. Pantin, F.R.S. (vice-chairman), Department of Zoology, University of Cambridge;
* Dr. O. W. Richards, Department of Zoology and Applied Entomology, Imperial College of Science and Technology, London;
* Dr. M. A. H. Tincker, The Laboratories, Royal Horticultural Society, Wisley;
† * Dr. C. H. Waddington, Department of Zoology, University of Cambridge;
Dr. T. Wallace, Long Ashton Research Station, Bristol;
Dr. E. B. Worthington, Freshwater Biological Association's Laboratory, Wray Castle;
J. Z. Young, Department of Zoology, University of Oxford.

In conclusion, although the full Committee has only been in existence a short time, it has been very active. The Committee is inquiring into a number of suggestions and problems raised both by biologists and by the Government. These cover a surprising range of topics and many of its activities relate to confidential matters, in respect of some of which the Government has accepted the Committee's proposals.

Biologists who wish to put forward suggestions, proposals or problems under this scheme should communicate with G. E. Blackman, Hon. Secretary, Biology War Committee, Imperial College of Science and Technology, London, S.W.7, or with other members of the Committee. In this connexion it is pointed out that under the terms of reference all such suggestions are communicated to the Joint Committee of the Department of Scientific and Industrial Research, Medical Research Council and Agricultural Research Council which will, if necessary, refer them to the appropriate Government Departments for consideration. In the first instance all suggestions are couched in general terms and in an anonymous form.

* Members of the original joint committee of the Association of Applied Biologists, British Ecological Society and Society for Experimental Biology.
† Members of Executive Committee.

METEORITES AND THE AGE OF THE SOLAR SYSTEM

By W. J. ARROL, R. B. JACOBI and PROF. F. A. PANETH

BETWEEN the years 1928 and 1931 several papers on the age of iron meteorites were published, based on analyses of their helium and uranium content[1]. The last paper gave age values for twenty-two iron meteorites which were fairly evenly spread over periods ranging from 100 to 2,800 million years.

It was pointed out in this paper that the helium measurements were considered satisfactory, while the method used for the estimation of uranium still lacked in accuracy and was to be replaced by an improved one[2]. Owing to external circumstances these experiments had to be abandoned after two years, and before final results could be achieved[3]. A new attempt to increase the accuracy of the age determinations was begun in 1937 in the Chemical Laboratories of the Imperial College of Science and Technology in London, and has been continued since 1939 in those of the University of Durham. Since general conditions a while ago again enforced an interruption, we propose to describe here briefly the main alterations made in the method, and the results so far obtained.

(1) The meteorite material used for each determination was increased to 15–20 gm. (2) The large quantities of hydrogen evolved by its dissolution in hydrochloric acid were burned with oxygen in a special device developed in connexion with another problem[4]. (3) Alternatively, the meteorite was dissolved, without evolution of hydrogen, in a reagent consisting of cupric and potassium chlorides and hydrochloric acid. (4) During this process sometimes not only the helium but also the radon liberated from the meteorite was collected and measured[5]. (5) After dissolution, the radium and thorium-X contained in the meteorite were precipitated, together with barium, as sulphates, and converted into chlorides. (6) The thoron and radon evolved from these chloride solutions were measured in an ionization chamber connected with an electrometer valve[6]; the thoron immediately after preparation of the solution in a constant nitrogen stream, the radon after regeneration and transference into the chamber. (7) The α-ray emission from the thoron and radon respectively was recorded photographically[7] over periods of some twenty hours. (8) The radon measurement was repeated at least once after a few weeks time.

The combined effect of these various changes has considerably improved the accuracy of our activity measurements, as control experiments showed. If quantities of radium of the order of 1×10^{-13} gm. (corresponding to an equilibrium amount of uranium of 3×10^{-7} gm.) and of (aged) thorium salts of the order of 1×10^{-6} gm. were added to the reagents, and the radium and thorium-X precipitated and measured as described, the agreement was good within about 10 per cent. It was especially gratifying that it thus became possible for the first time to determine the thorium content of iron meteorites[8]; previously, it was not known to what extent the age values calculated on the basis of the helium and uranium contents only might be too high, but it now appears that consideration of the thorium has little influence on the result, least of all in the case of the oldest meteorites.

In the main, the age values are determined by the helium and uranium content ; "uranium" is here understood to comprise "actino-uranium" as well. This isotope, although not a member of the uranium-radium series, is known to be present in constant proportion[9], so that the radon content is a measure of it too. Owing to its comparatively short half-life period, it is responsible for most of the helium[10] in the oldest meteorites, and the somewhat indirect method of its evaluation necessarily introduces a certain degree of inaccuracy which, however, does not seem to exceed that due to the other sources of error[11].

A repetition of the helium and uranium analyses of several of the meteorites examined before 1931 showed that the old helium values are reliable ; small corrections in a few cases are without appreciable influence on the main conclusions. The old uranium values, however, were found to be in most cases too high. It is, unfortunately, impossible to give a certain explanation for this deviation ; it is not unlikely that most of the large glass vessels[12] used for the meteorite solutions in Königsberg liberated during the time of radon accumulation a sufficient amount of radon from the walls to falsify the figures. Since this assumption, however, cannot now be checked, we give it only as a tentative explanation. There is no doubt about the greater reliability of our present method of determination, into which the various improvements have been introduced ; in particular, the small bulbs now used for the accumulation of radon did not, when tested, show of themselves any measurable radon activity.

The lower radon values now obtained made us particularly anxious to convince ourselves that no activity was lost in the process of extraction of radium from the meteorite ; for this reason we not only used two different solvents (see above) but tried to compare also in several instances the radon regenerated from the radium-containing solution with the radon occluded in the solid meteorite ; the agreement was very satisfactory and does not seem to leave any uncertainty as to the genuineness of the low activities as revealed by our new measurements.

We have concentrated mainly on six iron meteorites chosen for their widely differing helium contents ; of the meteorite "Thunda" as many as seven samples were analysed, of the others only two to four. In all cases we feel confident that further investigation will not alter the result greatly, although we hope, in a later resumption of the work, to be able to narrow down the limits of error. These are greatest still in the thorium, and negligible in the helium measurements. (As an illustration of the difficulties arising from the extraordinarily low radioactivity of iron meteorites, it may be mentioned that the average uranium content of a 20-gm. sample causes in our ionization chamber only 8, the thorium content not more than 2, α-rays an hour.) It would take too long to attempt here a discussion of the probable error of the calculated new age values ; we give them in round figures to indicate that only approximate data are at present obtainable[13]. We do not believe, however, that the highest ages are wrong by more than ± 1,000 million years, and the lower ones by more than a proportional amount, namely, about 20 per cent.

There is, however, one principal objection which might be raised against all age calculations based on the helium, uranium and thorium content : the fact that beryls and a few other minerals contain more helium than can be accounted for by the disintegration of their uranium and thorium within the geological periods during which they have existed in the solid state. (A helium deficit, caused by the escape of this gas, can occur only in minerals but not in iron meteorites[8].) So long as the origin of this 'surplus helium' is not explained, one has to be sceptical about the reliability of all the age values calculated by the helium method. This point has recently been stressed by Keevil after his discovery that, in addition to the cases of beryllium and potassium minerals detected long ago by the present Lord Rayleigh, some magnetites too contain surplus helium[14]. It will be very interesting to learn whether the magnetites which abound in helium have a peculiar chemical composition, for in the case of beryls it has been shown as very probable that not the suspected element beryllium but some other element, believed to be stable, is actually disintegrating with helium production[15]. The variety in the chemical composition of beryls (which has not yet been critically related to their helium content) gives scope to this possibility. All iron meteorites, on the other hand, are very similar in composition (see below) ; this, and the fact that, unlike minerals, they do not permit of any helium or radon migration, renders it rather likely that the differences in their helium contents are due to differences in geological age, thus making the application of the helium method possible.

In spite of the present doubt as to the value to be attached to the helium method, we think it worth while here to put our results on record ; not only because of our belief that the method is admissible for such uniform and helium-tight material as iron meteorites, but also because we think it necessary to correct our old experimental figures, irrespective of the certainty of their interpretation. If age values of iron meteorites are to be mentioned at all, the results given here and not those previously found should be quoted. Moreover, the radioactivity of iron meteorites has been used in the discussion of geophysical questions; these calculations too will have to be revised in the light of our new measurements.

In Table 1 we have entered against each meteorite the class it belongs to (O = octahedrite, H = hexahedrite, f = fine, m = medium, g = coarse), its nickel,

TABLE 1.

Name of Meteorite	Class	Nickel (per cent)	Uranium (10^{-8} gm./gm.)	Thorium (10^{-8} gm./gm.)	Helium (10^{-6} c.c./gm.)	Age in millions of years calculated from	
						individual measurements	average activity
Bethany, Goamus	*Of*	8	1	4	0·15	60	75
San Martin	*H*	5·3	0·6	8	1·6	500	800
Bethany, Amalia	*Of*	8	1	4	3·0	1100	1500
Carthage	*Om*	7·7	0·5	4	25	6500	5800
Thunda	*Om*	8·5	0·8	4	28	6100	6200
Mount Ayliff	*Og*	6·6	0·4	2	40	7600	6800

TABLE 2.

Name of Meteorite	Class	Nickel (per cent)	Helium (10^{-6} c.c./gm.)	Age in 10^6 years
Cape York, Savik	*Om*	7	< 0·0002	< 0·11
,, ,, Ahnighito	*Om*	8	< 0·001	< 0·55
Bethany, Goamus	*Of*	8	0·15	75
,, Amalia (Foote)	*Of*	8	0·2	110
,, Grondorn	*Of*	8	0·2	110
,, Lion River	*Of*	8	0·2	110
Coahuila	*H*	6	0·2	110
Uwet	*H*	6	0·4	220
Braunau	*H*	5	0·5	270
Cañon Diablo	*Og*	7	1	550
Cape York, Dog	*Om*	8	1	550
,, ,, Woman	*Om*	8	1	550
Magura	*Og*	7	1	550
San Martin	*H*	5	1·6	800
São Julião de Moreira	*Ogg*	6	2	1000
Seeläsgen	*Ogg*	6	2	1000
Murnpeowie	*Og*	6	2	1000
Mount Joy	*Hb*	5	2	1000
Copiapo	*Ob*	9	2·5	1250
Bethany, Amalia (Krantz)	*Of*	8	3	1500
Santa Rosa	*Ob*	7	3	1500
Nejed	*Om*	7	4	2000
Tamarugal	*Om*	8	4	2000
Henbury	*Om*	7	5	2400
Cranbourne	*Og*	7	5	2400
Augustinovka	*Of*	8	5	2400
Toluca	*Om*	8	6	2700
Arispe	*Ogg*	7	7	3000
Mooranoppin	*Ogg*	7	7·5	3200
Narraburra Creek	*Off*	10	11	4100
Wichita County	*Og*	8	11·5	4200
San Angelo	*Om*	8	12	4400
N'Goureyma	*Ob*	9	14	4700
Cosby's Creek	*Og*	7	14	4700
Hraschina	*Om*	9	15	4800
Sacramento Mountains	*Om*	8	15	4800
Seneca Falls	*Om*	8	15	4800
Charcas	*Om*		16	5000
Silver Crown	*Og*	8	17	5200
Staunton County, Mass. III	*Om*	10	19	5400
,, V	*Om*	9	19	5400
Joe Wright Mountain	*Om*	8	19	5400
Burlington	*Om*	9	19	5400
Nelson County	*Ogg*	7	20	5500
Williamstown	*Om*	7	21	5600
Lenarto	*Om*	9	22	5700
Carthage	*Om*	8	25	5800
Thunda	*Om*	8	28	6200
Yardea	*Om*		30	6300
Mount Ayliff	*Og*	7	40	6800
Morden			40	6800

uranium, thorium and helium content, and, in the last column but one, the age calculated from the three preceding figures. The following conclusions can obviously be drawn. (1) The helium content of the six iron meteorites studied varies greatly. (2) The uranium and thorium contents of these meteorites are confined within narrow limits; considering the still somewhat unsatisfactory state of our activity measurements we are not sure whether these variations exceed those found occasionally in samples of one and the same meteorite. (3) The remarkable uniformity in the uranium and thorium content is in good agreement with the great similarity displayed by all iron meteorites in their main chemical composition as indicated by the percentage of nickel in the above table, the rest being practically all iron. It may be added that it has been shown that all of them contain about 0·6 per cent cobalt, and traces of copper, chromium, carbon, sulphur, phosphorus, and of most of the other elements for which the methods of detection are sensitive enough; the quantities in which these elements are represented[16] scarcely vary from one meteorite to another to an extent exceeding the possible limits of analytical error. (4) The ages calculated for the three meteorites with the highest helium content amount to 6,000 and 7,000 million years.

