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CONTENTS

| | Page |
|---|---|
| Labour and Management in Reconstruction | 559 |
| Speculations in Immunology. By Prof. A. A. Miles | 562 |
| Logical Foundations of Psycho-analysis. By Dr. John Cohen | 563 |
| Materials and Structures | 563 |
| Work of Pierre Duhem. By Dr. H. Jeffreys, F.R.S. | 564 |
| Mainly on Wild Geese. By Seton Gordon, C.B.E. | 564 |
| Radioactivity and the Completion of the Periodic System. By Prof. F. A. Paneth | 565 |
| Mosquito-breeding in Static Water Supplies. By J. F. Marshall, C.B.E. | 568 |
| Science and War. By Austin H. Clark | 570 |
| Statistical Control of Production. By Dr. C. G. Darwin, F.R.S. | 573 |
| Obituary : | |
| Mr. W. P. Pycraft. By Sir Arthur Smith Woodward, F.R.S. | 575 |
| News and Views | 576 |
| Letters to the Editors : | |
| Cosmic Rays and Magnetic Storms.—Dr. A. Duperier | 579 |
| Dispersion of Cellulose Strands in Cell Walls.—Dr. R. D. Preston | 580 |
| Effect of Certain Chemotherapeutic Agents on Experimental Eye Lesions produced by *Staphylococcus aureus*.—Dr. J. M. Robson and Major G. I. Scott | 581 |
| Microbial Synthesis and Autolysis in the Digestive Tract of Herbivora.—Frank Baker | 582 |
| Spore Dispersal in the Mucorales.—Dr. C. G. Dobbs | 583 |
| Sea Waves : Their Growth and Subsidence. By P. J. H. Unna | 584 |
| New Seismograph Station at Logan, Utah | 585 |

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# LABOUR AND MANAGEMENT IN RECONSTRUCTION

THE Labour Party has shown both imagination and vision in the interim report on reconstruction problems in war-time issued early in March under the title "The Old World and the New Society"* for discussion at a series of regional conferences and at the annual conference of the Labour Party being held at Whitsuntide. It seems to have grasped the relation of reconstruction to the war effort and puts the point as forcibly as it has been put by Sir Stafford Cripps, and in a brilliant introductory note to a recent P E P broadsheet on bodies in Great Britain considering reconstruction problems. In detail and in broad outlook, the report is evidence that the Labour Party is facing up to the problems of the post-war world, and the whole spirit of the report justifies the hope that an attack on the problems of post-war reconstruction may be possible on non-party lines.

Any such prospect is sure of the attention of scientific workers if only for the wider possibilities it would offer of utilizing their services in the solution of such problems and the establishment of the four freedoms. Their interest in the present report is already claimed by its reference to the importance of research and to the need for a more creative attitude on the part both of Government and of industry to science and scientific research. The report calls for the conscious and deliberate endowment of scientific research, both pure and applied, the financial responsibility for which shall be shared by industry and the State. There is no field of economic life in which the possibilities of scientific advance are not immense. Many of them are arrested or withheld from enjoyment by the motives inherent in a profit-making society, and while it may take time to liberate such potentialities from the present restricting forces, the necessary endowment of scientific research should not be jeopardized.

In the same vein, the report directs attention to the need to apply in the work of reconstruction some of the important lessons of management and industrial welfare that have been learned or reinforced during the War. The widespread failure to apply the knowledge acquired by the Industrial Health Research Board and its predecessor twenty or more years ago has been profoundly discouraging to many scientific workers. Their support should be assured for any move to secure the fuller utilization of the experience which is now being accumulated on such matters as communal feeding in factories, the organization of medical inspection and care as a normal function of management, the evolution of factory discipline based on consultation and co-operation rather than on coercion, the full use of industrial psychology in testing fitness for the job, the selection for executive posts in terms of tested competence and the proper planning of factory accommodation.

The importance of wise management, utilizing the scientific techniques which are now at its disposal as

* The Old War and the New Society : a Report on the Problems of War and Peace Reconstruction. Pp. 32. (London : The Labour Party, 1942.) 3*d*.

well as the increasing store of factual knowledge as a basis for policy and decisions, in reconstruction as well as in furthering the war effort, is not easily overstressed. It is one indispensable factor in removing friction and misunderstanding between management and worker and developing the spirit and practice of co-operation upon which efficiency depends. The seventh and eighth reports of the Select Committee on National Expenditure afford sufficient evidence on this point, and they show also that co-operation involves a contribution from the workers themselves. A new outlook and an unprejudiced and forward-looking mind are demanded of labour as well as of management in order that the national effort for winning the war and building the peace may attain its full power and efficiency.

The hope that full co-operation between management and labour may yet be achieved and that the problems of post-war reconstruction may be approached untrammelled by the prejudices of party politics is encouraged by the views expressed almost sumultaneously by Mr. Bevin and Mr. Samuel Courtauld. Referring particularly to problems of demobilization after the War, Mr. Bevin stressed the importance of closer co-operation between workers and employers and the inevitability of greater State control. The fatal error would be to allow the desire for quick profits or the disposition towards rapid and undue expansion to distort post-war development into a disastrous boom. The controls which have apportioned resources in war economy must be maintained in order to distribute those resources in the most beneficial manner after the War ends. There must be a great national discipline as keen and efficiently imposed as that which has grown up during hostilities, until the problems of re-settlement have been worked out.

Clearly Mr. Bevin is prepared to face the unpopularity which may accompany the attempt to guide the nation along a difficult path which promises permanent recovery rather than a temporary boom. The twitterings which Sir William Beveridge's stimulating article in *The Times* has caused in the strongholds of individualism and *laissez-faire* are token of the opposition that will be forthcoming from the recalcitrant and backward-looking minority who find their private interests threatened. Mr. Samuel Courtauld's article in the *Economic Journal* of April shows, however, that there are leaders in industry as in science who are facing the future with unprejudiced and receptive minds.

Mr. Courtauld's article makes particularly interesting reading in the light of the Labour Party's report. Much of the ground covered is common, though the Labour Party's report includes colonial and international affairs in its survey. Mr. Courtauld, on the other hand, states the questions rather than gives the answers, and if he seems tentative, he is free from the flavour of dogmatism which at times creeps into the Labour Party's report.

When we have reached agreement as to the questions to be asked, we are entitled to expect some measure of co-operation in the search for the answers, and there is a striking resemblance in this respect in the programme outlined by Mr. Courtauld and by the Labour Party's report, and also by Prof. Carr in his chapter on "Britain at Home" in his recent book, "Conditions of Peace". Mr. Courtauld believes that the time is ripe for a review of the principles and bases of industry, and he points to the acceptance by industrialists to-day of the inevitableness of change, to their conscious respect for British ideals and the progressive gains of the past in which all classes have had a hand. There is also a re-awakening of conscience: an industrial career is now a *métier*, not merely a road to private acquisition. Fourthly, he notes a growth of real fellow-feeling with the lower ranks of industry, who have come to share the sensibilities and aspirations of those placed above them.

For all this, the leaders of industry are not likely to be stampeded into throwing everything overboard, for they have a firm faith in many enduring principles without which there is no possibility of sound rebuilding. There are many passages in the Labour Party's report which indicate at least some appreciation of this position, and it would be true to say that most, if not all, of Mr. Courtauld's assumptions are implicit in its own argument. Mr. Courtauld assumes, for example, that production is the most vital activity of the nation and that Government control has come to stay. No Government, he says, can tolerate the existence within its borders of an organized and completely independent power with a radius of action as wide as its own, and the same overriding principle that makes it the duty of the Government to control industrial combination applies to trade unions as developed to-day. Government bureaucracy, he holds, as it exists in Great Britain, has proved itself incapable of conducting business efficiently; it would require drastic reform to enable it to take any part in the running of business.

Mr. Courtauld assumes that labour will increasingly share in the management policy and rewards of industry and that the trade unions will long continue to represent labour. Gambling in industrial counters is a malignant disease for industry itself. The English genius for social evolution and for compromise can find a middle way between pure individualism and pure socialism which will bring the greatest attainable good to the nation and leave more and more vested interests out in the cold. Furthermore, he draws a distinction between productive industry and financial institutions which many scientific workers have voiced before, and his views on advertising and patent law as well as on financial institutions are tolerably certain to be the subject of lively debate.

We might well hope that these fundamental assumptions are sufficiently shared by the labour side of industry to induce a common approach to the broad questions bearing on the relations of Government and industry which Mr. Courtauld puts without always suggesting an answer. Fixed policy and the choice between free trade and protection, with the allied question of subsidies; Government ownership and control and the method of exercising control, with the relation between industrial representation and the Government; the rights of labour, with such allied questions as their voice in control and management, family allowances, security and wages; industrial planning, both geographical and human, including technical education; the protection of consumers; and the control of speculation—these are all broad questions on which it is for the nation, and not industry itself, to indicate in advance the main lines of the policies concerned.

On all these topics Mr. Courtauld offers pertinent comments and suggestions, as well as on the highly technical questions, similarly calling for a national lead in policy, such as the taxation of industry, limitation of profits, depreciation and research expenditure; equalization of rates; patent law; and whether selling and distribution cost too much to-day. It is true that on certain points he is somewhat nebulous. He recognizes the importance of retaining the private initiative which fosters economic progress, and at the same time wishes to make Government control in the interests of the community effective. The solution he offers, namely, the appointment of Government directors, is nevertheless scarcely adequate.

The key problem of post-war economic policy is, however, fairly raised. Industrial management must be efficient; this cannot be achieved by Civil Service formulæ, and there must be communal control. The solution may well lie first in the training and evolution of a new type of Civil Servant competent in industrial and economic affairs, and second, and more important, as Mr. Geoffrey Crowther insists, in drawing a sharp distinction between those essential industries, such as food, transport, and probably fuel, which must be managed directly under the eye of the community, and those which must be permitted to develop within the usual pattern of private enterprise but also within the pattern of a decent, civilized and democratic society.

Mr. Courtauld's article is stimulating and refreshingly free from prejudice. His observations on the planning of the human side of industry as well as its geographical aspects are welcome, and it is encouraging to find an industrial leader openly expressing the opinion that no business should be allowed to employ more juveniles than it can absorb later into the ranks of full-grown workers. The elimination of blind-alley occupations is a task that calls imperatively for the closer co-operation of Government, employers and labour. It is a matter on which scientific workers may well be expected to make their own contribution, and Mr. Courtauld's observation that care should be taken not to educate people into blind alleys is one that teachers, too, should note. The comment that colleges and technical schools pay insufficient attention to their responsibility for relating the number of pupils taught to the probable demand for them is fair, if it is true that quantitative training and recruitment imply willingness on the part of the employer to forecast and estimate his requirements.

By and large, much of Mr. Courtauld's article finds its counterpart in the Labour Party's report. Recognition of the inability of an unplanned society to maintain a reasonable standard of life for many of its citizens is common ground, as are the consequences that flow from unregulated private enterprise—the restriction of production, waste of skilled man-power and failure to use the achievements of science and technology to raise the standard of life. Implicit in both Mr. Courtauld's paper and the Labour Party report is the acceptance of the objectives stated by President Roosevelt and of the establishment of the four freedoms; indeed, the whole of the Labour Party's report shows that the party has grasped the truths outlined so ably by Prof. Carr in "Conditions of Peace", and that it recognizes the strength which a reconstructive policy will lend to the war effort. Democracy cannot develop its full power in winning the War unless we begin during its course to organize the conditions which will ensure that when victory is won the great ends of life will be effectively served for every citizen.