This last result is of special interest if compared with current views on the age of the earth, the solar system, or the universe. A few years ago there seemed to be reasons to believe that the highest age found by radioactive methods for rocks and minerals in the earth's crust, about 2,000 million years, coincided with the age found by entirely different methods for the solar system or the universe[17]—a rather surprising result. The authors of a more recent survey[18] come to the conclusion that various astronomical lines of evidence make it probable that the universe must be less than 10^{10} years old, without, however, giving a lower limit. If our highest figures for the age of meteorites are considered together with the evidence implying that meteorites have since their formation been members of the solar system[19], we are led to the conclusion that the age of the solar system cannot be less than 7,000 million years, that is, considerably more than the age of the oldest rocks in the earth's crust. This figure would definitely exclude an age of only 2,000 million years which a bold extrapolation from the data of the 'expanding universe' seems to suggest.

It might be asked whether there is much chance that a search among other iron meteorites would discover some with greater quantities of helium, indicating still higher ages. It may, therefore, be useful to give here a list, which includes the values published in 1931 and later measurements, of all the iron meteorites so far examined for helium. It appears that among fifty there is none exceeding the helium content of the meteorite "Mt. Ayliff" included in Table 1. Altogether there are about 450 iron meteorites in museums; but the probability of finding one with much more helium does not seem to be great, considering that, beyond a helium content of about 10×10^{-6} c.c./gm., the number of specimens within constant helium ranges decreases rapidly. Besides, even a helium content of twice the highest value, namely, 80×10^{-6} c.c./gm., the uranium and thorium content being the same, would raise the age of this hypothetical meteorite to no more than 8,500 million years.

Finally, the similarity of the uranium and thorium content in the six meteorites so far fully analysed justifies an attempt to calculate their ages on the basis of an average value for both these elements. This has been done in the last column of Table 1, assuming the uranium content to be $0{\cdot}7 \times 10^{-8}$ gm./gm. and the thorium content $4{\cdot}0 \times 10^{-8}$ gm./gm. A comparison with the age values calculated on the basis of individual measurements, as given in the last column but one, shows that the figures are not greatly altered. Since it is unlikely that in the near future we shall have an opportunity of measuring the uranium and thorium of the forty-four other meteorites, for the sake of a preliminary rough forecast we have calculated their ages (last column of Table 2), on the assumption that the average activity found for the six iron meteorites in Table 1 is valid for the others as well.

It is not without interest to see how the probable age values are distributed among these fifty meteorites[19], but it should not be forgotten that this column is calculated on the simplifying assumption of constancy of the uranium and thorium content; the last column but one of Table 2, showing the helium content, is, however, based on measurements and, quite apart from the question of age, is worthy of attention. It shows, for example, that some meteorites usually comprised under the same name are clearly distinct. Thus the "Bethany, Goamus" and "Bethany, Amalia" irons, which were recently described as part of the "Gibeon shower"[20], can scarcely all belong to the same fall, since a fine piece of the latter possesses a much higher helium content; and of the four famous "Cape York" meteorites, "Ahnighito" and "Savik" obviously form one group, while "Woman" and "Dog" belong to another—a conclusion not inconsistent with the localities where they have been found[21]. The absence of any detectable helium in iron meteorites of normal chemical composition like "Ahnighito" and "Savik"[22] entitles us to add iron and a few other elements to the list of those[15] which cannot be responsible in beryls for the occurrence of 'surplus helium'.

We hope to discuss these and other minor points when publishing our method and results in greater detail. Our thanks are due to Mr. D. B. Janisch and Dr. E. Glückauf for valuable help in the experiments.

[1] Paneth, F. A., in collaboration with Gehlen, H., Günther, P. L., Urry, Wm. D., and Koeck, W., *Z. Elektrochem.*, **34**, 645 (1928), **36**, 727 (1930); NATURE, **125**, 490 (1930); *Naturwiss.*, **19**, 164 (1931); *Z. physikal. Chem.*, A, **152**, 110, 127 (1931); *ibid.*, Bodenstein-Bd. 145 (1931).
[2] Loc. cit., pp. 158–59.
[3] See Urry, Wm. D., *J. Chem. Phys.*, **4**, 40 (1936).
[4] Glückauf, E., and Paneth, F. A., *Proc. Roy. Soc.*, A, **165**, 229 (1938).
[5] cf. Keevil, N. B., *Amer. J. Sci.*, **236**, 304 (1938).
[6] Du Bridge, L. A., and Brown, H., *Rev. Sci. Instru.*, **4**, 532 (1933); Penick, D. B., *ibid.*, **6**, 115 (1935).
[7] Urry, Wm. D., *J. Chem. Phys.*, **4**, 34 (1936).
[8] Paneth, F. A., *Occas. Not. Roy. Astro. Soc.*, No. 5, 57 (1939).
[9] Nier, O., *Phys. Rev.*, **55**, 150 (1939); Evans, R. D., Hastings, Jane L., and Schumb, W. C., *Field Museum Geolog. Series*, **7**, 71 (1939).
[10] Keevil, N. B., *Amer. J. Sci.*, **237**, 195 (1939); Wickman, F. E., *Sveriges Geologiska Undersökning*, Ser. C, No. 427 (Stockholm, 1939).
[11] Nier, O., *Phys. Rev.*, **55**, 153 (1939).
[12] *Z. phys. Chem.*, A, **152**, 130 (1931).
[13] The individual measurements from which the mean values in Table 1 were calculated are given in the Ph.D. thesis of R. B. Jacobi (London, 1942).
[14] Keevil, N. B., NATURE, **148**, 445 (1941).
[15] Fay, J. W. J., Glückauf, E., and Paneth, F. A., *Proc. Roy. Soc.*, A, **165**, 238 (1938).
[16] Farrington, O. C., *Field Museum Geolog. Series*, **3**, 59 (1907); for rare metals in iron meteorites see Goldschmidt, V. M., *Nach. Gesell. Wiss. Göttingen*, Fachgruppe III, 377 (1932).
[17] See Holmes, A., "The Age of the Earth" (London, 1937).
[18] Bok, Bart J., and Watson, Fletcher G., Report of Committee on Measurement of Geologic Time, 92 (Nat. Research Council, Washington, 1940).
[19] Paneth, F. A., "The Origin of Meteorites" (Halley Lecture, Oxford, 1940).
[20] Spencer, L. J., *Min. Mag.*, **26**, 19 (1941).
[21] Bøggild, O. B., *Saert. Meddel. Grønland*, **74**, 11 (1927).
[22] Goldschmidt, V. M., *Naturwiss.*, **18**, 999 (1930).

HYPOTHESES NON FINGO

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I THINK it was H. E. Armstrong who once remarked, "we are told that the electron does everything but *how* it does anything we are not informed". The criticism is that the word 'electron' is the name of a thing endowed with all the potential properties it is supposed to explain. Much the same criticism was current at one time about the ether, but the electron has perhaps attained more the status of an induction from a wide range of phenomena, while the ether could never have been ranked as more than a hypothetical entity. All the same, there are obvious difficulties in the way of regarding the electron in the light of a scientific realism: its discontinuous and continuous properties seem out of all possible relation with each other; presumably they defeat our mental imagery because of the incommensurability of perceptual space and time—for this seems to be the crux of the distinction between geometrical and causal laws. One is occasionally struck with a suspicion that there are epistemological implications in modern scientific methods. The division of scientific writers into those who adopt a positivist attitude and those who have reacted towards realism marks, maybe, a difference in the broad conceptions of methodology. There are some writers who, as Burtt has remarked, have inverted the relation and have made a metaphysic out of a method. It is not the aim of this short article to attempt to disentangle the philosophical problems involved in the writings of those who have summarized the modern scientific interpretations of phenomena. This has already been done—at least to some extent—by acknowledged leaders in the fields of philosophy and logic. The burden of this article is that there has been, at least from the time of Huygens and Newton, an ultimate divergence in the conceptions of scientific method and in the philosophy tacitly assumed by men of science.

The best setting in which to view Newton's great achievement is against the background of the great speculative systems his work demolished. Newton held that science must keep close to observed facts at all times, must set itself the task of discovering quantitative laws which might be extended by induction. His well-known polemics against hypotheses expressed his distaste for ideas not derived from empirical study of phenomena. Thus the First Book of his "Optics" begins: "My design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by reason and experiments. . . ." His attitude was made clearer in a letter to Oldenburg written in July 1672. Here he said ". . . the theory which I propounded, was evinced to me, not by inferring, *it is thus, because it is not otherwise*; . . . but by deriving it from experiments concluding positively and directly. The way therefore to examine it, is, by considering whether the experiments, which I propound, do prove those parts of the theory to which they are applied; or by prosecuting other experiments which the theory may suggest for its examination".

This rebuke to critics like Hooke and Huygens reminds one of his famous general scholium at the end of the "Principia". Here Newton wrote:

"Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses, *hypotheses non fingo*; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena and afterwards rendered general by induction."

It is clear that it was not the mere multiplication of explanatory principles that Newton opposed. The tendency to reduce these to a minimum is found in other writers. Newtonian physics was based on the concepts of matter and force. The atomists went further and attempted to substitute a process of atomic collision for the operation of forces. As Duhem remarked: "The Principia of Newton had scarcely seen the light of day before it aroused the sarcasms of the atomistic clan grouped around Huygens."[1] In their turn the atomists were frequently assailed by the Cartesians. Papin, a more or less orthodox Cartesian, criticized Huygens for supposing that hardness or impenetrability was the essence of bodies, and to this Huygens replied bluntly that the Cartesian doctrine of extension was absurd. Newton perceived that scientific theories could gain no general acceptance so long as they were made dependent on a particular metaphysic: the opposing schools of thought each considered that the others retained occult properties. Burtt considers that Newton regarded an exact mathematical formulation of natural processes as the most that one can achieve[2]. Newton recognized also that there is a considerable problem in the validity of induction and that there is no absolute certainty in the conclusions of physical science.

Huygens discussed this last point at the beginning of his "Traité de la Lumière". Contrasting classical geometry with seventeenth-century physics, he remarked that "whereas the Geometers prove their Propositions by fixed and incontestable Principles, here the Principles are verified by the conclusions to be drawn from them; the nature of these things not allowing of this being done otherwise. It is always possible to attain thereby to a degree of probability which very often is scarcely less than complete proof." Writing to Tschirnhaus in 1687, Huygens explained that in physical research it is impossible to start "except by starting from experiments . . . then by conceiving certain hypotheses upon which experiments must be made. . . ." One is reminded of Whitehead's remark that ". . . it is the establishment of the procedure of taking the consequences seriously that marks the real discovery of a theory". Both these great men of science followed their own methods incompletely. Newton was not empirical about space and time, and Huygens followed the procedure of taking the consequences seriously only up to the point at which his elegant geometrical constructions were threatened. For him, as for Kepler, there was another important bias: the inclination to believe that mathematical elegance is in some way an index to reality.

Newton's concept of gravity may rightly be regarded as an abstraction and an induction from the accumulated work of Kepler, Galilei and others. Duhem[3] has pointed out that the idea of universal gravitation has an interesting history which goes back to the time of Aristotle and includes reference to da Vinci, Copernicus, Mersenne, Galilei, Gilbert, Kepler, Francis Bacon, Roberval and Borelli. The idea attained the status of a true induction only with Newton, just as that of the atom became an induction with the work of Dalton.

What we call a theory may not be possible of attainment through purely inductive processes. Newton's method of rendering laws general by induction is surely possible only if we employ some general principle of correlation: "If an induction is worth making it may be wrong." Recognizing this, men of science have for a long time been prepared to make pretty shaky inductions; Clerk Maxwell admitted as much in his paper on "The Dynamical Evidence of the Molecular Constitution of Bodies". An attitude of provisional belief is in accordance with Huygens's methodology—and the readiness for the purely provisional in the structure of science is one of the more startling anomalies in modern civilization which in all else demands an impossible finality.