The right note of caution is sounded both in this report and in Mr. Courtauld's address. The first aim is to win the War, and any measures now adopted must be justified first by their contribution to victory. At the same time, unless opportunities that now present themselves of destroying rather than consolidating abuses and obstructions are used, it is useless to talk of rebuilding industry or society on better foundations.

While, however, there is much indeed in the report that indicates that the Labour Party is prepared to face questions of industrial organization and the like in a new spirit and with an open mind, one grave defect mars the report. It is common ground in almost every impartial analysis of the new society that is to be established after the War that stability will depend on stress being laid on obligations rather than on rights, on services rendered to the community rather than on benefits to be drawn from it. A social order in which the four freedoms of President Roosevelt are to be embodied in a social minimum for every citizen must have as its counterpart a standard of duties and obligations which are expected of all.

It is this note that is lacking in an otherwise admirable statement. No united attack on the problems of post-war reconstruction, no non-party approach is possible in its absence. If the emphasis falls exclusively on benefits and rights, no democratic order will be established wide enough to resist the renewed onslaught of authoritarian regimes when the present aggressors have been overthrown. The essential task is to transmute into the service of peace and the building of a new order of society, that spirit of comradeship, of unselfish and strenuous endeavour that the very preservation of democracy now demands. If in the discussions that ensue on the basis of this report this note of service and duty is firmly sounded, as the open-mindedness of the report warrants the hope, it should not be vain to look further for that united attack on the problems ahead of us, free from prejudice and party restrictions, in which the resources and skill of scientific workers among others can be fully and effectively deployed, to the lasting benefit of mankind.

# SPECULATIONS IN IMMUNOLOGY

## The Production of Antibodies

A Review and a Theoretical Discussion. By Dr. F. M. Burnet, with the collaboration of Mavis Freeman, A. V. Jackson, Dora Lush. (Monographs from the Walter and Eliza Hall Institute of Research in Pathology and Medicine, Melbourne, No. 1.) Pp. viii+76. (Melbourne and London: Macmillan and Co., Ltd., 1941.) 8*s*. 6*d*. net.

THE introduction of complex foreign substances of high molecular weight into the tissues of warm-blooded animals often leads to the appearance of antibodies in the blood serum. The reaction of these antibodies with the substance that stimulated their appearance—the antigen—is largely specific. Immunologists have in the main confined their attention to the specificity of combination between antigen and corresponding antibody. Analysis of natural antigens and the artificial modification of protein antigens by the addition of active chemical groups to the surface of the molecule have both proved fruitful in revealing what particular configurations in the antigenic particle determine its power to induce and to combine with specific antibody. Much less is known about the origin and structure of antibodies themselves. A single injection of antigen sometimes yields a detectable amount of antibody, but its most striking effect is to sensitize the animal in such a way that a subsequent injection induces a rapid and disproportionately large output of antibody. Upon the readiness of this 'secondary response' depends the persistence of immunity in man and animals; though all detectable protective antibody has disappeared, the antibody response of the conditioned animal to later contact with antigen is immediate and effective.

Antibodies are modified serum globulins, and it appears that antibody globulin is produced in the cells of the reticulo-endothelial system, and perhaps in lymphoid tissue. In the absence of an antigenic stimulus these cells presumably synthesize the normal globulins. We have little knowledge of the origin of the normal serum proteins, and are consequently unable to speculate very profitably about that of the modified antibody globulin. The current hypothesis is an extension of Fischer's lock-and-key analogy for the specificity of combination between an enzyme and its substrate. It supposes that the amino acids destined for synthesis into globulin are specially disposed in a cell containing antigen; they are held in a pattern determined by the nature and arrangement of the active groups on the surface of the antigenic particle, and in this pattern are incorporated into the globulin molecule, which thus bears the specific impress of the antigen.

Dr. Burnet rejects this view of the antigen as a 'template' for antibody production. Not only does the type of antibody induced by a single antigen change as immunization of the animal is prolonged, but also the animal may still synthesize antibody when it is reasonable to suppose that the injected antigen has been destroyed and the cells originally stimulated by it have been replaced by their more or less remote descendants. Moreover, the 'secondary response' is often characterized by a sharp exponential rise in the curve of antibody production, which suggests a proliferation of the antibody-forming mechanism, rather than a rapidly accelerating production from an already established set of 'templates'.

As an alternative, Dr. Burnet in the first place postulates that the serum globulins are products of self-reproducing intracellular proteinases, and represent a stage in the synthesis at which the molecule has the essential structure of the enzyme itself, and is consequently stable. In this form it is presumably turned out in the circulation; in the cell, the proteinase reproduces itself fully. A similar conception has been advanced for the multiplication of plant viruses, but there is no valid reason why it should not apply to syntheses in normal cells. In the next place, it is postulated that these proteinases, in virtue of their enzymic function, come into contact with any foreign antigens taken into the cell, and are lastingly modified by this contact. There is no synthesis of a new unit in spatial contact with the antigen (the 'template' view) but a change in the atomic structure of the proteinase that allows its effectively hydrolytic action on the foreign antigen. When the antigenic stimulus is removed, the modified proteinase continues to synthesize itself, both in the original cell and its descendants, but there is a slow reversion to the original type, and the antibody gradually disappears from the circulation. A second injection of the antigen is followed by a rapid hydrolytic action by the specifically modified proteinase, which yields products of partial hydrolysis that in turn modify the proteinase further, and so lead to progressive change in the nature of the antibody.

Speculative hypotheses of this kind serve two useful purposes. They may direct attention to the facts they purport to co-ordinate, and they may link up the field of speculation with hitherto unrelated fields of science. Dr. Burnet's speculations serve the second purpose less happily than the first. As an example, we may consider the modification of the self-reproducing proteinase by the antigen. We may assume that the proteinase exists, and that globulin is not formed by a more complex synthetic unit which is in effect self-reproducing. But the justification for the type of modification postulated rests on doubtful analogies with adaptive enzymes, and with the training of a bacterial culture to utilize substances that are not normal participants in its metabolism. The adaptive enzyme, that becomes evident when bacterial cells are brought into contact with its corresponding substrate, bears a superficial resemblance to antibody in that it is specifically called into being by the substance on which it acts, but there is little evidence to support Dr. Burnet's suggestion that it represents a modification of an existing constitutive enzyme. So far as is known, a bacterial species possesses a finite range of adaptive enzymes, and though peculiar conditions are required for their synthesis, they characterize the species as surely as the constitutive enzymes.

The analogy with trained bacteria is even more remote. We are dealing here with an adaptation that in many cases may fairly be considered as the restoration of synthetic powers lost in the evolution of a bacterial species from an autotroph to a heterotroph, and not a calling forth of a new and abnormal activity, for there is ample evidence to suggest that heterotrophs may retain synthetic powers which are not ordinarily manifest because they are quantitatively insufficient for the full needs of the organism.

The review is wide, and includes original work by the author and his colleagues, particularly on the formation of antibodies in lymph nodes, and on the logarithmic phase of the secondary response. Dr. Burnet's fondness for the pursuit of remote and sometimes misleading analogies may be overlooked, for his speculations, backed by the experimental facts that he has marshalled, will admirably serve the purpose of challenging immunologists to explain those aspects of antibody formation that are inadequately covered by current hypotheses.

A. A. Miles.

## LOGICAL FOUNDATIONS OF PSYCHO-ANALYSIS

### Psychoanalytical Method and the Doctrine of Freud

By Dr. Roland Dalbiez. Vol. 1: Exposition. Translated from the French by T. F. Lindsay. Pp. xvi+415. Vol. 2: Discussion. Translated from the French by T. F. Lindsay. Pp. xii+331. (London, New York and Toronto: Longmans, Green and Co., Ltd., 1941.) 40*s*. net.

It is fitting that a critical survey of Freud's writings should appear at this time. Insistent questions are provoked by the War. We witness the spectacle of a civilized people reduced to barbarism within a few years and wonder how this is possible. Are wars, it is often asked, simply the outcome of unconscious aggressive impulses? How deep is the impress that civilized living makes upon the psyche? Are humane dispositions wholly part of ontogenesis, and can no such dispositions be transmitted genetically? To what extent can re-education in the post-war world nullify the effects of totalitarian propaganda instilled during the impressionable years of youth, and what are the limits of variation in conduct that the same individuals may express under diverse conditions? No satisfactory answer to these and other urgent questions can be attempted without enlisting the aid of psycho-analysis. Indeed, it is probably true to say that without psycho-analysis, contemporary social behaviour is unintelligible and no coherent picture of human failings and aspirations in the modern world can be drawn without invoking its theories, provisional though they may be.

The foundations of psycho-analysis still lie in the writings of Freud. The reader of Dr. Dalbiez's work should therefore be led to the conclusion that the task of disentangling Freud's analytic technique from the theory with which it is interwoven and of subjecting both technique and theory to close examination were well worth undertaking, and that Dr. Dalbiez has acquitted himself with rare competence. He brings to psycho-analysis a standard of logical demonstration that has long been painfully lacking in this field. Several chapters, particularly the one entitled "The Various Neuroses", are noteworthy for their qualities of lucid exposition.

A specially useful contribution is the systematic attempt to relate the work of Freud to that of Pavlov. This effort to unify two independent hypotheses formulated in very different terms meets with a large measure of success. Even the much-disputed Œdipus conflict is readily translated into the language of reflexology, and is shown to result from a clash between the processes of excitation and inhibition, the genital reflex mechanisms failing to differentiate between two similar stimuli. Dr. Dalbiez maintains that the development of the sexual impulse reveals itself in a progressive differentiation of object. This view is presented as more in accordance with the facts than is the Freudian notion of sexual development as a succession of perverse phases culminating in normal heterosexuality. The belief that sexual development proceeds by integration of separate responses is in line with recent experiments on aggressive impulses in certain animal species.

Mention should be made of the discussion on symbolism, which clears up many confusions in the literature. In particular, the retrospective, reductive, causal interpretation of symbolism which is adopted by Freud is contrasted with the prospective, synthetic, teleological view of Silberer.

A number of criticisms rise to mind. The size of the second volume could have been much reduced; its matter is scarcely commensurate with its bulk. One may blame the philosophic training of the author for a number of purely academic discussions. The chapter dealing with the psychoses in the first volume can scarcely be regarded as having achieved its aim. About a third of this chapter is devoted to a criticism of Dr. de Clérambault's theories of mental automatism, an allocation of space which is surely unmerited. It is disappointing to find that aggressive behaviour is not accorded due recognition. The word 'aggression' does not even appear in the index. The world of Dr. Dalbiez seems to be the peaceful world of the philosopher, and one would gather that Freud attached little or no importance to aggressive impulses—a deduction which would be very misleading.

John Cohen.

## MATERIALS AND STRUCTURES

### Materials and Structures

By D. A. R. Clark. Pp. xii+384. (London, Glasgow and Bombay: Blackie and Son, Ltd., 1941.) 25*s*. net.

### Practical Design of Simple Steel Structures

By Dr. David S. Stewart. Vol. 1: Shop Practice, Riveted Connections and Beams, etc.; a Text-Book suitable for Civil Engineers, Structural Engineers, Road and Railway Engineers, and Students at Universities and Technical Colleges. Second revised and enlarged edition. Pp. xv+184. (London: Constable and Co., Ltd., 1941.) 14*s*. net.

The first of these two books has been written to meet the needs of students preparing for examinations in strength of materials and structures such as those in Part I of the University of London degree, Higher National Certificate and engineering institutions' syllabuses. These demand knowledge of the calculus, and the book assumes a similar standard. It is therefore disconcerting to find several pages given up to the working out in detail of the formulæ for the moments of inertia of such simple sections as form the most elementary exercises in integration. If the reader requires to be shown the detailed operations in the case of these geometrical sections, by how much the more must he need some light on the methods by which practical sections can be evaluated. From this point of view the author has not gone far enough to satisfy the needs of students at the stage indicated.