The curious feature of Newton's position is that he would not explicitly allow even guiding hypotheses in more than a vague way. But his work shows that his real objection was to hypothetical *entities* rather than to hypotheses essential to the design of an experiment. His concentration on mathematical *relations* and on that *quantity* which, in Kepler's words, is "prior to the other categories", were in the strongest contrast with the metaphysical and deductive system of Descartes. The space and time of Newton's system were not identical with the space and time of ordinary sense experience: they were abstractions. Huygens was unable to understand Newton's more positivist attitude here, and the realist nature of his own conceptions makes him a convenient starting-point from which to derive the second main stream of scientific thought. Huygens might have accepted the term 'correlate' as equivalent to 'hypothesis' but, in the manner of all atomists of that time, his concepts assumed a garb from perceptual phenomena. For him the atom was a potential phenomenon, as it still is for some modern men of science.

The questions we seem to face then are these: What scientific concepts now employed are to be regarded as inductions from phemomena and not mere hypotheses? Is a scientific realism only to be founded by ignoring Newton's misgivings about hypothetical entities? Are there not to-day the same methodological objections to a process of world-building as applied in the seventeenth century to the Cartesian vortices, namely, its remoteness from empiricism and its employment of fictitious entities? It appears to be impossible to lay down hard and fast rules for the distinction of those concepts which satisfy Newton's demands and those which are purely hypothetical. The history of science suggests that in fact the former often develops out of the latter. Newton's general law of universal gravitation developed out of a particular hypothesis that heavy bodies tend to move towards the centre of the earth. Dalton's atoms were essentially the same as those of the seventeenth-century atomists, but Dalton avoided the error of Gassendi, who multiplied his hypothetical atoms so as to explain the perceptions of all the senses by their means. The atomic theory of Dalton completed a process of induction whereby science gained a principle of quantitative correlation. In the same way, the electron entered modern science. Pure abstractions such as the Newtonian mass, space, time and force appear to be insufficient in themselves, and the scientific interpretation of phenomena not capable of treatment by classical mechanics has proceeded with the use of entities which must possess the qualities of hypotheses. This method Huygens, if not Newton, would have supported.

Science is fortunately an activity in which no one need consider whether his methods have the blessings of the patriarchs or not, but it is interesting to see that in this particular respect the Huygensian attitude confers more freedom than the Newtonian and that, in regard to the broad conception of the place of hypotheses, Huygens was a more profound methodologist than Newton. Newton, on the other hand, showed the clearer grasp of the nature of scientific laws as inductions from which the particular event might be deduced. Whether he would have regarded it as the true function of science to build up a colligative system from which, ultimately, laws might be deduced is another matter. Probably it would appear to him, as it does to certain modern critics, to be a repetition of the errors of Cartesianism though in a more scientific—if not less metaphysical—guise.

In the scientific work of the eighteenth and nineteenth centuries the distinction which presents itself before many others is that of the status of mathematical laws. Following Newton's example, some physicists considered that it was undesirable to go beyond the mathematical formulation of laws. Ampère and Cauchy investigated electric forces mathematically on the Newtonian law of inverse squares; in Great Britain Faraday and Clerk Maxwell studied the effect of the intervening medium and tried to picture a mechanism by which electric forces could be transmitted. The former is the Newtonian, the latter the Huygensian method. The same divergence is clear in the treatment of the concepts of energy and entropy. As mathematical quantities they are doubtless valid abstractions; as fictitious entities existing in their own right, they raise questions of their reality which cannot be answered with final agreement except by those who adopt the same metaphysics. To keep science clear of metaphysical entanglements has long been regarded as of the first importance. It is surely in this light that Newton's famous axiom should be remembered.

[1] "La Théorie physique", 18 (1906).
[2] "The Metaphysical Foundations of Modern Science", 207 (1932).
[3] Op. cit., 370 *et seq.*

OBITUARIES

Mr. Arthur Grove

THE death of Arthur Grove on February 2, in his seventy-eighth year, removes a noted personality from the ranks of the scientifically minded gardeners. The youngest son of Sir George Grove, editor of the "Dictionary of Music and Musicians" and first director of the Royal College of Music, Arthur Stanley Grove was, like his father, an engineer, but early in his life he became attracted to horticulture, as his father had been to music, and he acquired a remarkable collection of plants in his garden near Henley. Stimulated by H. J. Elwes, Grove became interested in the genus Lilium and made this a lifelong study, but though he specialized on lilies he maintained a very wide interest in gardening and was for many years the chief contributor on horticultural subjects to *The Times*. He had an unusual knowledge of good plants and wrote extensively in the gardening Press.

The success of Grove's horticultural skill should be appraised in the light of virus diseases. He took infinite pains with his lilies and achieved remarkable success, but some species repeatedly failed after a few years. In any lily collection there is virus-infected material from which the most carefully raised seedlings may become affected. Grove was baffled and mystified, but never gave in. He attributed his failures to such factors as soil acidity, or alkalinity, or to Botrytis. This was not surprising since plant pathologists could give him no light. Virus diseases of lilies were, indeed, not generally recognized in Great Britain until some years after Ogilvie's work had been published (1928).

The most important work upon which Grove embarked was the preparation of a Supplement to Elwes's "Monograph of the Genus Lilium", and in this he was able to incorporate the results of his unrivalled experience. He had a profound respect for authority, and as an amateur he hesitated to run counter to the writings of professional botanists. It was partly owing to this fine feeling and partly on account of ill-health that he sought expert help when he came to face the critical and much confused species that had to be described for the later parts of the Supplement.

Grove was elected a fellow of the Linnean Society in 1903. He was awarded the Victoria Medal of Honour of the Royal Horticultural Society in 1924, the Veitch Memorial Gold Medal in 1934, and the Gold Medal of the Massachusetts Horticultural Society in 1936. He was, moreover, the first recipient of the Lyttel Lily Cup. He had an attractive literary style and will always be remembered for his great charm of manner and for his whimsical humour.

A. D. C.

Dr. Edward Bureš and Prof. Stefan Kopeć

IT has recently been learned that Dr. Edward Bureš, lecturer in organic chemistry at the Charles University, Prague, until the Germans closed the university, has died. He was arrested soon after the outbreak of war and has presumably died in a German concentration camp.

Dr. Bureš had made several contributions to the study of plant chemistry, having described a number of new sterols, for example, raphanisterol from mustard, alkaloids (nymphæine from water-lily) and glycosides.

Prof. Stefan Kopeć, professor of biology in the University of Warsaw, was killed some months ago by the Germans. He was arrested among the 150 Polish persons, every tenth of whom was shot. They had not committed any offence but were taken as hostages after an unsolved incident in which a German was killed.

We regret to announce the following deaths:

Dr. Frank Conrad, assistant chief engineer of the Westinghouse Electric and Manufacturing Company of Pittsburgh, on December 11, aged sixty-seven years.

Prof. P. H. Hanus, emeritus professor of education in Harvard University, on December 14, aged eighty-six years.

Prof. Simon Isaac, formerly professor of internal diseases in the University of Frankfort, an authority on diabetes.

NEWS and VIEWS

Dr. W. Temple : Archbishop of Canterbury

THE King has been pleased to nominate the Right Hon. and Most Rev. William Temple, Lord Archbishop of York, Primate of England and Metropolitan, for election by the Dean and Chapter of Canterbury in the place of the Right Hon. the Most Reverend Cosmo Gordon Lang, Lord Archbishop of Canterbury, Primate of All England and Metropolitan. Dr. Temple, who is sixty years of age, has been Archbishop of York since 1929. He was president of the Oxford University Union in 1904, and afterwards a fellow of Queen's College. He was appointed headmaster of Repton School in 1910, where he stayed four years.

Educationists will welcome this appointment, for Dr. Temple has associated himself with social problems and education for many years. He was president of the Workers' Educational Association from 1908 until 1920, and has thus guided it to the great influential position which it now holds in adult and other educational activities throughout the country. His lectures and addresses, especially to undergraduates and other young people, are always listened to with eager attention. In a sermon during the jubilee celebrations of the Victoria University of Manchester, he especially emphasized the need for greater encouragement of research among the staffs of our universities. He is a philosopher of outstanding merit.

The King has also been pleased to nominate the Right Rev. C. F. Garbett, Lord Bishop of Winchester, for election by the Dean and Chapter of York in the place of Dr. Temple. Dr. Garbett has also shown himself deeply interested in social questions.

Dr. Peter Kapitza, F.R.S. : Faraday Medallist

DR. PETER KAPITZA, director of the Institute for Physical Problems of the Academy of Sciences of the U.S.S.R., has been awarded the Faraday Medal of the Institution of Electrical Engineers, "for his notable contributions to science in the generation and utilization of intense magnetic fields". Dr. Kapitza began his scientific career some twenty years ago in the Cavendish Laboratory, Cambridge, where he succeeded in producing magnetic fields much stronger than had hitherto been obtainable. To avoid the difficulty of the heating up of the coil in which the magnetic field was produced, he used a large power for a very short time only ; the time was, however, long enough for most magnetic effects to establish themselves, and Kapitza developed many ingenious devices for studying the magnetic properties of matter in such transient fields. With this powerful technique he cleared up various doubtful points about magnetization at high fields and was able to measure the magnetostriction of diamagnetic substances for the first time ; a series of researches on the change of electrical resistance of metals in the new region he had opened up also brought to light many interesting new features.

Since magnetic properties generally become much more marked at low temperatures, Kapitza set about the building of a cryogenic laboratory, and here again he brought to bear that rare combination of the talents of the physicist and the engineer which characterizes so much of his work. His work in Cambridge culminated with the development, in the Royal Society Mond Laboratory, of a new method of liquefying helium using the principle of adiabatic expansion. In 1934 he returned to his native country to set up and direct the new Institute for Physical Problems of the Academy of Sciences of the U.S.S.R., where he has continued both his magnetic and his low temperature researches. Recently he has made important discoveries about the remarkable 'superfluid' properties of liquid helium, and he has also developed a much more economical method of liquefying air. For this latter work, which is of considerable industrial as well as scientific interest, he was awarded the Stalin Prize last year.

Rev. T. E. R. Phillips

THE University of Oxford is conferring the degree of D.Sc., *honoris causa*, on the Rev. T. E. R. Phillips on February 28. Mr. Phillips's astronomical work began in 1896, while he was curate at Hendford, near Yeovil, where he started systematic work on the planets, especially Jupiter and Mars. In 1916, he was appointed rector of Headley and he set up an observatory in the rectory glebe, where he added an 18-in. reflector (mirror by With) to his other equipment. Mr. Phillips's work on Jupiter has consisted mainly in investigating the surface currents, and the times of the passage of various surface features across the central meridian of the planet are included in this. Other features to which he gave a considerable amount of attention are a 'circulating current' in the southern hemisphere of the planet, the Red Spot, and the South Tropical Disturbance. He has recorded more than a dozen distinct surface currents, which are well defined and show minor variations of period ; incidentally, it may be mentioned that he obtained some 30,000 spot transits. The 8-in. refractor loaned by the Royal Astronomical Society was used for double-star measurements and also for determining the light-curves of long-period variables. The results of his harmonic analysis of the light-curves of about eighty stars were given in his presidential address to the British Astronomical Association in 1916.

Mr. Phillips has been president of Commission 16 of the International Astronomical Union, specially concerned with the physical study of the planets, president during 1914–16 of the British Astronomical Association, and during 1927–29 president of the Royal Astronomical Society. Mr. Phillips is also a meteorologist, and has kept an unbroken daily record of temperatures and rainfall at Headley for twenty-five years. These results and also the results of his harmonic analysis of a number of annual temperature curves in various parts of the British Isles will be published after the War. He is a keen botanist and is specially interested in British and Alpine flora. He is still continuing to use the observatory at Headley, although he retired from active work in the church a year ago, owing to failing health. His many friends hope that he will be long spared to carry on his scientific work.

Home-grown Timbers

THE Forest Products Research Laboratory at Princes Risborough has recently issued a third edition (War Emergency Edition) of the "Handbook of Home Grown Timbers" (H.M. Stationery Office, 1941). This has become necessary owing to the greater demand being made on home resources due to a decrease of imports through the restriction of available shipping and an intensified demand. The edition includes information on a number of timbers not in general use, if not entirely unutilizable in peace-time, which can, it is stated, be satisfactorily employed for various useful purposes under war-time conditions. A notable addition, which the wholesale destruction caused by the German bomber has rendered advisable, is information on the fire-resisting properties of most of the species. These valuable data are the result of extensive experimental work carried out at the Laboratory. British grown timbers, we are told, as can be readily imagined, are now being employed in greater quantities and for very exacting purposes, and users are frequently at a loss to know how a particular species may best be utilized for some specified object for which imported timbers have been used in the past. It would be useful and interesting if a list could be made of the actual purposes throughout the country for which home-grown timbers are now being employed.