The pattern of the book is determined by its devotion to the subject of structures, and this gives it a distinct character. Instead of ranging over the aspects of strength of materials which appertain to machines, it confines itself to those concerned with structures and its examples are drawn from this branch of engineering. It is therefore to the structural engineering student in particular that it will appeal and whom it will most greatly help. He will find it a valuable medium of preparation for his profession as well as for the more immediate needs of examinations.

Following a very able presentation of the behaviour of materials under stress, there is a useful chapter on the production of metals and on the various treatments by which steel, in particular, can be improved or developed in certain desired directions. Besides familiarizing the student with the metals, this will assist him in understanding their specifications. A lengthy chapter on testing contains much information regarding the recognized tests and test-pieces, and describes various testing machines and appliances, the text being supplemented by diagrammatic sketches illustrating their principles. The latter part of the book is devoted to structures, and treats of framed structures, influence lines, deflections, suspension chains and bridges, and finally masonry. As a concise and educative statement of the theory of structures, this section can be confidently recommended to the student to whom the subject is as yet unfamiliar.

In contrast to the volume just described, Dr. Stewart's book deals with the practical design of some of the more simple structural details. In its earlier chapters it provides information regarding the rolled sections used, drawing office practice and procedure in relation to estimates and tenders, specifications and the ordering of material—all extremely useful information to the uninitiated. Equally so is the description of the template loft, the works and the machines and processes which are to be seen there.

The designs which are dealt with in this first volume of the series are of simple riveted fastenings and splices for various purposes and of beams. The complete design of a 20-ft. span gantry girder forms the tail-piece. As compared with the earlier edition, several new features have been introduced in the form of amendments to bring the text into line with the latest specifications, fresh material has been added, and an innovation is made in the arrangement of the text on the final design so that the calculations and explanatory notes are kept apart on opposite pages. thus giving a greatly improved presentation of the subject in its two aspects, instruction and example.

## WORK OF PIERRE DUHEM

### The Methodology of Pierre Duhem

By Armand Lowinger. Pp. ix+184. (New York: Columbia University Press; London: Oxford University Press, 1941.) 15*s*. 6*d*. net.

IN a prefatory note the author states that his aim is to provide a summary in English of the work of Prof. Pierre Duhem (1861–1916) on scientific methodology. The greater part of the book consists of extracts from his papers and from his book "La Théorie Physique", published in 1906 and 1914, with connecting passages supplied by the author. In the last chapter the author offers some criticisms.

It is rather difficult to assess the value of a book published in these conditions. At the present time there is little in it that is not familiar to workers in the subject, many of whom will emphatically accept it and others equally emphatically reject a great deal of it; to them the only question will be whether Duhem anticipated work that has been generally attributed to others. It does not appear to me that he did. His approach is substantially that of Mach, except that he does not wholly reject metaphysics; he only draws a line distinguishing it from science. But this distinction involves the rejection of "models" and explanations from science; he insists on abstract formalism. I should say that the latter is very good if it can be done, but before accepting it as a general principle I should like to see the facts of geophysics represented without the aid of the model that we call 'the Earth'. He accepts Mach's view that the whole aim of scientific theories is economy of statement. He agrees that we make inferences from them beyond the original data, and expect the results to be verified, but says that this has no logical justification and is not science. I should say, so much the worse for such a narrow logic.

There is no mention in the book of Karl Pearson's "Grammar of Science", published in 1892, which does face this problem; and I consider that any later work on methodology that omits reference to the "Grammar" is like one on gravitation that omits Kepler's laws or one on the theory of the complex variable that omits Cauchy's theorem. Duhem actually insists at a much later stage that we must accept testimony; but we could not even arrive at the meanings of words without the process that he has rejected.

H. JEFFREYS.

## MAINLY ON WILD GEESE

### Through the Air

Adventures with Wild Fowl, and Small-boat Sailing. By Michael Bratby and Peter Scott. Pp. 128+21 plates. (London: *Country Life*, Ltd., 1941.) 10*s*. 6*d*. net.

THIS is a reprinted account of a series of broadcasts which Michael Bratby has made, mainly upon wild geese, and there are a number of black and white illustrations by his friend Peter Scott. There are also two excellent photographs of a pink-footed goose on her nest, evidently in Iceland or Greenland, and a magnificent photograph of an Arctic hare. Unfortunately, we are left in the dark as to who took these outstanding photographs. There are two very good photographs (facing pp. 57 and 60), taken, we are told, in Icelandic waters. Judging from the icebergs the climatic conditions that season in Iceland must have been exceptionally severe, and the conditions recorded resemble, rather, Greenland or Spitsbergen. There is a most interesting account (pp. 29, 30) of a flight of blue snow geese descending on the water on migration in dense mist and being swept over Niagara Falls. Most were killed during that tremendous descent, but a few survived, and were brought to England.

Another interesting record (pp. 66–68) is of a pink-footed goose which had been shot and slightly 'winged'. This bird became very tame, a mate was provided for her, and seven years after she was 'winged' the goose nested in a flower bed in the garden and reared a family. There she lived for twelve years, and then, as circumstances had altered for her human friends, she was given away. She was less happy in her new surroundings, and being full-winged, joined a skein of wild pink-footed geese, and was not seen again.

By the way, the author is wrong when he writes of the wings of the lesser black-backed gull (p. 80) as being "as black as coal". The wings of the British lesser black back are soot-grey, but the wings of the Scandinavian lesser black back are black.

SETON GORDON.

# RADIOACTIVITY AND THE COMPLETION OF THE PERIODIC SYSTEM*

By PROF. F. A. PANETH
University of Durham

ONE of the tributes paid by the Government and men of science of the U.S.S.R. to the memory of Mendeléeff on the occasion of the centenary of his birth[1] was the publication of his "Collected Works". It contains all the different tables of the Periodic System designed by Mendeléeff at various times, and so offers an excellent opportunity to study at what pace, and up to what point, the completion of the system proceeded during his life. In the first tables, which appeared around the year 1870, empty spaces were left for the elements eka-boron, eka-aluminium and eka-silicon; for eka-, dwi-, tri-, and chatur-manganese; for the rare earth elements between barium and tantalum (many of which were known but could not be assigned to definite places); for several elements between bismuth and uranium; and, to complete the last horizontal row, for a few trans-uranium elements. The atomic weight of tellurium was assumed to be 125, instead of the experimental value 128, in order to make it smaller than that of the following element iodine, 127. All these statements were equivalent to predictions. Which of them did the author see fulfilled?

In 1886 Mendeléeff himself answered this question in a very original way. He had four photographic portraits mounted in a common frame, with the autographs of the persons represented and his own commentary on the back. (The second volume of the "Collected Works" contains a reproduction of this highly interesting historic piece.) The four men and their contributions to the development of the Periodic System, as explained by Mendeléeff, were: Lecoq de Boisbaudran, discoverer in 1875 of gallium (eka-aluminium); L. F. Nilson, discoverer in 1879 of scandium (eka-boron); Cl. Winkler, discoverer in 1886 of germanium (eka-silicon); and the Czech chemist B. Brauner, who had greatly pleased Mendeléeff by confirming by a re-determination of the atomic weight of tellurium that it actually was 125, as expected by Mendeléeff.

When in 1905, reviewing the question, Mendeléeff prepared for the last time a table of the Periodic System, scandium, gallium and germanium were filling the lowest three of the formerly empty spaces, but still none of the higher homologues of manganese could be added, and the placing of the rare earths had made but little progress; there were now indeed twenty instead of nineteen spaces available between barium and tantalum—thanks to the addition of the rare gases as Group 0 of the System, the only really important development between 1886 and Mendeléeff's death in 1907. The atomic weight of tellurium was now assumed to be equal to that of iodine; Mendeléeff apparently persistently refused to believe that it was even higher (as proved by the best atomic weight determinations), although Brauner himself had admitted that his earlier low value was an experimental error. The last horizontal row shows radium in its proper position and so points to a future development—the placing of the other radioactive elements and hence the conception of isotopy—which Mendeléeff did not live to see.

PERIODIC CLASSIFICATION OF THE CHEMICAL ELEMENTS

| Period | Group | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 |
| I | | | | | | | | | | | | | | | | | 1 H<br>1·0080 | 2 He<br>4·003 |
| II | 3 Li<br>6·940 | 4 Be<br>9·02 | | | | | | | | | | | 5 B<br>10·82 | 6 C<br>12·010 | 7 N<br>14·008 | 8 O<br>16·0000 | 9 F<br>19·00 | 10 Ne<br>20·183 |
| III | 11 Na<br>22·997 | 12 Mg<br>24·32 | | | | | | | | | | | 13 Al<br>26 97 | 14 Si<br>28·06 | 15 P<br>30·98 | 16 S<br>32·06 | 17 Cl<br>35·457 | 18 A<br>39·944 |
| IV | 19 K<br>39·096 | 20 Ca<br>40·08 | 21 Sc<br>45·10 | 22 Ti<br>47·90 | 23 V<br>50·95 | 24 Cr<br>52·01 | 25 Mn<br>54·93 | 26 Fe<br>55·85 | 27 Co<br>58 94 | 28 Ni<br>58·69 | 29 Cu<br>63·57 | 30 Zn<br>65 38 | 31 Ga<br>69·72 | 32 Ge<br>72·60 | 33 As<br>74·91 | 34 Se<br>78·96 | 35 Br<br>79·916 | 36 Kr<br>83·7 |
| V | 37 Rb<br>85·48 | 38 Sr<br>87·63 | 39 Y<br>88·92 | 40 Zr<br>91·22 | 41Nb<br>92·91 | 42 Mo<br>95·95 | 43 — | 44 Ru<br>101 7 | 45 Rh<br>102·91 | 46 Pd<br>106·7 | 47 Ag<br>107·880 | 48 Cd<br>112·41 | 49 In<br>114 76 | 50 Sn<br>118·70 | 51 Sb<br>121·76 | 52 Te<br>127·61 | 53 I<br>126·92 | 54 X<br>131·3 |
| VI | 55 Cs<br>132·91 | 56 Ba<br>137·36 | 57–71<br>Rare Earths† | 72 Hf<br>178·6 | 73 Ta<br>180·88 | 74 W<br>183·92 | 75 Re<br>186·31 | 76 Os<br>190·2 | 77 Ir<br>193·1 | 78 Pt<br>195·23 | 79 Au<br>197·2 | 80 Hg<br>200·61 | 81 Tl<br>204 39 | 82 Pb<br>207·21 | 83 Bi<br>209·00 | 84 Po<br>210 | 85 — | 86 Rn<br>222 |
| VII | 87AcK<br>223 | 88 Ra<br>226·05 | 89 Ac<br>227 | 90 Th<br>232·12 | 91 Pa<br>231 | 92 U<br>238·07 | 93 — | | | | | | | | | | | |

43
61
85
87

† Rare Earths.

| | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| VI<br>57–71 | 57 La<br>138·92 | 58 Ce<br>140·13 | 59 Pr<br>140·92 | 60 Nd<br>144·27 | 61 — | 62 Sm<br>150·43 | 63 Eu<br>152·0 | 64 Gd<br>156·9 | 65 Tb<br>159·2 | 66 Dy<br>162·46 | 67 Ho<br>164 94 | 68 Er<br>167·2 | 69 Tu<br>169·4 | 70 Yb<br>173·04 | 71 Lu<br>174·99 |

Fig. 1

If to-day we resume the never-ending task of drafting a table of the Periodic System embodying present knowledge, it will take the form of Fig. 1, or some table similar to it. We are now aware that atomic number and not atomic weight defines the position of an element, and that between barium (atomic number 56) and tantalum (atomic number 73) there are only sixteen places. We have given up hope of assigning the fourteen known rare-earth elements to consecutive groups in the System, and therefore prefer to write them outside its framework. The discovery of hafnium (element 72) filled in 1922 the place of eka-zirconium; that of rhenium (element 75) in 1925 the place of dwi-manganese. Polonium, radon, actinium *K*, actinium, and protactinium have taken

* Substance of a lecture delivered before the Institute of Chemistry in London on Feb. 18.

their respective places in the last two horizontal rows. Only three positions below uranium are left empty, those corresponding to the atomic numbers 43, 61 and 85, that is, eka-manganese, a rare earth, and eka-iodine. Before saying more about these, it seems necessary to add a word of explanation to the assignment of an element actinium K to place 87 (eka-cæsium).