Genes, Viruses and Proteins

Vol. 9 of the Symposia on Quantitative Biology, published by the Biological Laboratory, Cold Spring Harbor, Long Island, has just appeared. It covers in about 300,000 words the immediate field of "Genes and Chromosomes" indicated by its title, and also a wider range of recent research which can be related to the study of chromosome structure either technically or theoretically. There are thirty-four papers and discussions dealing with optical observations, including the use of the ultra-violet and electron microscopes, X-ray experiments on cell-activity and chromosome breakage, the theory of protein and nucleic acid structure and behaviour, the chemistry and mutations of viruses and genes, and finally the use of the heavy nitrogen isotope in the study of amino-acid exchanges. The series ends by a summary and review by H. J. Muller. This volume is remarkable not only for almost every contribution being new in detail of technique and observation, but also for the whole point of view having arisen from the converging development of genetics, cytology and chemistry during the last ten years.

Electric Cables

Mr. S. W. Melsom, of the Cable Makers' Association, devoted the greater part of his chairman's address to the Transmission Section of the Institution of Electrical Engineers (*J. Inst. Elect. Eng.*, 89, Pt. 1, No. 13; 1942) to the subject of cable engineering, first making brief reference to the development of solid plastic materials and proceeding to the question of their application to the manufacture of cables. The fierce light thrown on the older dielectrics by competition from the synthetics has revealed the extraordinarily high qualities of rubber and paper, and in the face of this experience new materials cannot be accepted without the fullest assurance of at least as good service with economic advantage. Although chemists have worked hard on the subject, their products have yet to be proved superior or even equal to vulcanized rubber, but doubtless the apparently parallel paths of the rubber and plastics chemists will ultimately be found to converge, with great benefit to both. The address then dealt briefly with very high voltage cables, research and the much discussed question of standardization, and it was closed with an outline of the standing and responsibilities of the engineer in relation to society, suggesting that it is time the engineer took his fair share in responsibility for the control of affairs, for otherwise the most lucrative profession would be that of reaper and not cultivator, a process which, if carried too far, will choke the life out of any industry or nation permitting it.

Heat Requirements of Buildings

A valuable compendium of information has been issued by the Institution of Heating and Ventilating Engineers in a publication entitled "Recommendations for the Computation of Heat Requirements for Buildings" (Pp. iii+41. 1*s*. 9*d*.) This comprises a section of the guide to current practice which was recently compiled for the use of members, and it places at the disposal of all interested in the subject a most comprehensive collection of data strictly in accordance with present-day practice. The information is conveniently grouped in three parts. That on temperature-rise and rates of change gives the recommended values applicable to buildings ranging alphabetically from aircraft sheds to warehouses. The design of heating and ventilating installations has been, in recent years, greatly influenced by legislation affecting factories, and a special section has been devoted to this class of building. Heat transmittance co-efficients for walls, floors and roofs in a variety of materials constitute the second part, which also includes data as to the allowances to be made for height and for conditions of intermittent heating. The third part deals with conductivity data and the calculation of overall coefficients for composite walls, floors and roofs. Included in it is a table of thermal conductivity and resistivity of practically all the proprietary materials used in building construction. Much of the information given has been obtained from such independent sources as the National Physical Laboratory and the U.S. Bureau of Standards, and for the benefit of those unfamiliar with heat-loss calculations as applied to buildings a typical example is included.

Long-Distance Telephony

Voice-frequency signalling and dialling in long-distance telephony forms the subject of a paper read before the Institution of Electrical Engineers by W. G. Radley and E. P. G. Wright. The paper first sets forth the reasons for modern methods of signalling and dialling over long-distance telephone circuits and for preferring the use of voice-frequency methods for these purposes. An outline is given of the technical problems involved in the design of voice-frequency signalling systems and the author deals with operating requirements as well as with receiver and system design. Reference is also made to the recommendations of the International Consultative Committee on Long-Distance Telephony regarding means of avoiding interference between different signalling systems on international connexions and a forecast is given of the future development of signalling and dialling over long-distance telephone circuits. An interesting table is included which compares the various long-distance signalling systems in great detail.

The Yellow Fever Situation

According to the *Journal of the American Medical Association* of January 10, owing to perfection of control methods initiated by Gorgas and Oswaldo

Cruz, there have been no reports of yellow fever in Brazil or in the whole western hemisphere during 1941 or in the previous three years. Attention now centres on jungle yellow fever, which differs from urban yellow fever in that it is not transmitted by *Aedes ægypti*. Risk of yellow fever, however, will remain so long as the jungle type persists. It is no longer enough to accept clinical opinion, however experienced, for the diagnosis of yellow fever, but laboratory information by viscerotomy and protection tests is necessary. Vaccination has been carried out on a large scale with satisfactory results. The International Health Board of the Rockefeller Foundation continues to co-operate in the epidemiological and laboratory studies of the disease and in the preparation of vaccine.

Seismology in the South-western Pacific

DURING the past year, 1941, a new teleseismic station was established at Auckland, New Zealand. The station is equipped with a Milne-Shaw seismograph, and accurate time is maintained on the records. It is administered by the Dominion Observatory, Wellington (acting director, Mr. R. C. Hayes), as part of the New Zealand seismological service. The instrument was established with the co-operation of the Auckland Institute and Museum, and is operated by members of the Museum staff. This new station is proving of great value in the recording of earthquakes, not only in the New Zealand region, but also in the south-western Pacific generally.

Rockefeller Medical Studentships

THE Rockefeller Foundation is again offering twenty-five studentships to enable men or women to take part of their clinical courses at medical schools in the United States and Canada. The offer is made primarily to help students in schools which have suffered in consequence of enemy action, but suitable applications from all medical schools in England, Wales, Scotland and Northern Ireland will be welcomed. The studentships, which are tenable for two years, will cover all tuition fees, maintenance, etc., but not the cost of travel to and from America. Students must return to Great Britain in order to take their final examination. Application forms may be obtained from the Dean of the Medical Faculty or School and must be sent in only by the Dean not later than April 25.

Journal of the Franklin Institute

IN spite of the difficulties of Atlantic transport, copies of the principal American scientific journals continue to reach us. The *Journal of the Franklin Institute* is outstanding from those of general interest and the December 1941 number, including the index to Vol. 232, maintains the usual high standard and variety of interest. A paper by Carl van Doren on Franklin himself deals with the remarkable American after whom the Institute is named. His qualities as man of science, diplomatist, statesman, business man, economist, printer, humorist and wit, writer and sage are all surveyed. Two technical papers are followed by notes from the National Bureau of Standards and the Biochemical Research Foundation, and a lively group of notes on current topics and book reviews. The *Journal* is published monthly from the Franklin Institute in Philadelphia.

Night Sky in March

THE length of day and night will reach equality when the sun enters the vernal equinox on March 21d. 6h. U.T. The moon is full on March 3d. 0h. 20m. and on March 2–3 there will be a total eclipse of the moon, visible at Greenwich. The circumstances of the eclipse are as follows:

Moon enters penumbra ...	March	2d.	21h.	27·6m.
,, ,, umbra		2	22	31·3
Total eclipse begins		2	23	33·2
Middle of eclipse		3	00	21·5
Total eclipse ends		3	01	09·8
Moon leaves umbra		3	02	11·5
Moon leaves penumbra ...		3	03	15·0

The moon is new on March 16d. 23h. 50m. and there will be a partial eclipse of the sun on March 16–17, invisible at Greenwich, visible only in the South Pacific and Antarctic Oceans.

The occultations of the planets by the moon are as follows: Venus on March 13d. 15h., Venus 2° N.; Mercury on March 14d. 22h., Mercury 3° S.; Saturn on March 21d. 15h., Saturn 3° N.; Mars on March 22d. 20h., Mars 7° N.; Jupiter on March 23d. 08h., Jupiter 5° N.; Mars and Neptune will be in conjunction on March 2d. 7h., Mars being 1·6° N. Mars, Jupiter and Saturn are well placed for observation during the month. Venus is a morning star, and in the middle of the month rises at 4h. 30m. Among the occultations of stars not fainter than mag. 4, the following may be noted: March 4d. 1h. 22·2m., β Virginis; March 22d. 18h. 41·3m., θ^1 Tauri; March 22d. 18h. 47·8m., θ^2 Tauri.

Announcements

LIEUT.-GENERAL A. G. L. MCNAUGHTON has been elected an honorary member of the Institution of Electrical Engineers. This distinction has been conferred upon General McNaughton, who was until recently commander of Canadian troops in Great Britain, in appreciation of the outstanding services rendered by him to promote the practical application of science to industry, in which respect his work on high-voltage research during his presidency of the Canadian National Research Council has been specially notable.

COLONEL S. J. THOMPSON, governing director of Messrs. John Thompson, Ltd., Wolverhampton (water tube boilers, motor frame pressings, etc.), has been elected president of the Institution of Mechanical Engineers, in succession to Mr. W. A. Stanier, chief mechanical engineer of the London Midland and Scottish Railway, whose term of office has expired.

JOHN SCOTT Medals and Premiums of the American Philosophical Society have been awarded to Major Edwin H. Armstrong, professor of electrical engineering, Columbia University, for his work in frequency modulation in radio; and Robert R. Williams, chemical director, Bell Telephone Laboratories, for his work on thiamin (vitamin B_1).

ARRANGEMENTS have been completed for the publication of a volume of "Essays in Anthropology" to be presented to Rai Bahadur Sarat Chandra Roy, the distinguished Indian worker in this field. A limited edition is being printed. Orders for copies, with remittance (*Rs.* 12), should be sent to Dr. D. N. Majumdar, Anthropology Laboratory, University of Lucknow.

LETTERS TO THE EDITORS

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

Neutralization of Sulphonamide Inhibition of Yeast Growth by *p*-Aminobenzoic Acid

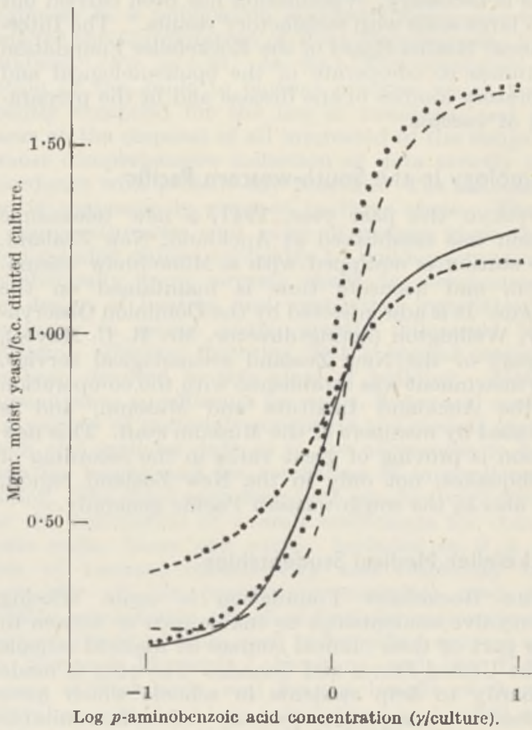
FROM the many theories concerning the mode of action of sulphonamides, one by Fildes[1] has recently emerged which, because of the volume of evidence brought forth in its support, is now considered the most logical explanation of sulphonamide bacteriostasis. Fildes has suggested that inhibitors of microbial growth are effective as a result of interference with an "essential metabolite", a substance which he defined as having an essential part in the chain of synthesis necessary for growth. Woods[2] was able to show that *p*-aminobenzoic acid nullified the growth-inhibitory action of sulphanilamide for a number of bacteria. Studies in this laboratory[3] have confirmed Woods's original observations. It was found that the *in vitro* bacteriostatic effect of sulphanilamide, sulphapyridine, and sulphathiazole on streptococci, pneumococci, and staphylococci respectively was neutralized by *p*-aminobenzoic acid. Dimond[4] has recently reported similar growth-inhibition of the fungus *Trichophyton purpureum* by sulphanilamide and neutralization of this inhibition by *p*-aminobenzoic acid. The present report describes the application of Woods's findings to yeast growth and suggests the importance of *p*-aminobenzoic acid in yeast metabolism.

The test organism, *Saccharomyces cerevisiæ* No. 139, was cultured in the synthetic basal medium of Snell, Eakin and Williams[5], made complete by the addition of sufficient biotin to support half-maximum growth. The complete basal medium in 10 c.c. quantities was inoculated with 0·06 mgm. moist yeast, and incubated for sixteen hours at 30° C. Growth was measured with a photo-electric nephelometer.

The inhibitory effect of sulphanilamide, sulphapyridine, sulphaguanidine and sulphathiazole was determined by adding the sulphonamide under test to flasks of inoculated medium and incubating as above. Complete inhibition of yeast growth was obtained with the following amounts of sulphonamides: sulphanilamide, 2·0 mgm.; sulphapyridine (sodium salt), 2·5 mgm.; sulphaguanidine, 2·0 mgm.; sulphathiazole, 1 mgm.

The neutralizing effect of *p*-aminobenzoic acid on yeast growth inhibition was demonstrated by the addition of varying amounts of *p*-aminobenzoic acid to flasks of inoculated media, each containing that amount of sulphonamide necessary for complete inhibition of growth. The results are illustrated in the accompanying figure, in which the quantity of yeast growth, plotted against *p*-aminobenzoic acid, was, in general, similar for all the sulphonamides tested. In all cases, the variation of *p*-aminobenzoic acid concentration from 0·1 to 10·0 micrograms per culture resulted in complete range of neutralization.

Since the basal medium used in these experiments was synthetic in composition and did not contain *p*-aminobenzoic acid, it was of interest to determine whether or not this substance was synthesized. A sixteen-hour culture in the complete basal medium as described above was autoclaved to effect hydrolysis and centrifuged to remove cellular debris. The supernatant was assayed for *p*-aminobenzoic acid content by comparing its neutralizing effect on sulphathiazole with that of a reference standard set of *p*-aminobenzoic acid dilutions. It was found that such supernatants contained a substance which neutralized the inhibitory effect of sulphathiazole. When measured in this way, 10-c.c. cultures contained a neutralizing substance equivalent to approximately 1·5 micrograms of *p*-aminobenzoic acid.



NEUTRALIZATION OF SULPHONAMIDE INHIBITION OF YEAST GROWTH BY *p*-AMINOBENZOIC ACID.

— · — · — ·, sulphapyridine (Na), 2·5 mgm./culture.
. . . ., sulphaguanidine, 2·0 mgm./culture.
— — — —, sulphanilamide, 2·0 mgm./culture.
————————, sulphathiazole, 1·0 mgm./culture.

On the basis of the foregoing evidence, the following conclusions seem warranted:

Yeast growth is completely inhibited by sulphonamides. This inhibition is neutralized by *p*-aminobenzoic acid.

Yeast synthesizes either *p*-aminobenzoic acid or its biologically active equivalent. *p*-Aminobenzoic acid may, therefore, be important in yeast metabolism, possibly as the "essential metabolite" of Fildes.

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Jan. 5.

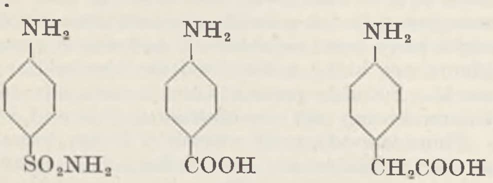
[1] Fildes, *Lancet*, 1, 955 (1940).
[2] Woods, *Brit. J. Exp. Path.*, 21, 74 (1940).
[3] Landy and Wyeno, *Proc. Soc. Exp. Biol. and Med.*, 46, 59 (1941).
[4] Dimond, *Science*, 94, 420 (1941).
[5] Snell, Eakin and Williams, *J. Amer. Chem. Soc.*, 62, 175 (1940).

Relative Acidity and Basicity of Sulphanilamide and *p*-Aminobenzoic Acid

Since no determination of the relative acidic or basic dissociation constants of sulphanilamide appears to have been published, this drug was potentiometrically titrated against $N/1$ hydrochloric acid followed by $N/1$ sodium hydroxide. *p*-Aminobenzoic acid and *p*-aminophenylacetic acid were treated similarly for purposes of comparison. In each case, a solution of freshly recrystallized, pure material (0·0005 gm. mol. in 20 ml. of water) was titrated in a beaker fitted with a stirrer, glass electrode and potassium chloride agar bridge connected to a calomel half-cell and a Leeds and Northrup universal *p*H potentiometer assembly. The results are shown in the accompanying figure and the derived (uncorrected) dissociation constants are given in the table.

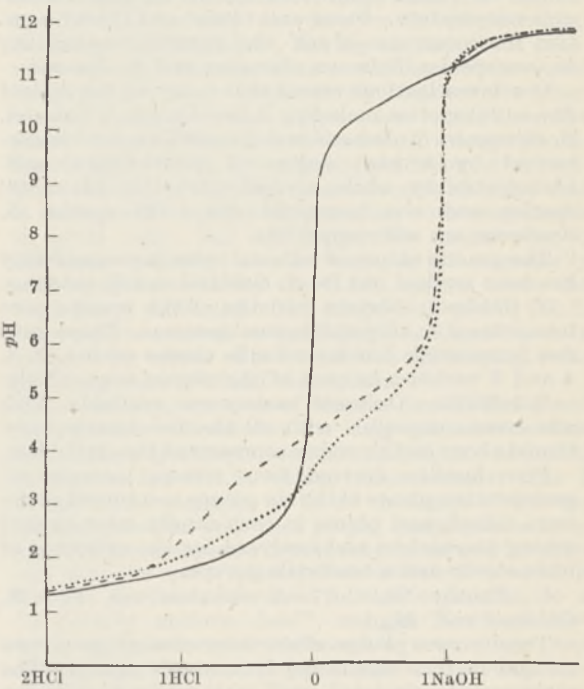
Substance	K (base)	K (acid)
Sulphanilamide	$1·6 \times 10^{-12}$	$6·3 \times 10^{-11}$
p-Aminobenzoic acid	$3·1 \times 10^{-12}$	$1·5 \times 10^{-5}$
p-Aminophenylacetic acid ...	$3·1 \times 10^{-11}$	$6·3 \times 10^{-6}$

Woods[1] showed *in vitro* that *p*-aminobenzoic acid can counteract the antibacterial action of sulphanilamide; Selbie[2] found that *p*-aminobenzoic acid inhibited the curative action of sulphanilamide in the streptococcal infection of mice; Findlay[3] found that *p*-aminobenzoic acid interfered with the curative action of sulphanilamide on mice infected with the virus of lymphogranuloma; Rubbo and Gillespie[4] first showed that *p*-aminobenzoic acid was a growth factor for a bacterium (*Clostridium acetobutylicum* (W.)) and that its activity could be neutralized by a large excess of sulphanilamide; finally, Lipmann[5] reported that sulphanilamide inhibited the oxidation of *p*-aminobenzoic acid by peroxidase and hydrogen peroxide. There is thus good support for Fildes's hypothesis[6] that sulphanilamide acts biologically by competing against the essential metabolite, *p*-aminobenzoic acid, for the possession of particular groups (receptors) in enzyme systems. Fildes further states that such an inhibitor as sulphanilamide requires sufficient chemical similarity to the essential metabolite in order to fit the same enzyme system and yet must differ sufficiently to be devoid of metabolic activity.



Sulphanilamide　　*p*-Aminobenzoic acid　　*p*-Aminophenylacetic acid

Sulphanilamide answers to this description since, as the above formulæ show, it has a certain steric similarity to *p*-aminobenzoic acid and resembles it in forming salts with strong acids and strong bases. As a result of the potentiometric titrations, this comparison can now be put on a more quantitative basis. Sulphanilamide and *p*-aminobenzoic acid are found to be bases of approximately the same strength, but sulphanilamide is about a million times weaker as an acid. This weak acidity of sulphanilamide is probably unessential as is shown by the high efficiency of 4 : 4′-diaminodiphenylsulphone. On the other hand, the basicity seems highly significant, being common to both substances. The biological importance of such a feeble basicity (K base = 10^{-12}) may be connected with chemical reactions other than salt formation, possibly with the formation of hydroxylamino compounds. At least, it is known that stronger, simple aromatic bases such as aniline (200 times) and *p*-aminophenol (2,000 times stronger than sulphanilamide) do not possess sulphanilamide-like activity.



Potentiometric Titration Curves at 20° C.
...... *p*-aminobenzoic acid;
—— sulphanilamide;
- - - *p*-aminophenylacetic acid.

p-Aminophenylacetic acid was included in these titrations since Rubbo *et al.*[7] showed that it is ten times as potent a growth factor for *Cl. acetobutylicum* (W.) as *p*-aminobenzoic acid and, unlike the latter, possesses no anti-sulphanilamide activity[8]. The titrations show that both growth factors have approximately the same strength as acids, but *p*-aminophenylacetic acid is ten times as strong a base. While it would be premature to draw conclusions from these results, the further study of growth-factor chemistry along these lines seems indicated.

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Nov. 21.

[1] Woods, D. D., *Brit. J. Exper. Path.*, 21, 74 (1940).
[2] Selbie, F. R., *Brit. J. Exper. Path.*, 21, 90 (1940).
[3] Findlay, G. M., *Brit. J. Exper. Path.*, 21, 356 (1940).
[4] Rubbo, S. D., and Gillespie, J. M., Nature, 146, 838 (1940).
[5] Lipmann, F., *J. Biol. Chem.*, 139, 977 (1941).
[6] Fildes, P., *Lancet*, 1, 955 (1940).
[7] Rubbo, S. D., Maxwell, M., Fairbridge, R. A., and Gillespie, J. M., *Aust. J. Exper. Biol. Med. Sci.*, 19, 185 (1941).
[8] Rubbo, S. D., and Gillespie, J. M., *Lancet*, in the press.

Genetic Nature of Self- and Cross-Incompatibility in Potatoes*

INVESTIGATIONS on the various types of sterility in potatoes were begun in 1936 and in the present note the genetic nature of sterility resulting from physiological incompatibility is reported.

Although a great amount of work on self- and cross-incompatibility has been done in a variety of crops, very little information on the subject is available on potatoes. Stout and Clark[1] and Clark[2] mention the occurrence of self- and cross-incompatibility in two species, *Solanum chacoense* and *S. Jamesii*.

Our investigations reveal that many of the diploid ($2n = 24$) species, including *S. aracc-papa*, *S. Caldasii*, *S. chacoense*, *S. Jamesii* and *S. subtilius*, are characterized by a high degree of physiological self-incompatibility while several tetraploid ($2n = 48$) species and the hexaploid ($2n = 72$) species *S. demissum* are self-compatible.

The genetic nature of self- and cross-incompatibility has been worked out for *S. Caldasii* and *S. subtilius*.

S. Caldasii: Sixteen varieties of this species were intercrossed in all possible combinations. These gave five intra-sterile but inter-fertile classes with 1, 2, 4, 4 and 5 varieties in each of the classes respectively.

S. subtilius: Only one variety was available. This was cross-compatible with all the five classes mentioned above and therefore represented the sixth class.

Four families derived from crosses between representative plants of the six groups mentioned above were raised, and plants in each family inter-crossed among themselves and analysed for the existence of intra-sterile and inter-fertile groups.

1. Family No. C–77—*S. subtilius* var. V_1 × *S. Caldasii* var. M_{05}.

Twenty-two plants when inter-crossed gave two (*A* and *B*) inter-fertile but intra-sterile classes. The male parent associated itself with group *A* while the female parent (group *C*) was cross-compatible with both the groups.

2. Family No. D–27—*S. Caldasii* var. 012 × *S. Caldasii* var. 07.

Seventeen plants when inter-crossed gave results similar to those obtained in cross No. 1. Class *E* included the male parent while class *F*, the female parent, was cross-compatible with both *D* and *E* classes.

3. Family No. D–25—*S. Caldasii* var. 04 × *S. Caldasii* var. 01.

Twenty-two plants when inter-crossed gave four almost equal-sized intra-sterile but inter-fertile groups. Both *K* and *L*, the male and female parents respectively, were cross-compatible with all the four groups, *G*, *H*, *I* and *J*.

4. Family No. *D*–26—*S. Caldasii* var. 07 × *S. Caldasii* var. 01.

The behaviour of this cross was similar to cross No. 3. The four intra-sterile inter-fertile groups, *M*, *O* and *P* were compatible with the two parental groups *Q* and *R*.

The four crosses above gave in all eighteen groups of plants, represented by the letters *A* to *R*. Examination of the data showed that certain groups contained genotypically similar plants with regard to self-sterility factors. To confirm this conclusion and to determine the major groups, two plants from each of the eighteen groups were selected and the thirty-six plants were inter-crossed in all possible combinations. The results so obtained are given in the accompanying diagram, which clearly brings out the relationship between the various groups. The two plants under each group behaved similarly with regard to compatibility relationship. The eighteen original groups of plants, it will be seen, are composed of only eight major groups.