It is well known that most of the elements from thallium (81) to uranium (92) are represented by more than one atomic species, thanks to the three radioactive disintegration series which start from uranium, actino-uranium, and thorium and emit α- as well as β-particles, thus covering some parts of the Periodic System repeatedly ("Radioactive Displacement Laws"). All three series have, however, one peculiar feature in common, namely, the successive emission of four α-particles, starting from the thorium isotopes ionium, radio-actinium, and radio-thorium, respectively. As a consequence, immediately after element 90 the elements 88, 86, 84, and 82 are formed. The elements 89 and 83 are nevertheless represented in the Periodic System, because the former is produced in the actinium and thorium series before, and the latter in all three series after, the 'four α-particle run'. Elements 87 and 85, however, are missing in the three disintegration series; compare Fig. 2, in which the full line represents the actinium series in the generally accepted form.

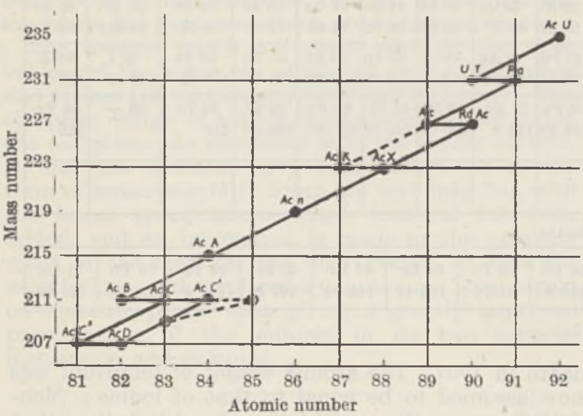


Fig. 2

RELATION OF ELEMENTS 87 AND 85 TO THE ACTINIUM SERIES.

There was always hope of finding the missing elements outside the main series as so-called 'branch products', and it was easy to predict theoretically, on the basis of the displacement laws, which atoms would have to undergo a 'dual disintegration' in order to produce the looked-for effect. If any one of the emanations emitted β-particles (in addition to their well-known α-radiation), or if either actinium or its isotope mesothorium 2 emitted α-particles (in addition to their β-radiation), the result would necessarily be an isotope of element 87. Experiments proved that neither in the uranium nor the thorium series does such a branching occur[2]. On the other hand, it was found in Vienna[3] so early as 1914 that carefully purified actinium emitted, in very small intensity compared with the activity of the main series, α-rays of the range (in air at N.T.P.) of 3·4 cm. The possibility was considered that this indicated a "dual disintegration of actinium, a small percentage of the atoms emitting α-particles, the great majority β-particles". The result of the α-radiation would, of course, be the element 87. The continuation of these researches was prevented by the outbreak of war, but three years ago Mlle Marguerite Perey[4] in Paris not only confirmed the existence of α-rays from actinium of the range observed in Vienna, but also was able to show that in purified actinium solutions to a small extent a β-radiating substance of a half-value period of 21 minutes is constantly formed; that this substance cannot be removed by the precipitation of sulphides or carbonates, but that it can be crystallized together with cæsium perchlorate or chloroplatinate. It seems most likely that this β-radiating substance with the properties of an alkali metal is the product of those comparatively few actinium atoms which emit α- instead of β-particles, and possesses therefore the atomic number 87. Of 1000 atoms, less than 10 seem to follow this path. (In the disintegration of actinium C the proportion is reversed; about 2 in 1000 emit β-particles while the rest decay with α-emission to actinium C''.) The β-radiation displayed by this element 87 would necessarily lead back to an element 88, probably identical with actinium X, in the same way as both routes starting from actinium C unite in actinium D. See Fig. 2; the abscissa shows the atomic number (that is, number of protons) of each atom, the ordinate its mass number (that is, nearest integer to the atomic weight, or number of protons plus neutrons).

Further confirmation of Mlle Perey's experiments is certainly desirable. The fact, however, that she was able to follow the decay as well as the growth of the 21-minutes substance, and the agreement of the chemical evidence with the observed α-radiation of actinium, is very assuring. As the name "actinium K" has been suggested for this new element we have inserted its symbol AcK in place 87 of our Periodic Table (Fig. 1).

There is so far no trustworthy indication of a branching of any of the main radioactive series leading to an element 85, nor has a stable form of this element been found. We must assume at present that the elements 43, 61 and 85 are missing on our earth. The regularities of the Periodic System enable us to describe approximately the properties to be expected from these elements; but especially for 85, since it is on the border line between metals and non-metals, and without a higher homologue, such predictions are necessarily vague. Fortunately for the interests of chemists who are systematically minded, there is a possibility of studying experimentally the properties even of elements which are not represented in Nature: we can try to make them, and if we are lucky enough to obtain them in a (not too short-living) radioactive form, the well-known methods of radio-chemical investigation, that is, a combination of electrometrical measurements with analytical-chemical operations, will enable us to explore their chemical behaviour even if quantities of only $10^{-10}$ gm. or less have been produced.

The most efficient instrument for atomic transmutation is E. O. Lawrence's cyclotron. Of the overwhelming number of nuclear reactions carried out with its help, for the purpose of our present survey, in which we intend to omit all details of no immediate interest to the chemist, we need remember only two. In many cases when an atom of atomic number $Z$ and mass number $M$ is bombarded with deuterons ($^{2}_{1}D$), an atom of atomic number $Z+1$ and mass number $M+1$ makes its appearance while one neutron per atom is liberated. If the bombardment is carried out with helium nuclei ($^{4}_{2}He$), an element $Z+2$ of mass $M+2$ may be formed, this time with emission of two neutrons per atom. The newly created atoms are mostly unstable; thus these two reactions give us the possibility of producing radioactive elements one and two places higher in the Periodic System than the bombarded elements. Their application has provided active forms of all three missing elements for chemical investigation. Here are the main results.

Element 43, Mendeléeff's eka-manganese, was obtained by bombarding element 42, molybdenum, with deuterons[5]. At least five radioactive isotopes of atomic number 43 were recognized; as their half-value periods were of the order of hours and days, a study of their chemical behaviour was possible. It was found that element 43 can be precipitated with hydrogen sulphide from alkaline and (not too) acid solutions; that it can easily be deposited electrolytically; and that its oxide is volatile. All this shows that it resembles more its higher homologue, rhenium, than its lower, manganese. It may be mentioned that the identity of one of the isotopes with element 43 could be verified even by the measurement of the $K_\alpha$ line of its X-ray spectrum; not in the usual way, of course, by exciting it on the anticathode of an X-ray tube, but through the spontaneous emission of this line as a consequence of the transition from a higher to a lower isomeric state of the nucleus.

For the production of element 61 both the atomic reactions mentioned above have been successfully tried. When element 60 (neodymium) was bombarded with deuterons[6], a radioactive isotope of element 61 with a half-value period of a few hours was obtained; bombardment of element 59 (praseodymium) with α-particles[7] resulted in a radio-element 61 of a half-value period of about 200 days. In this case the chemical character can be deduced with great certainty from the position in the Periodic System; element 61 must be a rare earth, of a basicity higher than samarium and lower than neodymium. It is a task for the chemist now to test this prediction with the help of one of the radioactive isotopes of 61, but scarcely anything else of interest can be expected from an investigation of invisible quantities of this new member of the rare earth group.

Far more important, for the reasons given above, is the chemical study of element 85, and it deserves special consideration also on account of its radioactive behaviour. Since the element one place lower down, polonium, is not available in sufficient quantities for a bombardment, the most promising reaction was the treatment of the element two places below, bismuth, with helium nuclei accelerated in the cyclotron. It was found that, besides others, the following sequence of reactions took place[8]:

$$^{209}_{83}\mathrm{Bi} + {}^{4}_{2}\mathrm{He} \longrightarrow {}^{211}_{85} + 2\,{}^{1}_{0}n$$

$$^{211}_{85} + {}_{-1}^{0}e \longrightarrow {}^{211}_{84}\mathrm{AcC}'$$

$$^{211}_{84}\mathrm{AcC}' \longrightarrow {}^{207}_{82}\mathrm{AcD} + {}^{4}_{2}\mathrm{He}$$

The first line shows that element 85 is formed according to expectations; its instability is, however, of a peculiar type, as indicated by the second line: instead of emitting positrons its nuclei capture electrons (out of the $K$-shell) and are thereby transformed in atoms of a well-known natural radio-element actinium C′, an isotope of polonium. This process goes on with a half-value period of 7·5 hours. Actinium C has a much shorter period, only $5 \times 10^{-3}$ sec.; it is, therefore, always present in element 85 in equilibrium quantity, and its α-radiation (range 6·5 cm.) decays with the half-value period of its parent, namely, 7·5 hours. That makes it possible to investigate the chemical properties of element 85 as though it were itself emitting the α-rays. The final product must be the same as that of the ordinary AcC′, namely actinium D, or 'actinium lead', of atomic weight 207. (It seems likely that the same final product is also reached by another way, as element 85 actually undergoes a dual disintegration, about half its atoms emitting α-particles.) In Fig. 2 we have indicated by dotted lines the way in which element 85, artificially produced from $^{209}_{83}$Bi, is changing into the natural radio-element AcC′. This example, which is not the first of its kind, illustrates well that there is no fundamental difference between 'artificial' and 'natural' radioactivity; it is therefore better to speak of 'artificially produced', and not of 'artificial', radio-elements.

Thanks to the favourable decay period of more than seven hours, the chemistry of element 85 can be thoroughly studied. The following reactions are of special interest. Mixed with bismuth it is quantitatively precipitated by hydrogen sulphide in acid solutions. Fractional hydrolysis of bismuth nitrate enriches element 85 in the first fractions; since it is known that the tendency of polonium to hydrolyse is already greater than that of bismuth, it would be interesting to compare in this respect element 85 with polonium. Closely connected with hydrolysis is the very characteristic feature of polonium of behaving like a colloid in all but strongly acid solutions; the same pecularity should be easily detectable in solutions of element 85. If this 'eka-iodine' is mixed with iodine, it cannot be precipitated, together with the latter, by silver nitrate, and it is only partly extracted by carbon tetrachloride. It can be deposited electrolytically. It is not precipitated by hydrochloric acid, in presence of lead. Its faculty of forming a volatile hydride has not yet been demonstrated conclusively; this one chemical property, however, can be predicted with complete confidence, since it depends directly on the position of an element in the Periodic Table—which must precede a rare gas by not more than four places—and is shown even by the still more metallic elements polonium and bismuth. It should be not too difficult an experiment to compare the stability of the gaseous hydride of 85 with that of HJ and $H_2Po$.

It is clear from the above that element 85 differs in important reactions from its lower homologue iodine and resembles more its left-hand neighbours in the VIth period. It is all the more interesting that some similarity to iodine is reported in its physiological behaviour; it is concentrated in the thyroids of guinea pigs.

It has been mentioned before that Mendeléeff, in some of his tables, reserved room for trans-uranium elements. The ease with which in many cases artificial transmutation generated elements higher in the Periodic System than the bombarded ones naturally led to experiments to create these trans-uranides. For a while it was believed that, by a neutron bombardment of uranium, radioactive elements up to atomic number 96 could be obtained. Careful chemical investigation proved, however, that most of the elements thus produced were fission products of uranium, radioactive isotopes of elements placed much lower in the Periodic Table; for example, bromine, strontium, antimony, tellurium, xenon and barium. However, element 93, that is, the one immediately following uranium, was synthesized in an active form and its chemical character studied[9]. It seems that here, too, the similarity with the preceding element, uranium, is greater than with its lower homologue, rhenium. Element 93 is, for example, not precipitated by hydrogen sulphide in acid solution, and does not possess an oxide volatile at red heat.

Finally, the question may be considered how great is the chance that the elements which so far have only been produced artificially occur in Nature. For the elements 43 and 61 it is certainly very small, for experimental as well as theoretical reasons. We have seen that the analytical-chemical properties of element 43 are very much like those of rhenium, and that there is no reason to doubt that element 61 follows the other rare earths. Now rhenium has been prepared in hundreds of kilograms, but not the slightest trace of element 43 was ever found associated with it. For element 61 a special search has been made, among others, by Auer von Welsbach, who not only had unusually large quantities of rare-earth material at his disposal but possessed also an unrivalled experience in their fractional separation; he came to the conclusion that no rare earth existed between neodymium and samarium.

To this we may add that, quite independently of these experimental facts, a general rule seems to exclude the possibility of a stable element 43 or 61. J. Mattauch[10] has directed attention to the absence, or, at least, extreme rarity, of stable isobars (that is, atoms of equal atomic weight) belonging to neighbouring elements. If two neighbours in the Periodic Table have isobars, one of the atomic species is almost invariably unstable, changing into its isobar by emission of an electron or a positron (or both). For example, ${}^{40}_{19}K$ is isobaric with ${}^{40}_{18}A$ and with ${}^{40}_{20}Ca$; in accordance with Mattauch's Rule, ${}^{40}_{19}K$ emits β-particles, thus changing into ${}^{40}_{20}Ca$ (and perhaps also positrons, thereby producing ${}^{40}_{18}A$, the astonishing terrestrial abundance of which has been tentatively explained by this assumption). If Mattauch's Rule is valid in the region of the elements 43 and 61, there are no stable isotopes of these two elements possible, as all mass numbers are occupied by stable isotopes of their neighbours. Element 42, molybdenum, possesses the stable isotopes 92, 94, 95, 96, 97, 98 and 100; element 44, ruthenium, 96, 98, 99, 100, 101, 102 and 104; so for element 43, the combining weight of which ought to be between molybdenum and ruthenium, no atoms with masses between 94 and 102 are available. The case of element 61 is similar.

For element 85 Mattauch's Rule makes no prediction, since from element 84 onwards no stable atoms are known. Nor are the negative results of the experiments carried out so far in search of this element conclusive; for it was generally supposed that in the course of analytical separations it would follow iodine, while we know now that it is much more like polonium and bismuth. The task of a chemist embarking to-day on a new attempt at its discovery would be facilitated not only by this knowledge but also by the opportunity of adding the artificially produced radioactive form of element 85 as an 'indicator' to his material; the α-rays, decaying with the characteristic period of 7·5 hours, would show him at once in which fraction the looked-for natural element, if present at all, is concentrated.

Even if it should be impossible to detect any of the three missing elements on the earth, it is not unlikely that they possess a transitory existence in the course of the innumerable thermo-nuclear reactions occurring in the interior of the sun and the other stars.

[1] See NATURE, 134, 799 (1934).
[2] Hahn, O., *Naturwissen.*, 14, 159 (1926).
[3] Meyer, St., Hess, V. F., and Paneth, F. A., *Sitzber. Wien. Akad. Wiss.*, 123, 1459 (1914).
[4] Perey, M., *C.R.*, 208, 97 (1939); *J. Phys. et le Radium*, 10, 435 (1939).
[5] Seaborg, G. T., and Segrè, E., *Phys. Rev.*, 55, 808 (1939): Segrè, E., NATURE, 143, 460 (1939).
[6] Pool, M. L., and Quill, L. L., *Phys. Rev.*, 53, 437 (1938): Law, H. B., Pool, M. L., Kurbatov, J. D., and Quill, L. L., *Phys. Rev.*, 59, 936 (1941).
[7] Kurbatov, J. D., MacDonald, D. C., Pool, M. L., and Quill, L. L., *Phys. Rev.*, 61, 106 (1942); Wu, C. S., and Segrè, E., *Phys. Rev.*, 61, 203 (1942).
[8] Corson, D. R., MacKenzie, K. R., and Segrè, E., *Phys. Rev.*, 58, 672 (1940).
[9] McMillan, E., and Abelson, P. H., *Phys. Rev.*, 57, 1185 (1940).
[10] Mattauch, J., *Phys. Z.*, 35, 617 (1934); *Naturwiss.*, 25, 738 (1937).

# MOSQUITO-BREEDING IN STATIC WATER SUPPLIES

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In countries such as Great Britain, where neither malaria, yellow fever nor other mosquito-borne diseases are endemic, consideration of the habits of these insects in connexion with war has scarcely ever been necessary. During the War of 1914–18, it is true, quite extensive outbreaks of malaria occurred in England owing to the fact that soldiers who had contracted the disease abroad were sent to convalesce in districts where *Anopheles maculipennis*, the chief malaria-carrier of Europe, was then (as now) particularly prevalent[1]. It may perhaps also be noted that neglect of agricultural drainage during that war led to a marked (and in some cases serious) increase in the mosquito infestation of many low-lying areas of Great Britain.

Neither of the above-mentioned contacts between British mosquitoes and war, however, attracted more than local interest; and it was only in the early part of last year, when fire-fighting authorities commenced to instal very large, open-air tanks all over the country, that any such association can be said to have become a matter of general concern. Although public knowledge regarding mosquitoes is extremely limited, most people are aware of the fact that static water is liable to encourage them: hence the appearance of these conspicuous receptacles throughout our urban areas has unavoidably given rise to considerable apprehension. It seems therefore advisable to consider, first, how far these misgivings are justifiable; and, secondly, to what extent precautionary measures can and should be adopted.

In theory, the 'control' of mosquitoes is a comparatively simple matter, owing to the fact that both their larval and pupal stages of development are passed entirely in water. Mosquitoes of the Culicine tribe (to which all but four of the British species belong) breed exclusively in static water; but those of the Anopheline tribe breed also (and in the case of many species chiefly) in running water—their larvæ being able to anchor themselves to surface weeds by means of certain hook-ended hairs. It thus follows that mosquito-control operations include (in addition to the obvious procedure of abolishing potential breeding-places by drainage, filling-in, etc.) the clearing and re-grading of ditches and the removal of aquatic vegetation. It frequently happens, however, that lack of time (or of funds) renders such measures impracticable, in which case an alternative method is to kill the larvæ, either by spraying oil upon, or mixing 'larvicidal' chemicals with, the infested water. In the former case the oil dissolves away a hydrofuge secretion from within the breathing orifices of the larva and then enters the tracheal system with fatal results[2]; in the latter case the larva is poisoned when it feeds. For larvicidal purposes preparations based on coal tar derivatives and introduced into 30,000–50,000 times their own volume of the infested water are most commonly employed. It is important to note, however, that water should on no account be treated with either oil or larvicide if the presence of mosquito larvæ has not been actually observed: treating uninfested water being not only a sheer waste of both time and money but also liable to destroy fish, water bugs and various other 'mosquito enemies' that natural collections of water so frequently contain.

Although all the above-mentioned operations are simple in themselves, the practical application of them is a much more complicated matter than it sounds. This is chiefly due to the numerous idiosyncracies that characterize different species of mosquitoes, notably in regard to the quality and environment of the water that they instinctively select for oviposition. (Some British species lay their eggs separately on water; others lay them separately on land liable to flooding; others stick them together during laying, so as to form 'egg-rafts'. The species *Culex pipiens*, *C. molestus* and *Theobaldia annulata* belong to the last-mentioned group.) Of the thirty species at present known in Great Britain, seven have been but rarely recorded, but the habits of the remaining twenty-three have been more or less thoroughly studied. Of the species included in the latter group, eight are known to breed chiefly in woodland pools; three, exclusively in rain-filled cavities in trees; two, almost exclusively in slow-running, weedy water; two, chiefly, and one, exclusively, in the salt or brackish water of coastal marshes—and so on. The coastal species have an exceptionally long flight-range and are consequently able to cause annoyance in localities several miles distant from their breeding-grounds.

In regard to the breeding of mosquitoes in tanks, reservoirs and other containers, it has until recently been believed that only two British species—namely *Culex pipiens* Linnæus and *Theobaldia annulata* Schrank—are at all likely to do so. Of these two mosquitoes, the former breeds impartially in either clean or foul water; the latter chiefly in water contaminated by sewage or other nitrogenous matter. Under natural conditions, *C. pipiens* rarely, if ever, bites human beings (its customary victims being birds), but *T. annulata* avidly does so. These facts indicate that tank-stored water, unless allowed to become contaminated in the manner above mentioned, is unlikely to give rise to any 'mosquito annoyance' other than the possible public apprehension that the sight (or sound) of any species of mosquito is liable to arouse.

Although, however, this assumption may still hold true where tanks in open-air situations are concerned, it would be unsafe to consider it generally applicable until the breeding habits of another British mosquito, namely, *Culex molestus* Forskal, have been more fully investigated. Both in the larval and adult forms this species resembles *C. pipiens* so closely that its presence in Great Britain remained unnoticed until 1934, when it was accidentally discovered at Hayling Island (Hants)[3]. Previous to its being found in Great Britain, the presence of *C. molestus* in other parts of Europe (for example, in France[4], Germany[5], Greece[6], Hungary[6] and Malta[6]) had already been detected, and its biology studied, by Continental and other entomologists, who considered it to be an abnormal 'variety' of *C. pipiens* rather than a separate species. The author's reasons for preferring the latter classification have been fully expounded elsewhere[7].

To say the least, the biological peculiarites of *C. molestus* are extremely striking. Of the two thousand odd species of mosquitoes at present described, less than a dozen are known to be stenogamous (that is, able to mate in a very small space) and only three to be autogenous (that is, able to lay fertile eggs without having had a blood meal before or after mating). Not only does *C. molestus* differ from both *C. pipiens* and *T. annulata* in being both stenogamous and autogenous, but it also differs from the former species in being an extremely fierce 'man-biter' whenever opportunities occur.

It was suggested that the detection of *C. molestus* in Great Britain might supply the long-awaited explanation of mysterious cases, reported from time to time, in which human beings had been fiercely bitten (notably in houses and tube railways in London) by mosquitoes diagnosed as *C. pipiens*. The accuracy of this suggestion was afterwards established by the investigation of '*molestus* plagues' occurring in houses in Hull[9]; in houses in Westminster and other Thames-side districts of London (for example, Battersea, Bermondsey, Chelsea, Deptford, Fulham and Greenwich); and in various stations of the already mentioned tube railways.

In addition to being both stenogamous and autogenous, *C. molestus* is also homodynamic (that is, able to breed at any time of the year provided that external conditions remain favourable). Possessing these three unusual characteristics, it is very easily bred in the laboratory all the year round; but, in spite of this fact, our knowledge regarding its natural breeding habits is unfortunately far from complete.