The results outlined above show that self- and cross-incompatibility in *S. Caldasii* and *S. subtilius* are distinctly inherited characters.

Groups

Constitution:	s^1s^2				s^1s^3	s^1s^4			s^1s^5		s^2s^3	s^2s^4			s^2s^5		s^4s^5	
Groups	A	F	H	O	C	D	G	P	L	Q	B	E	K	R	J	M	I	N
A	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+
F	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+
H	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+
O	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+
C	+	+	+	+	−	+	+	+	+	+	+	+	+	+	+	+	+	+
D	+	+	+	+	+	−	−	−	+	+	+	+	+	+	+	+	+	+
G	+	+	+	+	+	−	−	−	+	+	+	+	+	+	+	+	+	+
P	+	+	+	+	+	−	−	−	+	+	+	+	+	+	+	+	+	+
L	+	+	+	+	+	+	+	+	−	−	+	+	+	+	+	+	+	+
Q	+	+	+	+	+	+	+	+	−	−	+	+	+	+	+	+	+	+
B	+	+	+	+	+	+	+	+	+	+	−	+	+	+	+	+	+	+
E	+	+	+	+	+	+	+	+	+	+	+	−	−	−	+	+	+	+
K	+		+		+		+		+	+		−	−	−	+		+	
R	+	+	+	+	+	+	+	+	+	+	+	−	−	−	+	+	+	+
J	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−	−	+	+
M	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−	−	+	+
I	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−	−
N	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−	−

DIAGRAM SHOWING THE RELATIONSHIP OF THE EIGHTEEN INTRA-STERILE GROUPS.

—, cross- and self-incompatible combination; +, cross-compatible combination.

Detailed examination of the data available shows that there are at least five sterility factors S^1, S^2, S^3, S^4 and S^5 which, operating in various combinations, determine self- and cross-compatibility. The genetic composition of the eight major groups of plants shown in the diagram has been established and is given in the diagram. The mode of action of these factors is in conformity with the oppositional factor hypothesis developed by East and Mangelsdorf[3]. Here it is of interest to note that the five '*S*' factors can give only ten possible combinations out of which eight have been established, and the representative plants are being maintained as 'testers' for further work. Suitable crosses have been made to secure the remaining two combinations, S^3S^4 and S^3S^5.

There is evidence of a sixth '*S*' factor, but the data so far available are not sufficient to establish its existence positively. Some varieties of *S. chacoense* appear to possess in addition to '*S*' factors a fertility factor '*F*'. Investigations in these directions are in progress.

It may be pointed out here that *S. subtilius* and *S. Caldasii*, which resemble each other closely in their floral and vegetative characters and in their reactions to short-day conditions, also possess '*S*' factors in the same allelic series. Therefore there does not appear to be any reason to regard them as separate species.

The investigation reported above forms part of the work of the Potato Breeding Scheme for Northern India financed by the Imperial Council of Agricultural Research the assistance of which is gratefully

* Slightly abridged.

acknowledged. We are indebted to Dr. S. Ramanujam, second economic botanist at this Institute, for valuable advice and suggestions.

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Oct. 28.

[1] Stout, A. B., and Clark, C. F., *Bull. U.S. Dept. Agric.*, **1159**, 1-32 (1924).
[2] Clark, C. F., *Mem. Hort. Soc.*, N.Y., **3**, 289 (1927).
[3] East, E. M., and Mangelsdorf, A. T., *Proc. U.S. Nat. Acad. Sci.*, **9**, 166 (1925).

Colour Measurement

Messrs. Smith, Guild and Donaldson's fears[1] of misconceptions were not at all justified. My letter[2] directed attention to certain *general* aspects of the physics of colour measurement. Emphasis was upon the need for knowledge in order to provide, for the concretest possible colour problems, the fullest possible analysis. Technical developments were a secondary consideration. I have not committed the elementary errors (*a*) of confusing the C.I.E. standards with the procedure, or (*b*) of thinking that errors in $\chi_{\text{C.I.E.}}$ values are possible owing to the procedure followed in the original experiments. The C.I.E. standards as such are exact. Also, I have no difficulties with the C.I.E. standard observer. All my references to the C.I.E. colorimetric system involved purely its place in a general physics of colour. The C.I.E. system of colorimetric standards, physical constants and agreed procedure is a convention which I do not regard as open to question, or as involved radically in this discussion. I do not question Messrs. Smith, Guild and Donaldson's appraisement of the merits of the C.I.E. system and have expressed parallel sentiments recently[3].

It would appear that Messrs. Smith, Guild and Donaldson hold the C.I.E. system to be capable of fulfilling the conditions of physical measurement for any field size, field brightness, etc. With this I cannot agree. The original measurements of Guild and Wright only provide evidence for the use of the C.I.E. system in the conditions under which such measurements were obtained. Ives[4], in referring to this same question of validity in relation to field size, brightness and the "visibility function", which is a very essential part of the C.I.E. data, says: "The International Illumination Commission in adopting a certain luminosity curve for the equal energy spectrum has in effect committed itself to the photometric conditions under which this curve was made and holds . . . and has authorised the use of a small photometric field." Guild[5], after the institution of the C.I.E. colorimetric system, has stated "that the colour-matching relations of stimuli can be expressed quantitatively in terms of two independent variables" [for example, x and y, in $x+y+z = 1$] "has to be qualified by restricting it to a range of conditions within which the visibility curve is known to be constant." This clearly is an application of the fact that, although a convention may be created at will, its physical validity is limited to the range of conditions within which it is known to apply. Its present bearing is obvious.

That on passing from foveal to peripheral vision matches of colour vary is clearly stated by Abney[6], Hering[7], von Helmholtz[8] and others. Such differences as these are not to be ascribed merely to peculiarities distinguishing the properties of the observer from a statistical average, as might be concluded from the letter of Messrs. Smith, Guild and Donaldson, but to common differences between the colorimetric properties of foveal and extra-foveal vision. Some consequences of this have in fact been described by Dr. Guild[9] in the words: "A large photometric field which averages the properties of a large region of the retina will in general give greater weight to radiation of shorter wavelength than a smaller field not extending so far beyond the fovea." This effect, which may be considerable, may readily be demonstrated and probably accounts for the phenomenon, described by Harrison[10], having an important practical bearing, namely, that a standard white equal in brightness to a comparatively bluish white when viewed by foveal vision, appears less bright than the same bluish white when viewed with a large photometric field. Thus, the essential condition to be complied with by a physical measurement, namely, that it shall correlate physical quantities with magnitudes in serial order, and not assign two magnitudes to the same quantity, or vice versa, is not necessarily still complied with by the C.I.E. system when the field size is extended. The adaptation of the C.I.E. colorimetric system to large fields is therefore only possible by virtue of a convention embodying some arbitrary and artificial restriction.

Let me say, apropos of Messrs. Smith, Guild and Donaldson's reference to the large colorimetric field used with a spectrophotometric match, as a technical device to aid C.I.E. colour measurement, that I intend these remarks to be no criticism of the use of this, provided only that proper qualifications as to validity are accepted; but it may be as well to point out that the spectrophotometric restriction, which is implied in their reference in this connexion to a "physically uniform field", reduces Newton's law, upon which colorimetry is based, to a tautology, and destroys its essential independence of spectrophotometric distribution. Even in such circumstances, that is, with a physically uniform field, differences in appearance in different parts of the field will sometimes be perceived, as when, for example, the radiation comprises two spectrum bands (their half-width may be at least 500 A.) centred approximately about wave-lengths 0·49 μ and 0·62 μ. In this case, although the major part of the field appears white, the central region of the field of vision appears pink, due to the effect of the macula lutea. In ordinary circumstances, vision synthesizes the whole impression received, into a visually homogeneous complex corresponding[2] to χ_N. That "regional variation of retinal properties is formally irrelevant to appearances in an extended field", as stated by Messrs. Smith, Guild and Donaldson, results in such cases in two differing appearances having simultaneously the same colorimetric specification. It is clear, however, that when conditions are not restricted to the sphere of significance of the quantities measured, "we can obtain a colour and brightness match under any conditions whatever and reduce the results to the *form* of a colorimetric relation without realising that the figures so obtained are meaningless"[5]. It will be clear from the foregoing that for conventional measurement to be physical measurement it must follow the *ABC* of physical science, namely, (*A*) experiment, (*B*) hypothesis, (*C*) verification, (*D*) standardization and measurement.

The statements of Messrs. Smith, Guild and Donaldson concerning accuracy may be accounted for by my statements being understood too narrowly, as applying to the C.I.E. system, instead of to the physics of the subject. Other points arise from Messrs. Smith, Guild and Donaldson's letter, which cannot be considered here out of considerations of space, but I hope it will be perceived that there remains material for investigation and grounds for new and broader conceptions in this subject.

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[1] Smith, Guild and Donaldson, Nature, **149**, 76 (1942).
[2] Perry, Nature, **148**, 691 (1941).
[3] Perry, *Proc. Phys. Soc.*, **54**, 9 (1942).
[4] Ives, *Proc. Opt. Conv.*, 143 (1926).
[5] Guild, "Discussion on Vision", 83/4 (London, 1932).
[6] Abney, "Colour Measurement and Mixture" (London, 1891).
[7] Hering, *Arch. f. ges. Physiol.*, **60**, 533 (1895).
[8] von Helmholtz, "Handb. d. physiol. Optik", 373 (1896).
[9] Guild, *Proc. Opt. Conv.*, 94 (1926).
[10] Harrison, *Proc. Phys. Soc.* (in the press).

The Electromagnetic Mental Picture

Recent text-books[1] reflect a certain uneasiness concerning the presentation to students of 'engineering' electricity. Some teachers advocate classical field theory alone, others introduce the electron theory, linking it with field theory at every stage. But the awkward unsolved problems of theory cannot then be avoided; thus, the two writers cited take quite different views concerning the induction of a current by motion of conductor and magnet respectively. These theoretical problems do not concern the designer and inventor, who needs above all a mental picture which will give a correct qualitative prediction of the functioning of any proposed apparatus. The formulæ which he will then apply for calculation may, as we know, be derived from various quite different hypotheses. Such differences as appear capable of deciding between different hypotheses are beyond the limits of error of our experimental powers[2].

Useful as the field picture undoubtedly is, the literature from the time of Faraday to the present day affords numerous examples of futile experiments and discussions based upon a too literal interpretation of it. Anyone can test the fact that it may mislead the most experienced. I have asked more than a dozen highly skilled physicists and radio research men whether the inductance of a straight wire is greater or less than that of the same wire bent into a circle. Without exception they answered wrongly. The only correct answer so far came from a young chemist. In the following cases the field picture as usually stated seems to be inadequate. If a toroid solenoid is placed with its plane at right angles to that of a horseshoe magnet and with a few of its turns in the pole gap of the magnet, the field picture seems to call for a torque on the toroid, but none on the magnet. Again, if a horseshoe magnet be rotated when placed so as to send flux through a few turns only of a short-circuited toroid, the field theory seems to call for an alternating current in the toroid, with no reactive torque on the magnet. It is interesting to consider also the effect of removing to a distance that part of the toroid which is *outside* the field of the magnet.

Radio is now perhaps the most important, and certainly the most progressive branch of electrical engineering, and here the particle picture is essential in vacuum tube discussion. Why not base the consideration of capacity, inductance and transmission upon it as well? Historical piety should not lead us to burden the beginner's mind with the difficulties and artificialities of the magnetic field picture, evidenced in the treatment of open and frame aerials, or of the fields near a transmitting antenna. If radio phenomena are dealt with on the lines given in my letter of August 16, 1941, the resulting mental picture of phenomena is at east as penetrating and illuminating as that given by field theory.

H. Stafford Hatfield.

Thatch End,
Hildersham, Cambs.
Jan. 24.

[1] e.g., Zeleny, "Elements of Electricity and Magnetism" (New York, 1935); Cullwick, "Fundamentals of Electricity and Magnetism" (Cambridge, 1939).
[2] O'Rahilly, "Electromagnetics: a Discussion of Fundamentals" (London and Cork, 1938).

Occurrence of Ctenodrilus in the Pacific

The annelid Ctenodrilus does not seem to have been recorded outside Europe, so that its occurrence in quantity at Ladysmith Harbour, on the east coast of Vancouver Island, B.C., is perhaps worthy of note. A small artificial lagoon was constructed there last summer in connexion with work on oyster culture in which the temperature of the water rose abnormally high. Material collected with a towed plankton net in this lagoon in August contained large numbers of *Ctenodrilus serratus* (O. Schmidt) which has probably been swept off the eel-grass and other vegetation growing there.

Ctenodrilus is of some special interest because divergent views have been held from time to time as to its taxonomic position. It is now generally regarded as a polychæte and placed among the Cirratulidæ.

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Nanaimo, B.C.
Jan. 2.

British Zoologist Prisoners of War in Europe

A fellow of this Society resident in a country at present under German control has generously offered to help British zoologists and entomologists who are prisoners of war in Germany or the enemy-occupied countries of Europe by correspondence and the loan of literature and such other means as are within his power.

The Royal Entomological Society of London is in regular communication with this fellow and invites information regarding zoologists and entomologists known to be prisoners of war in Europe. Exact details of name and address are the only essentials required, and it is emphasized that the Society has no means of transmitting messages to prisoners except through the service organized by the British Red Cross. Information may be sent to the Registrar, Royal Entomological Society of London, 41 Queen's Gate, London, S.W.7.

Francis J. Griffin
(Registrar).

Royal Entomological Society of London.

RESEARCH ITEMS

Morphology of a Cicada

J. W. EVANS has recently described the structure of the anomalous cicada *Tettigarcta* White. Little has been known of this insect, which betrays certain primitive features and is widely divergent from all other Cicadidæ (*Papers and Proc. Roy. Soc. Tasmania* for 1940; pub. 1941). Most of the specimens of the species *T. tomentosa* were taken at an altitude of 2,500 ft. during the Tasmanian winter (May and June), being attracted to lights at night. During the day the insect shelters beneath bark. Among various features the absence of tympanal organs in both sexes is notable. Tymbals, however, are present with their muscles slightly less developed in the female than in the male. It is conjectured that Tettigarcta is a descendant from an early cicadan stock that possessed well-developed sound-producing organs and sound-detecting organs in both sexes. For some reason, possibly associated with its nocturnal habits and cold environment, for other cicadas are essentially scent-loving insects, it has ceased to produce sound. Among other structural features the nymphs of Tettigarcta have nine segments to the antennæ—a higher number than is found in other cicadas. The venation is exceptionally complete with a separate costal vein in the hind wing. The male genitalia have a true ædeagus, also there is an unusual development of the basal plates, and harpagones are present.

Origins of Garden Roses

THE continuous blooming of garden roses must be a matter of wonder to those who compare it with the limited flowering of wild types. C. C. Hurst shows (*J. Roy. Hort. Soc.*, 66, Pts. 7 and 8, July and August, 1941) that this fortunate gene was introduced early in the nineteenth century from the China rose, *Rosa Chinensis*. It was first crossed with the Tea rose, *R. gigantea*, to form pink, blush and yellow stocks. The Pink China crossed with *R. moschata* gave the Noisette rose and when separately combined with the Pink Autumn Damask, *R. rubra*, it gave the Bourbon Rose. Hybrid Perpetuals were obtained by crossing hybrid China forms with Bourbons, Noisettes, and an English stock named after the Duchess of Portland. Hybrid Tea roses, occupying the throne of garden beauty, resulted from a fusion of Hybrid Perpetuals and Teas. The twentieth century brought the introduction of *R. lutea* from Persia, and with it the Pernet rose (Hybrid Perpetual × Austrian Briar, *R. lutea*). A wealth of historical detail is combined with genetical information in the papers under review.

A New Strain of Potato Virus

SINCE the discovery that interveinal mosaic and crinkle virus diseases of the potato are caused by mixtures of viruses, much work has been devoted to the isolation and identification of the constituents. Phyllis M. Clinch has recently elucidated one of the remaining problems (*Sci. Proc. Roy. Dub. Soc.*, 22, No. 46, 435–45, December, 1941). One source of interveinal mosaic, which is a mixture of viruses X and F, was found to include also a strain of top-necrosis. Removal of the X virus from this complex left a dual virus which is responsible for virulent tuber blotch, and also produces streak symptoms in the foliage. This has been found to be identical with a potato 'streak' the symptoms of which were described earlier by Koch and Johnson, and thus is now shown to be caused by a mixture of top-necrosis and F viruses.

A Genus of the Discomycetes

A MONOGRAPH of the Discomycete genus Rutstrœmia has recently been published by W. Lawrence White (*Lloydia*, 4, No. 3, 153, September, 1941). There is no modern handbook to the genera and species of the Discomycetes, and any critical study like the present monograph is a welcome contribution. The genus Rutstrœmia was originally established by Karsten, and has been revived to include many of the inoperculate Discomycetes at present scattered through the current genera Helotium, Phialea, Ciboria, Sclerotinia and Cyathicula. Forty-one species are described critically, and the material has been obtained from as many countries as present world conditions allow. A key to the American species is given, and seventy-five half-tone figures elucidate the text.

Physiological Genetics of Neurospora

G. W. Beadle and E. L. Tatum (*Proc. Nat. Acad. Sci.*, 27, 499–506; 1941) describe useful methods for the analysis of gene action in biochemical phenomena. They irradiate the fungus Neurospora with X-rays and grow the resulting strains from single-spore culture on a medium containing as many of the synthesized products as possible. They also grow these spore strains on media containing only the primary ingredients. A normal strain is able to synthesize vitamin B_6, etc., from this latter medium, but a mutant strain may not survive through its inability to do so. By adding vitamin B_6 to such a medium, they can restore the growth-rate of the mutant strain to that of the normal strains. Out of two thousand single-spore cultures analysed, one was found to be pyridoxinless (unable to synthesize vitamin B_6), another failed to synthesize *p*-amino-benzoic acid, a further one could not synthesize thiazole. All these mutants differ from normal by one gene differences. The physiological methods of analysis which are used will enable the steps in synthesis to be studied in relation to gene changes.

Evolution of Continents

IN the Bruce-Preller lecture for 1941 Sir Thomas Holland discusses the data bearing on the highly controversial subject of continental drift, and makes suggestions towards a possible reconciliation of the conflicting evidence (*Proc. Roy. Soc. Edin.*, B, 61, II (13), 149; 1941). He directs attention to various geological lines of evidence "which cannot, so far as anyone knows, be explained in any way except by continental drift". The most formidable among these is the record of Upper Palæozoic glaciation on each of the now scattered continents of Gondwanaland. On the other hand, there is the geophysical difficulty of finding an adequate motive force for continental drift, if the view that the earth is solid and strong down to about half its radius be accepted as a scientific fact. Holland points to one obvious direction in which we may look hopefully for a clue to the reason for this apparent inconsistency: "the evidences for continental drift all belong to a relatively distant past", whereas "the seismic records . . . indicate the state of the Earth's interior to-day". He suggests that so long as geophysicists give good reasons for denying the physical possibility of continental drift, there are only two courses open: (i) to reconsider the possibility that some of the geological revolutions in the past were on a scale large enough to put the evidence of present-day conditions out of court; and (ii) to continue with the "prosaic task of accumulating more cold facts". The second of these is the more essential because by no means all geophysicists deny the physical possibility of continental drift. In this connexion reference may be made to the following papers: D. Griggs, *Amer. J. Sci.*, 237, 611; 1939; and B. Gutenberg, *Trans. Amer. Geophys. Union*, 556–58 (1941).

Activation of Nitrogen in the Silent Electric Discharge

S. S. Joshi and A. Purushotham, of Benares Hindu University, describe, in a communication to NATURE, dated November 17, 1941, some work on the formation of active nitrogen in a Siemens' ozonizer. Both alternating and unidirectional potentials were used. The walls of the ozonizer were suitably coated, and an idle volume was connected in series with the exhaust end of the ozonizer and the pump to act as a gas reservoir. With careful adjustment of the applied field and gas flow to suit the gas pressure, activation of nitrogen in the *silent discharge* was observed up to a pressure of 150 mm. mercury, using soft and pyrex glass tubes. By raising the frequency of the A.C. supply, both the yield and the maximum pressure could be increased appreciably; to do this it was necessary to use ozonizer tubes made of materials, such as silica, of high dielectric strength in order to withstand more intense fields; the walls were silvered. Apart, perhaps, from 'controlled electron' sources (Caress and Rideal, *Proc. Roy. Soc.*, A, 115, 684; 1927), with which the yield of active nitrogen was limited by the low pressure, the *silent discharge* was found to be the simplest in technique and most suitable for determining the electrical quantities characteristic of the reaction under given conditions. With the frequency n, the gas pressure p, and the temperature T kept constant, it was found (Joshi, *Current Science*, 8, 548; 1939) that as the applied potential was progressively increased, activation of the gas to produce a just perceptible afterglow set in at a critical potential V_m characterized by a sudden increase in both the electric current and the energy dissipated in the system. With constant n and T the critical potential increased sensibly linearly with p, the increase being pronounced at lower frequencies; at constant p and T the critical potential diminished with increasing n in the range 50–500 cycles per sec. Changes in V_m produced by admixture of a foreign gas or 'impurity' serve markedly to reveal its influence as a catalyst or otherwise in the activation of nitrogen. V_m resembles the corresponding Paschen potential. It is considered that ionization of nitrogen in one or more stages is a chief determinant of changes that precede its activation.

Laboratory Test for Preservation of Iron by Paint

To determine how well a paint protects a metal surface against corrosion, plaques covered with it are usually exposed outdoors. A laboratory test has recently been developed which shortens the weathering time to hours and does not destroy the film. The test, the technique of which is described by R. B. Gibney (*Bell Lab. Rec.*, 20, No. 2, Oct., 1941), gives evidence as to how well a paint protects metal, but little information on the weathering of the paint itself. In the presence of water, metallic corrosion results from the electrochemical action between small adjacent surface areas, potential differences being set up between them. The potential of the whole surface is that of all the small areas involved, and can be measured with reference to a standard electrode. The extent of corrosion can be determined by recording continuously the potential changes of the metal over a few hours. Surface areas of a corroding metal are of two kinds: anodic (or electronegative), where the metal goes into solution, and cathodic (or electropositive) where hydrogen is evolved. If the metal corrodes freely the cathodic areas tend to become 'clogged' with hydrogen and the resultant potential of the plate becomes more electronegative. This effect is strongly accentuated when oxygen is excluded. If the metal does not corrode it is because the anodic areas become insulated with an extremely thin film of corrosion products, the potential of the plate being nearly that of the cathodic areas. Thus the potential of a corroding metal becomes more electronegative with time, while that of a non-corroding metal becomes and remains electropositive, and this is the basis for the rapid determination of the behaviour of painted iron.

Components of Fehling's Solution

IN a paper read before the Society of Public Analysts and Other Analytical Chemists on February 4, J. G. N. Gaskin described the curves obtained when the optical rotatory power (polarization) of mixtures of copper sulphate, alkali tartrate and caustic soda are plotted against increasing alkali contents of the solutions. Interpretation of these curves suggests the existence of copper complexes stabilized by alkali tartrate, and the isolation of one of these, having some unusual properties, is described. Some analytical data were given for compounds formed when copper sulphate is dissolved with excess of either Rochelle salt or potassium tartrate. The bearing of this work upon the constitution and possible modification of Fehling's solution is discussed.

Lithium Alum

THE existence of a lithium alum, $LiAl(SO_4)_2, 12H_2O$, has been the subject of some discussion, and the preparation of the compound is evidently a matter of some difficulty. J. F. Spencer and Oddie (NATURE, 138, 169; 1936) prepared it by working at low temperature, but H. A. Horan and Skarulis (*J. Amer. Chem. Soc.*, 61, 2689; 1939) failed to obtain it at 0°. H. A. Horan and J. J. Duane (*J. Amer. Chem. Soc.*, 63, 3533; 1941), however, now report that in equilibrium with saturated solutions an invariant point with solution composition differing from that previously reported by Horan and Skarulis has been found, and more careful investigation of the region between these invariant points clearly shows the existence of a third phase. The results indicate the formation of a double salt of composition $LiAl(SO_4)_2$, $12H_2O$. Apparently the use of concentrated solutions of purified aluminium sulphate was the only difference in technique. The existence of lithium alum, with a limited region of existence, thus seems to be established, but some care is evidently necessary in its preparation.