In the case of the tube railways, however, larvæ of this species were found to be heavily infesting collections of water stagnating under station platforms and between the lines. They have also been found in flooded cellars and cesspools, in underground systems of household waste disposal, in septic tanks, in cavities within a water-logged rubbish dump and in the underground tanks of a Thames-side laundry. Consideration of the above and certain other facts have led some entomologists to suggest that *C. molestus* may breed chiefly in water which, in urban and other built-up localities, is allowed to stagnate in dark, underground situations; and since, both in London and elsewhere, static water is now being stored in the basements of many houses, the possibility of *C. molestus* being thus provided with facilities for extending the range of its undesirable activities appears to be far from remote. (Should any such developments occur, the covering of ventilators, window openings, etc., to prevent gravid females of this species from reaching the water, might perhaps be deemed advisable.) It is obviously important that tanks of this kind should be kept under observation, and specimens of any mosquito larvæ seen collected for identification.

Whether even the 'man-ignoring' *C. pipiens* is likely to breed in the very large outdoor tanks that are now in use is still a matter for speculation. Should such cases occur, the question of applying oil or larvicide merely to allay public apprehension must presumably receive consideration. It has to be noted, however, that oil is extremely injurious to tanks with bituminous linings.

As already mentioned, tank-stored water that is allowed to become contaminated by sewage or other nitrogenous matter will very probably become infested with larvæ of the man-biting species *T. annulata*. Attention has, moreover, been directed to the possibility of tank-stored water becoming a suitable breeding medium for certain of our more 'countrified' species, owing, for example, to algal development or to the accidental or mischievous introduction of vegetable or other organic matter. It is, however, obviously preferable to deal with such contingencies if and when they arise rather than to adopt the spendthrift policy—advocated in some quarters—of indiscriminately treating enormous numbers of tanks on the off-chance of a few of them containing mosquito larvæ. These creatures are so easy to recognize that anyone who has ever seen one—whether in a specimen tube or a tank—can have no difficulty whatever in deciding when anti-larval measures are necessary.


[1] Marshall, J. F., "The British Mosquitoes", p. 319 (British Museum 1938).
[2] Keilin, D., Tate, P., and Vincent, M., *Parasitology*, 27, 257 (1935).
[3] Marshall, J. F., and Staley, J., NATURE, 135, 34 (1935).
[4] Roubaud, F., *C.R. Acad. Sci.*, 188, 735 (1929).
[5] MacGregor, M. E., *Trans. Roy. Soc. Trop. Med. and Hyg.*, 26, 307 (1932).
[6] Tate, P., and Vincent, M., *Parasitology*, 28, 512 (1934).
[7] Marshall, J. F., and Staley, J., *Proc. Roy. Ent. Soc.*, A, 12, 17 (1937).
[8] Marshall, J. F., and Staley, J., *Parasitology*, 27, 4 (1935).
[9] Marshall, J. F., and Staley, J., NATURE, 136, 641 (1935).
[10] Shute, P. G., *Lancet*, 1, 6 (1941).


## SCIENCE AND WAR*


By AUSTIN H. CLARK
U.S. National Museum


FROM the earliest times of which we have a record to the present day, the history of man has been marked by constant changes in the social systems, changes that often were abrupt and violent. One form of social structure or of government has succeeded another. Small but powerful social or political units have grown by accretion or by conquest into great kingdoms or empires. These kingdoms or empires eventually have decayed or fallen apart.

Together with these frequent social, political, or economic readjustments of the past we see a constant and fairly continuous development of other forces which to a large extent are independent of transient political conditions. We note a growing interest in and understanding of the products of the earth and their uses. We also are able to trace the ever-increasing subjugation of the forces of Nature, which more and more come to be the servants instead of the enemies of man.

The greatest of all human achievements was the control and use of fire. We have no knowledge of when or how fire was first transformed from a terrifying natural phenomenon into man's most useful servant. Second among human achievements was the fashioning and use of tools and weapons. The origin of the use of tools is lost in the far distant past. Then came the weaving of textiles from plant or animal fibres and the moulding of pottery vessels, followed by the appearance of bronze. Not long after the appearance of bronze utensils of various sorts, the wheel and axle appeared in Asia, soon spreading to Europe.

From the Bronze Age onward the knowledge of the use of natural products and the control of natural forces have shown a fairly continuous development.

Since the beginning of the present century the advance in the knowledge and understanding of the products and forces of Nature has been greater than in all the thousands of years preceding—or at least since the subjugation of fire, the first fashioning of tools, and the domestication of animals and plants.

Whether we like the idea or not, we are now living in an age, and under conditions, in which science plays a dominant part, and the established scientific principles that underlie many of the most familiar of our present-day improvements were unheard of, or considered fallacious, no longer than a generation ago. This is self-evident to all; but the implications inseparable from a culture based ever more intensely and extensively on increasingly abstruse science are not as yet fully appreciated.

The progress that through the ages has been made in the understanding of natural phenomena and in the utilization of natural products is continuing at an accelerated pace, and will continue in the future, in spite of what may happen in the next few years. It may be locally obstructed, or even brought to an end, but somehow, somewhere, it will carry on.

The present struggle is no more a contest in the military field than it is in the field of science. It is quite possible to win the War on the battle front, but lose it in the laboratory. We must see to it that, so far as possible, the steady advance of science is maintained. At the present time we are utilizing to the maximum extent our scientific resources and our scientific personnel to aid in our war effort. But this is not enough. Various branches of science not of immediate military application are in the long run quite as essential for our progress and our welfare as are those forms of engineering, of physics, and of chemistry that underlie the construction and the use of modern implements of warfare. These are the many and varied types of pure science, lines of work leading to results seemingly of no importance, that all too often are regarded merely as a form of mental exercise undertaken solely for the personal satisfaction and gratification of the person concerned. What we call pure science is simply a branch of science for which no economic application has as yet been found. But at any time a body of uncoordinated facts may suddenly and unexpectedly fit into an integrated whole, to our advantage. Without its advance fringe of competent workers in pure science constantly probing the great unknown and accumulating masses of data with no apparent immediate application, the broader aspects of scientific progress soon would languish. Pure science is likely to suffer severely in times like the present—in times when it would seem to be the wisest course to give it the maximum encouragement.

In the democracies, progress in any line of science is mainly dependent upon the willingness of the people

* Substance of the address of the retiring president of the Washington Academy of Sciences delivered at the 309th meeting of the Academy on January 15.

to support work in that particular line which, in turn, is dependent upon popular interest and appreciation.

There are people who are by no means science-minded. Their attitude varies all the way from passive superciliousness to outspoken hostility. We who are engaged in scientific work, and who understand its importance in the general complex of present-day human affairs, often fail to realize how recently science has been able more or less successfully to overcome various forms of popular prejudice and to secure the favour of a very large section of the general public.

The present popular attitude towards science cannot properly be understood without some knowledge of the public attitude in the more or less recent past. I propose, therefore, to digress here in order to indicate briefly the changes that have taken place here and in England since the early days of the settlement of the United States. At that time, in the reign of Queen Elizabeth, Galileo was still a student at the University of Pisa, Tycho Brahe had just completed his observatory, and Paracelsus and Agricola only recently had died. In those days science was almost wholly included in the subject of theology, and scientific work was restricted within narrow bounds by the dogmas of the theologians.

For some time there had been a growing restiveness against the restrictions placed on scientific investigations by the theologians. This restiveness began to take the form of concerted action in the first half of the seventeenth century. As early as the reign of Charles I, about 1645, there existed in England an organization referred to by the Hon. Robert Boyle, seventh son of the first Earl of Cork, as the "Invisible College". This "Invisible College" was first suggested by Theodor Haak (or Hank), a German from the Palatinate, then resident in London. It consisted of weekly meetings at which the results of experimental work in philosophy, in its broad sense, were discussed. This was rather an unorthodox procedure for the time, but those who attended the meetings were among the ablest men of England, and included theologians as well as others. One of the theologians was Dr. John Wilkins, afterwards Bishop of Chester, who had married Robena, sister of Oliver Cromwell. Another participant was Sir Christopher Wren, who later laid down the plan for the College of William and Mary.

According to Dr. Cromwell Mortimer, "had not the Civil Wars happily ended as they did, Mr. *Boyle* and Dr. *Wilkins*, with several other learned men, would have left *England*, and, out of esteem for the most excellent and valuable Governor, *John Winthrop* the younger, would have retir'd to his new-born Colony [Connecticut] and there have established that *Society for promoting Natural Knowledge*, which those Gentlemen had formed, as it were, in *Embryo* among themselves".

Emigration to America was, however, forestalled. On November 28, 1660, the "Invisible College" became visible as "The Royal Society of London for Improving Natural Knowledge". On the Wednesday following, word was brought that King Charles II approved the design of the meetings; in October 1661, the King offered to be entered as one of the Society; and in the next year the Society was incorporated under the name of the Royal Society, the first charter of incorporation passing the Great Seal on July 15, 1661.

Although the Royal Society remained in England, both the College of William and Mary and Harvard College received considerable amounts of money from the estate of Boyle after his death in January 1691–92.

Science now began to assume a new aspect. Charles II had in effect declared that there is nothing irreligious in reporting facts. So records of observed facts and their interpretation in the light of other facts began to supersede introspection in which the aid of facts was regarded as superfluous, combined with interminable commentaries on the works of Aristotle.

Following the Restoration, science in Britain became largely an occupation of the aristocratic and the wealthy, and for the most part was followed along lines that had little or no economic application. In the public mind it came to be identified more or less completely with the aristocracy and to be regarded as partaking of the same aloofness from the general run of human affairs that characterized the social life of the upper classes. The natural result of this was that when, in the Victorian era, the champions of the lower classes began to gain a considerable following, they, or at least many of them, attacked science as one of the perquisites of the aristocracy. This attitude is well illustrated by Charles Dickens's "Mudfog Papers" published on the occasion of the first meeting of the British Association for the Advancement of Science.

Since that time science in England gradually has come more and more into popular favour. Applied science has made rapid strides and is now quite as fully developed and as highly regarded as it is in any other land. The rise in the prestige of applied science, however, has not been accompanied by any noticeable decline in the popularity of pure science, so that here we find the two types advancing side by side in more or less ideal balance. But, unfortunately, science in Britain still does not have the complete confidence of the public, and is not by any means free from neglect, disparagement, or even attack in the popular Press.

In the United States the history of science has been somewhat different. In early Colonial times scientific effort was devoted mainly to making known the natural resources of the new land, particularly the plant and animal life. But applied science early attracted the attention of the colonists. In later Colonial times applied science, especially in certain engineering branches, was systematically discouraged in the fear that the Colonies might become competitors of the mother country in the production of manufactured goods. It was possibly partly as a reaction from this suppression that after the Revolution science stood high in the favour of the representatives of the American people, its most insistent and powerful advocates being Thomas Jefferson of Virginia, Benjamin Franklin of Pennsylvania, and John Adams of Massachusetts. But it was some time before the new country was sufficiently well organized to enable the people to devote much thought to science. When they did, a spontaneous interest, taking various strange and crude forms, appeared, particularly in the agricultural areas. This crude popular science—and pseudo-science—gradually became amalgamated with the more orthodox science of the schools and colleges, and we note, especially after the middle of the last century, an enormous expansion of applied science in all forms, later very largely supported by Federal and State appropriations made possible by active and widespread interest among all the different groups in our population.

In the United States popular interest in science is twofold, arising both from the vista of economic betterment resulting from applied science, and from its appeal to the imagination. We all like to look forward to the day when we shall be even more comfortable than we are now. But we all have a non-material side. We like to get away from the hard realities of everyday life and to contemplate the unknown, and beyond that the unknowable. We all would like to know more about the world we live in. What would we find a few hundred miles down in the earth, or 20,000 feet below the surface of the sea ? We would like to know more about the stars : Are there any other worlds like ours ? And what is it like in interstellar space ? We would like to know more about ultimate human origins—indeed, about very many things concerning which our present information is vague and fragmentary. Now although popular interest in science is more general and more widespread in the United States than it is in most other countries, it tends to gravitate in these two directions, towards the directly economic and towards the mysterious. Between these two extremes lies a broad, intermediate field in which the American people as a whole takes little interest, but which is intensively cultivated elsewhere. This is the descriptive branch of pure science, the results of which are of no immediate economic import and are not mysterious.

Popular interest in science must not only be maintained, it must also be increased if we are to hold our own in the years to come, for whether carried on under Government support or in endowed institutions of learning, the full development of our scientific potentialities is dependent upon a sympathetic public attitude. In order to secure, to maintain and to increase public interest in and sympathy towards scientific work, and to convert the still disconcertingly numerous unbelievers, it is essential that we continually provide the public with news regarding scientific progress in all lines of interest to them, from both the material and non-material or philosophical points of view. Such impersonal news is especially desirable in times of national emergency.

Fortunately, in this respect the United States is in an excellent position. More and better science appears in its daily Press and other journals than in those of most other countries, and science is less frequently disparaged and denounced there than elsewhere. For this we have chiefly to thank the National Association of Science Writers, the members of which, in addition to knowing science, know the public mind and are able to present the advances in science in terms everyone can understand.

Progress in science is possible only with the support of an interested and appreciative public. It is also possible only through the efforts of a carefully selected and adequately trained personnel. This is a matter that heretofore has scarcely received the attention it deserves.

At the present time a very serious danger to our continued progress in science has arisen. This is the introduction into the Army of many young men who would be of vastly greater value to the country if they were permitted to continue their studies, or to remain in research positions. The matter is further complicated by the fact that as a rule the most valuable of these young men are those most likely to enlist on their own initiative.

After the War of 1914–18 there was a marked scarcity of able young scientific men. This was most noticeable, perhaps, in the biological sciences, though it was more or less noticeable in other branches as well. Many promising young men were killed. Others, as a result of several years spent in the various armies, found themselves unable to make the necessary readjustment to scientific work. Still others tried to readjust themselves but were only partially successful. Breaking the thread of continuity of effort between the impressionable boy in the formative period and the mature man cannot but result in a certain amount of dislocation. We are reminded of the old Berber proverb :

> "Teaching boys is like ploughing earth,
> Teaching men is like ploughing rock,
> Teaching old men is like ploughing water."

There are two ways out of this dilemma. Either the student may be placed on a deferred list so that he may be enabled to continue his studies uninterruptedly, or he may be assisted in carrying on his work, to whatever degree may be found practicable, while in the Services.

Many young botanists and zoologists would welcome an opportunity for collecting specimens and continuing their studies in regions new and strange to them. Such material as they collected could be sent home to be identified, or to be stored until their arrival. Activities of this nature carried on in their spare hours would go far towards overcoming that feeling of boredom that afflicts almost everyone stationed at an isolated army post or naval base, and there is no reason for believing that these activities would in any way detract from their military efficiency. Not only would this work benefit the men engaged in it; it would also go far towards filling many gaps in our knowledge of the distribution of animals and plants, and of other features connected with them.

In army posts and naval bases a young zoologist or botanist who spends his spare time catching insects or pressing plants will at first be an object of ridicule to his associates, both officers and men. His situation, however, is by no means without precedent—and most honourable precedent. It may comfort him to realize that the world's leading authority on the Hesperiidæ, a peculiarly difficult group of butterflies especially characteristic of America, is Brigadier General William H. Evans of the Royal Engineers, while in the Royal Navy Rear Admiral Hubert Lynes is the leading authority on a very puzzling group of small African birds. Some time ago the collections of the British Museum were enriched by a fine collection of butterflies presented by Captain Lord Byron. Looking at the matter in a more frivolous light, is a young man using his spare time to continue his studies, and at the same time to advance our knowledge of animals and plants, any more ridiculous than an ancient tough old sea-dog in the forecastle engaged in fine embroidery work with delicately coloured silks, to the accompaniment of blood-curdling oaths ?

A vast amount of such work has been done by the personnel of foreign armies and navies in the past, particularly by officers in the British services. In fact, at one time the United States Navy assigned interested young officers to the Smithsonian Institution for instruction in the collection and preservation of material.

Whether in its material or in its non-material aspects, progress in science is dependent upon the fostering of the scientific spirit. The scientific spirit is more than mere curiosity. It is an insatiable curiosity that impels one to learn everything that is known about a given subject, and then to go further and broaden and extend that knowledge by personal investigation and research, in spite of all difficulties and discouragements—and these are always many.

The spirit of science is inborn, though it may appear in anybody, anywhere, in any class, or group, or race. In order to develop the scientific spirit to the maximum, as it must be developed if we are to hold our place in the world of the future, we must watch for it at its inception, and whenever and wherever it is found encourage it.

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# STATISTICAL CONTROL OF PRODUCTION*

By DR. C. G. DARWIN, F.R.S.
National Physical Laboratory

FOR a number of years I had been interested in the general question of tolerances, first from the point of view of pure science and later in its more practical aspects. My interest in this subject was very much focused when I came to the National Physical Laboratory, where I found that work was being done on the actual verification of manufacturing and inspection gauges; this work, of course, deals with the subject in an enhanced degree—squared, so to speak, because there is not only to be considered the tolerance of the work, but also the tolerances on both the go and the not-go gauges which are to check the work.

From internal evidence of the gauge drawings, it looked as if some of the tolerances were assigned much closer than should be necessary, and I started to try to find out how they had been fixed. To accomplish this, on every occasion when I met an engineer I asked him how he decided the tolerances in his branch of the subject; I fear I bored a great many people at this time. I got a variety of answers which sometimes explained things a bit, but often not at all, and though I discussed it with quite a number of men, many of them occupying prominent places in different branches of the profession, I came away with the impression that scarcely any of them were really interested in the subject of tolerances. To exaggerate the picture which I got as the result of my inquiry, I concluded that in designing a new machine the chief engineer drew it free-hand with dimensions to the nearest inch, and sent it to the draughtsman to work out the detail to the nearest thousandth, who then gave it to his junior assistant to mark in the tolerances. Instructions were certainly always given that tolerances should be as easy as possible, but only lip service was done to them, and the junior assistant, anxious not to get himself into trouble, would, as a general rule, think of the smallest number he knew and then halve it. This is a caricature, of course, and has some of the absurdity of one, but also perhaps just a little of the resemblance. Seriously, it seemed to me that there was a defect in the habit of thought of many in the engineering profession, and that some sort of campaign was needed to inculcate in people's minds the idea that every number has a fringe, that it is not to be regarded as exact but as so much plus or minus a bit, and that the size of this bit is one of its really important qualities.

Without any very clear idea of where to catch hold of the subject, I had full intention of trying to do something about it at the time I went to the United States a year ago. There I came across the method of 'statistical control' of mass production, and it was obvious at once that this is the right approach. Similar work has been done in Great Britain too, in particular by the staff of the General Electric Company and by members of the Royal Statistical Society, and it was perhaps ill-luck that I never came across it before crossing the Atlantic; but that I did not shows that it was not very widely known. I would add that it is not by any means very widely used yet in the United States, though the use is spreading. In both countries it has been principally used in industries associated with electricity, such as the telephone industry, but I want to emphasize that it should be of even greater importance in the mechanical industries, and that it is specially applicable to a business like the manufacture of munitions in all their aspects.

One of the important points in the new method is that it gives reasoned instead of guessed values to the tolerances. I will take as an example the making of time fuses for anti-aircraft, and I am giving away no military secrets in doing so, since I shall invent the data and miss out a lot of other considerations that would really enter. Suppose that the lethal area of a bursting shell is such that, if it explodes within a tenth of a second of the set time, it will make a kill. The gunner therefore demands of the manufacturer that he make a fuse with accuracy a tenth of a second. The manufacturer works out his method, but finds that whereas it is easy to get one to a fifth of a second, he will have a lot of trouble to get to a tenth, and, indeed, he estimates that for the same effort of work and cost he could not hope to get more than a quarter as many fuses if they must have the accuracy of a tenth. Now half his shells will burst within the range I asked for and so, in fact, I shall be wise if I accept his inferior fuse, since I shall thereby get four times as many shells of which half will do what I want, and I shall therefore double the rate of killing. I need not say that I have over-simplified the business; on one side I have missed out the cost of the other parts of the shells, and on the other, I have forgotten that the gunner has uncertainties of range to consider, so that his demand for a tenth of a second is more exact than he can justify for practical use.

My example is intended to show that it is good business for the user and the maker of any article to get together before deciding the tolerances of manufacture. The user may be inclined at first to feel that in doing this he is surrendering some of his freedom of choice, but if you consider it closer you will see that this is not so. He has not got any real freedom of choice, since he must surely try to design the article so as to be as easy to make as possible, but he has foregone the quantitative information of what will in fact prove to be easy, and above all of what thing, easy to make, will be good enough for the job. I may summarize this aspect of the matter by saying that the user has tended to demand that everything should be made for him as well as possible, but he ought to want everything to be made for him as badly as possible, or perhaps not quite that, but as badly as permissible. It is in this aspect that statistical control specially gives the right information.

* From a paper before a joint meeting of the Institutions of Civil, Mechanical and Electrical Engineers held on April 15.

There are a good many varieties of procedure called for in the circumstances of applying statistical control. First, there are the two classes according to whether it is a quantity or a quality that is concerned. It may be a measure, say, of a length or perhaps an electrical resistance, which has to fall within certain prescribed limits; for this we can measure each specimen and record it for analysis. In other cases the test is qualitative, in that the specimen either passes a test or fails to pass—for example, a vessel is watertight or not; and even the measure of a length may fall in this class if it is tested with a gauge, so that the answer is given in the form that it passes or fails to pass the gauge. Then there is another distinction that divides either type of work into two classes. Some tests, such as a measure of length, can be applied to every article made but other tests are destructive of the specimen; for example, the test of the force of an explosive or of the tensile strength of a bar. In this last case the test must of necessity only be done on a sample, and it is obviously important to take the sample as small as permissible. Moreover, even in the case where every specimen could be tested, there will be great economy in only doing it for a sample; so that, in fact, we may reckon that sampling is one of the main features of the process, and to determine the advisable fraction of the whole number that must be sampled is an important part of the duty of the statistician. Then again the practical problem of statistical control itself falls into two parts, for there is first the business of starting a new process and getting it into statistical control, and then the business of carrying it on later, after the control has been established. All these matters are described in the British Standards Institution publications B.S. 1008 and 600 (revised), but I shall content myself with giving a very crude sketch of a single example.

The example I shall take is based on one I learnt about in the United States, and I am intentionally leaving the details incorrect and falsifying some of the facts. I learnt about it from Colonel Simon, who has shown great brilliance in developing the methods for munitions in the U.S. arsenals. The subject was a particular type of time-fuse, and the gunners had assigned certain limits of tolerance which are described in the diagram (Fig. 1). The manufacturers all had great trouble in satisfying the demand for the longer times, so that the tolerances were exceeded both above and below, and Simon was called in to set the matter right. This he did, but to simplify the story we will imagine he had been in at the start, and describe how the process might have gone. It is first necessary to be sure that the process is 'in control', in the technical sense of the term. We take a considerable batch of the fuses, keeping them sorted according to time and place of manufacture. Group these sets (or probably samples of them) in batches of five and set the fuses at 20 sec. Time each fuse, and work out the mean of each batch and its 'range', that is to say, the difference between the shortest and longest among the five. These are marked on two charts in a way described on the British Standards Institution publication (Fig. 2). From the 'range' chart it is possible with the help of tables that have been constructed by statisticians to draw on the other a pair of limits within which the dots should fall. Perhaps they will not do so; say the dots for fuses made by one particular tool fall outside. This gives sure knowledge that something is wrong with the process—the statistician cannot say what, though I was told of cases where he could go so far as to say either that there was only one thing wrong or else certainly several things wrong. It is for the engineer to re-examine his process and find the fault. Next, of course, similar work must be done at the other fuse settings. Suppose this has been done, and that the whole system is in control, and that the accuracy is good enough for the user. After this, much less sampling is needed, but every so often a group should be taken and tested in the same way; I will not go into detail, but this sampling will nearly always give warning of pending trouble, before the trouble is so bad that the fuses would actually fail in passing their test.