GREEN LEAVES AS A SOURCE OF PROTEINS AND OTHER NUTRIENTS

IT is natural that the restriction of imports, and the changes in our dietetic habits that this has necessitated, should quicken our interest in the efficient production of food in Great Britain. In a few quarters, notably the immediate vicinity of the Ministry of Food, it is maintained that there is not now, and is not likely to be in the later stages of the War, a food shortage. Fortunately for the country most scientific workers concerned with nutrition or agriculture do not share this complacent attitude. Their interest was clearly shown at the discussion organized on February 11 by the Nutrition Panel of the Food Group of the Society of Chemical Industry. The papers read dealt mainly with carotene and other lipoids; there was discussion on this subject and on a project that had been circulated beforehand dealing with the large-scale isolation of protein from leaves for the feeding of people and other non-ruminants.

The vitamin content of leaves was compared with that of various other plant products by V. H. Booth. When taken at the right time of year leaves constitute a valuable source of ascorbic acid and of carotene, but ascorbic oxidase is so widespread that it is unlikely that products derived from leaves would be of much value in practice as sources of ascorbic acid. Samples of lard containing enough carotene to give 640 I.U. per ounce (the amount officially added to margarine) were shown and also samples in which only partially fractionated grass fat had been added to give the same carotene content. The former were about as dark as normal margarine and the latter, though darker, were not unpleasantly so. It was clear, from this survey, that leaves are not of great significance as a source of any of the other vitamins except K.

During storage, the carotene content of ground-dried lucerne leaves falls. R. O. Davies described this phenomenon in some detail and pointed out that the loss was only significant if the leaf meal was not cooled after coming from the drying plant and before storage. The fall is not continuous but a steady state is reached after 1–2 weeks; A. C. Pollard, on the other hand, found a continuous fall in the carotene content of ground-dried carrot pulp. No wholly satisfactory explanation of this difference was given, but it was suggested that there may be more than one isomer of carotene present; that carotene may only be readily oxidized in those cells that have been thoroughly damaged by the grinding; that there may be a difference in the oxidizability of carotene in the free and protein-bound states. The other lipoids of the leaf were described by H. S. Jefferies. 4 per cent of dried lucerne leaves is soluble in benzene and half of this dissolves in acetone. Phosphatides constitute 35–37 per cent of the total lipoids—an unusually high phosphatide/fat ratio. Less than 3 per cent of the lipoid is sterol and there appears to be no ergosterol in lucerne although the lipoids from grasses have been shown to contain about 1 per cent.

In the contribution taken as read at the meeting[1] it had been urged that research should be undertaken on the separation and purification of leaf protein and on the cultivation of crops that would give a good yield of protein per acre coupled with a leaf that, in terms of its dry weight, was rich in protein. The most immediately available material for this research would be the normal pasture mixture, but special grass mixtures or crops such as lucerne or maize may well turn out to be the most suitable in practice. The fresh leaves would be macerated and a protein-rich liquor separated from the leaf residue; the protein can readily be separated from the liquor by a number of treatments such as heat coagulation. Experience gained in the course of work on the pigments, enzymes and viruses in leaves suggests that about one third of the protein can easily be liberated in this way; a rather larger proportion comes out if the original crude protein content of the leaf is high, that is, more than 18 per cent. The leaf residue would therefore still contain about two thirds of the protein as well as all the fibre. From 140,000 acres it should be possible to prepare sufficient leaf protein to supply 1 gm. per head of population every day for a year. The aim of the research would therefore be to 'skim' off part of the protein of the leaf in a form suitable for the non-ruminant while leaving a residue suitable for use as a feeding-stuff for ruminants. If the research were successful, several advantages are claimed for this method of husbandry. The yield of protein of high biological value would be higher than that attainable from an acre by any other means; the fibre residue would be of relatively constant composition and of considerably greater feeding value than the hay that would probably have been made on the land had it not been used for this purpose; there is good reason to think that leaf protein would cost less than the other proteins that can be produced in Great Britain.

A. C. Chibnall agreed with the general principle of leaf protein extraction, but thought that it would be premature to start any research on these lines until our agricultural policy had undergone thorough reorganization. Difficulties were predicted both in the operation of an acreage sufficient to make a significant contribution to our protein supply and in the maintenance of a high leaf protein content. These difficulties did not impress R. O. Davies who has experience of farming more than ten thousand acres of lucerne; for five months out of the year the crop contained 17–28 per cent of crude protein.

Figures for the amino acid composition of protein isolated from cocksfoot grass were given by A. C. Chibnall, and he pointed out that this fraction of the total leaf protein contains all the amino acids that are known to be essential in the nutrition of the rat, in about the right amounts. Estimations of some of the amino acids in proteins from the leaves of several plants suggest that these proteins form a group of fairly uniform composition and of high 'biological value'. The need for feeding experiments to supplement these analyses was stressed by S. K. Kon, for leaf protein fed in the form of dried grass had a lower 'biological value' than Chibnall's figures would lead one to expect. These experiments, as A. L. Bacharach pointed out, should not be confined to the rat, but should be extended to some of the animals in the nutrition of which we are more immediately interested.

N. W. PIRIE.

[1] N. W. Pirie, "The Direct Use of Leaf Protein in Human Nutrition", *Chem. and Ind.*, 61, 45 (1942).

PINE SHOOT BEETLES

An interesting account of pine shoot beetles has recently been published by Dr. Leon Ossowski (*Scot. Forest. J.*, 55. Part 2; 1941; Douglas and Foulis, Edinburgh). The author states that any forestry publication, British, Polish, German or Russian, is likely to contain an article on bark beetles and more especially the pine shoot beetles, which proves the destructive nature of these pests. In spite of many natural enemies the two pine shoot beetles are very plentiful, largely due, as Dr. Ossowski correctly states, to the usual technique of planting, "for planting concentrated in one year or on a single species gives a breeding ground for the beetles. Where we have natural regeneration and plantations of different ages and different species the beetles are rarely met with."

Equally correctly the author advocates that, in future, planting should not be confined to Scots pine, European larch and Norway spruce (33⅓ per cent of each) but that 30 per cent or even half of a plantation should consist of deciduous trees. It is an undoubted fact that in those temperate countries where considerable tracts of conifers either pure or mixed have been planted, sooner or later the areas have been visited by serious epidemics of pests—frequently the various bark beetles. There are others, for example, the plague of the pine noctuid (*Panolis piniperda* Panz) in Brandenburg and Pomerania in Germany which laid waste thousands of hectares of coniferous woods during 1921–25; or the great devastation in the spruce forests in the Vosges (1903–05) by bark beetles. In spite of the well-known examples of widespread attack and heavy losses the lesson is difficult to learn apparently—for the pure conifer plantation is still being planted on a wide scale in temperate climates.

The author discusses some of the well-known methods of combating attacks and incidentally affords an interesting side-light on his own country, Poland.

"A striking example of the motto 'Back to Nature' is given by the woodlands of my own country. While in West Poland the woods are mainly coniferous, those of the North and North-east Poland are of a more mixed type with natural regeneration. Between 1920 and 1939 there were in West Poland two attacks of pine noctuid (1922–1925 and 1931–1933), two of the pine moth (*Dendrolimus pini*) and the pine looper (*Bupalus piniaria*) and one of the nun moth (*Liparis monacha*). The damage done by the insects was enormous and, so far as I can remember, through the noctuid plague of 1922–25 about 250,000 hectares of coniferous plantations must have been ruined, and in 1931–33 it cost 3,500,000 zloty (£140,000) to combat the evil. In the forests of North and North-east Poland, on the other hand, there were no great disasters from insect pests to record, and if here and there destructive insects appeared the trouble was only local and the loss not very great. As already stated, mixed plantations and a combination of tree groups of different ages give the best defence against bark beetles and destructive insects in general."

Research and experience are showing that much the same general rule applies to the broad-leaved species of fine timbers in the tropical and sub-tropical forests. It is becoming equally plain that the ordinary practising forester (or agriculturist, for that matter) finds it difficult to appreciate the fact until he faces a disaster.

FORTHCOMING EVENTS

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

Saturday, February 28

ROYAL SANITARY INSTITUTE (at the Nuffield Institute of Clinical Research, Oxford), at 10 a.m.—Mr. H. H. Crawley: "The Storage of Emergency Drinking Supplies"; Mr. Stewart Smith: "Administration of the Government Evacuation Scheme".

NUTRITION SOCIETY (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 10.30 a.m.—Conference on "Food Production and Distribution in relation to Nutritional Needs". (Speakers: Sir John Orr, F.R.S., Dr. N. C. Wright, Mr. E. T. Halnan, and Sir John Russell, F.R.S.)

Monday, March 2

ROYAL GEOGRAPHICAL SOCIETY (at Kensington Gore, London, S.W.7), at 5 p.m.—Capt. G. S. Andrews: "Projects for the Alaska Highway Route".

Tuesday, March 3

ROYAL INSTITUTION (at 21 Albemarle Street, London, W.1), at 2.30 p.m.—Sir Lawrence Bragg, F.R.S.: "Metals", 1: "Metal Architecture".*

Wednesday, March 4

ROYAL SOCIETY OF ARTS (at John Adam Street, Adelphi, London, W.C.2), at 1.45 p.m.—Mr. Llewellyn S. Lloyd: "Modern Science and Musical Theory".

Thursday, March 5

TOWN AND COUNTRY PLANNING ASSOCIATION (in the Dome Lounge, Dickins and Jones, 224 Regent Street, London, W.1), at 1.20 p.m.—Rt. Hon. Sir Montague Barlow, Bart.: "Decentralization and the Location of Industry".

Friday, March 6

PHYSICAL SOCIETY (OPTICAL GROUP) (at the Science Museum, Exhibition Road, London, S.W.7), at 11.15 a.m.—First General (Business) Meeting; at 11.30 a.m.—Dr. W. M. Hampton: "Problems relating to Optical Glass"; at 3 p.m. (in the Small Physics Lecture Theatre of the Imperial College, Imperial Institute Road, London, S.W.7)—Mr. R. J. Bracey: "A Multi-Purpose Collimator"; Mr. J. W. Perry: "Thermal Effects on the Performance of Lens Systems".

SOCIETY OF PUBLIC ANALYSTS AND OTHER ANALYTICAL CHEMISTS (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 4 p.m.—Annual General Meeting; at 4.45 p.m.—Prof. J. W. Munro: "Entomology of Commerce".

INSTITUTION OF MECHANICAL ENGINEERS (at Storey's Gate, London, S.W.1), at 5.30 p.m.—Mr. P. J. Tams: "The Operation of the Mechanical Engineering Section of the Central Register in War Time".

Saturday, March 7

GEOLOGISTS' ASSOCIATION (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 2.30 p.m.—Mr. W. P. D. Stebbing: "A Review of References to Geology and Allied Subjects from the 16th Century" (Presidential Address).

APPOINTMENTS VACANT

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

GRADUATE LECTURER IN ENGINEERING—The Principal and Clerk to the Governing Body, Wigan and District Mining and Technical College, Wigan (March 7).

LECTURER IN CIVIL AND MECHANICAL ENGINEERING—The Secretary, Northampton Polytechnic, St. John Street, London, E.C.1 (March 16).

SCIENTIFIC ASSISTANT (woman not liable for National Service)—The Deputy Director, Imperial Bureau of Pastures and Forage Crops, Agricultural Research Building, Penglais, Aberystwyth (March 21).

PROFESSOR OF EDUCATION—The Registrar, University College of Swansea, Singleton Park, Swansea (April 11).

ASSISTANT MECHANICAL ENGINEER for the Electrical Branch of the Nigerian Government Public Works Department—The Secretary, Central Register, Ministry of Labour and National Service, Queen Anne's Chambers, London, S.W.1 (quoting reference O.N.C.793).

REPORTS and other PUBLICATIONS

(*not included in the monthly Books Supplement*)

Great Britain and Ireland

Proceedings of the Royal Society of Edinburgh. Section B (Biology). Vol. 61, Part 2, No. 14: Quantitative Characters of the Growth and Development of a Paurometabolous Insect, *Dixippus (Carausius) morosus* Br. et Redt. 1: The Loss of Water in relation to Ecdysis. By Dr. Beverley N. Smallman. Pp. 167–185. 1*s.* 6*d.* Vol. 61, Part 2, No. 15: A Simple Apparatus for the Direct Determination of the Volume of Small Irregular Objects. By Dr. Beverley N. Smallman. Pp. 186–187. 6*d.* Vol. 61, Part 2, No. 16: Observations on Artificially Induced Heat in Immature Guinea Pigs. By Dr. P. Bacsich and Dr. G. M. Wyburn. Pp. 188–196+2 plates. 9*d.* (Edinburgh and London: Oliver and Boyd.) [52