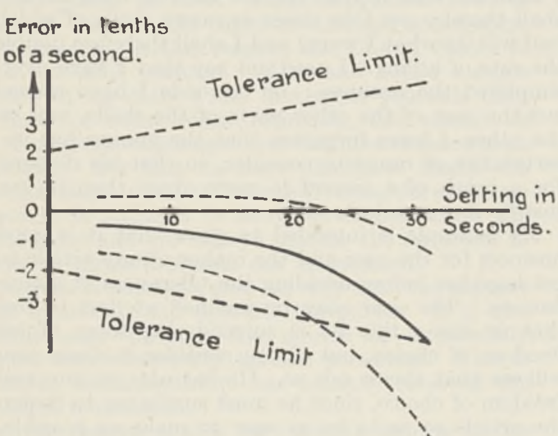



Fig. 1.

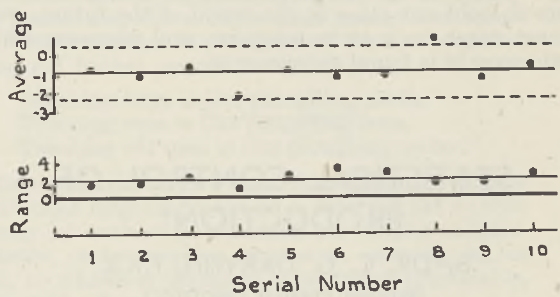



Fig. 2.

Returning to the actual history of these fuses, Simon plotted average time and 'range' for batches of five fuses at various timings of the fuse, and got the curves I show (Fig. 1) for the average timing and for the dotted lines on either side between which all values will fall. This was for the fuses of one manufacturer, and there were similar curves, some up and some down, for the others. It will be seen that the fuses mostly failed to fulfil specification at 20 sec. and above, and yet their tolerance was much finer than that which had been specified. This could be overcome in a variety of ways; for example, by regraduating the markings of the time on the fuse. The result was thus that an article which systematically failed to pass its test, was, with a quite trivial change, found to be actually better than had been asked for. I may add that as a consequence of his work Simon found that the old tests had destroyed something like twice as many of the fuses as is now necessary.

I will conclude with a few general remarks. When I first came across the method I knew it was of the greatest importance, but I doubted if the middle of a war was the time to start it. I asked this very question of two of the experts in the Bell Laboratory, and both contradicted this first thought of mine and said that it could be applied piecemeal to one process after another without delay in production. Then, as showing how quickly people could adapt themselves to it, I may recount an incident I experienced. Immediately after visiting Colonel Simon, I happened to be at Frankford Arsenal, which in peace-time was the principal factory making ammunition for the U.S. Army. As such, I expected it would be rather conservative and that it might be suspicious of new-fangled methods, and I therefore asked one of the chief colonels there what he thought of these methods, which, I may emphasize, are quite new in their application to military stores. His reply was that it seemed the only sensible way of doing the business.

Another point well worth consideration is that many works have already existing a mass of data of the kind used by the statistician, but they do not submit them to the same analysis. In such cases the change of procedure would be only slight, and a greatly improved control would be obtained by applying the statistical method.

Thirdly, the method gives information about the size of sample that should be tested. In some cases it may be inadequate, whereas in others it will be found that testing a much smaller sample would give sufficiently reliable information on the quality of the whole.

We cannot, of course, expect that the method is a panacea for all troubles, and I expect in some cases it will be tried and prove unsuitable; but I will venture the forecast that the opposite will much more often be the case, and that many processes to which at first sight it seems inapplicable will later be found to benefit greatly by the introduction of statistical control.

# OBITUARIES

## Mr. W. P. Pycraft

William Plane Pycraft, who died on May 1, was a well-known ornithologist and comparative anatomist. He was born at Great Yarmouth in 1868 and while a schoolboy was attracted to natural history by the wild life of the Norfolk Broads. He became a keen observer of all living things, but devoted himself specially to birds. After leaving school, he desired to follow natural history as a profession, and started as a private pupil with the curator of the Leicester Museum, where he learned the art of preserving and preparing animals for study and exhibition. In 1892 he was introduced to Prof. (afterwards Sir) Ray Lankester, who invited him to be his assistant in making preparations for the Oxford University Museum. While thus occupied he attended Lankester's lectures and demonstrations, and thus extended his outlook by acquiring a good knowledge of the structures and relationships of animals. When Lankester was appointed director of the British Museum (Natural History) in 1898, Pycraft accompanied him to London and became his temporary assistant there. Soon afterwards he joined the permanent staff of the Zoological Department of the Museum, where he remained as an assistant until his retirement in 1933. He spent his later life at Longcross near Chertsey, Surrey, in surroundings where he could continue the field observations which he had begun in early youth.

The original researches carried out by Mr. Pycraft were concerned chiefly with the anatomy of birds. His first paper, published in the *Ibis* in 1895, described and discussed the arrangement of the feathers in the Tinamous, and in 1898 he contributed a memoir on the feathering of the owls to the *Transactions of the Linnean Society*. Between 1898 and 1907 he wrote a valuable series of nine papers on the osteology of birds published in the *Proceedings of the Zoological Society*, and in 1900 he discussed the morphology and phylogeny of the Palæognathæ and Neognathæ in an extensive memoir in the *Transactions* of the same Society.

Mr. Pycraft also began to take much interest in the variations of the human skull, and in 1915 he proposed to substitute for the Frankfort base-line another line which passed wholly through the cranium avoiding the upper part of the face. This proved to be not generally acceptable, and he returned to the subject in a paper in *Man* in 1925. He wrote several accounts of human skulls, the most note-worthy being his description of the Boskop fossil from South Africa in the *Journal of the Royal Anthropological Institute* of 1913, and a description of the Rhodesian fossil skull in a British Museum volume in 1928. He took part in several discussions on the Piltdown skull, and in 1917 published in *Science Progress* an account of the lower jaw which pointed out its differences from the jaw of an ape.

Mr. Pycraft was also a prolific writer of popular books and articles on natural history, which brought him a large correspondence and not infrequently led to the discovery of new facts. His first popular books were the small "Stories of Bird Life", "Fish Life" and "Reptile Life", published by Newnes in London between 1900 and 1905. "A History of Birds", a more serious work, appeared in 1910, and his "Birds of Great Britain" followed in 1934. His volumes on the "Infancy of Animals", "The Courtship of Animals" (1913) and "Camouflage in Nature" (1925) are especially readable and contain original observations. His weekly article in the *Illustrated London News* was a much-appreciated feature of this paper for many years, and attracted wide attention.

In all his writings Mr. Pycraft showed great interest in the possible mode of evolution of the various structures and habits which he described, and an address on "Some New Aspects of Evolution" which he delivered to the Norfolk and Norwich Naturalists' Society in 1935 was reprinted in the annual report of the Smithsonian Institution, Washington, in the following year. His style was sometimes forceful, for when he had formed an opinion he could not readily be persuaded that it might be mistaken; but he had a versatile mind and was always inspiring. He is mourned by a large circle of friends who learned the more to appreciate him the closer they were associated with him.

A. S. Woodward.

We regret to announce the following deaths:

Prof. Charles Cohen, formerly of the Brussels Pasteur Institute, aged sixty-one.

Dr. R. L. Ditmars, the distinguished herpetologist, formerly curator of mammals and reptiles in New York Zoological Park, on May 12, aged sixty-five.

Dr. Bernhard Fischer-Wasels, professor of morbid anatomy at Frankfort-on-Main, president of the German Pathological Society and editor of the *Frankfurter Zeitschrift für Pathologie*, aged sixty-five.

Sir James Larmor, F.R.S., formerly Lucasian professor of mathematics in the University of Cambridge, on May 19, aged eighty-four.

Prof. B. Malinowski, professor of anthropology in Yale University, formerly University professor of anthropology in the London School of Economics, on May 16, aged fifty-eight.

Dr. C. Hart Merriam, founder in 1885 and until 1910 chief of the United States Bureau of Biological Survey, now known as the Fish and Wild Life Service, on March 19, aged eighty-six.

Dr. John Miller, director of aircraft production (factories), formerly chief engineer, London and North-Eastern Railway (N.E. Area), on May 16.

The Rev. T. E. R. Phillips, a past-president of the Royal Astronomical Society and of the British Astronomical Association, on May 13, aged seventy-four.

Dr. G. G. Stoney, F.R.S., who for many years was associated with C. A. Parsons and Co. Ltd., particularly in connexion with the development of the steam turbine, on May 15, aged seventy-eight.

Prof. G. A. Witherington, formerly professor of mathematics in the Royal Naval College, Greenwich, on May 1, aged sixty-nine.

Prof. W. J. Young, professor of biochemistry in the University of Melbourne, aged sixty-three.

# NEWS and VIEWS

## World Mineral Resources and Post-War Needs

IN the fourth clause of the Atlantic Charter, Mr. Roosevelt and Mr. Churchill state "that they will endeavour, with due respect for their existing obligations, to further enjoyment by all States, great or small, victor or vanquished, of access, on equal terms, to the trade and to the raw materials of the world which are necessary for their economic prosperity". The Division for the Social and International Relations of Science of the British Association is therefore arranging a conference on "Raw Materials and Industrial Needs : Mineral Resources and Outlook", to be held in London at or about the end of July. As Sir Richard Gregory, president of the Association, pointed out in submitting the proposal for such a conference, the world's natural resources —both organic and inorganic—are much too large a subject to be dealt with in a single conference, but a survey of the present position of minerals of industrial importance, with suggestions for further investigations into their geographical distributions and research into the production of substitutes, will show the close contact between science and fundamental national and international problems. The Conference will indeed be similar to a joint meeting of the Sections of Geology, Geography, Physics and Chemistry at an annual assembly of the British Association, and its papers will be of the nature of contributions to a report upon the distribution, output and industrial uses of the chief mineral deposits of the world. Such energy resources as solid, liquid and gaseous fuels belong to a class of their own, and the facts relating to them have been brought before a number of World Power Conferences. Whatever is known about the nature, distribution and uses of minerals in the earth's crust has been gained by scientific inquiry, and the knowledge is international in origin and scope. By presenting the chief facts as to natural resources of minerals and their geographical control, such a conference can do much to promote recognition of the interdependence of nations and the need for collaboration between them.

## Scientific Workers of the Argentine

DURING last March, Prof. E. D. Adrian, professor of physiology in the University of Cambridge, paid a visit to the Argentine at the invitation of the Argentine National Academy of Medicine. He was welcomed with much cordiality and was frequently assured of the sympathy of Argentine medical men and scientific workers for the Allied cause. Towards the end of his visit, Prof. Adrian was asked to receive a deputation from the Comisión Sanitaria Argentina de Ayvda a las Democracias (Health Committee to Aid Democratic Countries). This deputation asked Prof. Adrian to convey a message of solidarity to members of the medical profession and scientific men in Great Britain. The Committee said that the example set by British men of science working in their laboratories and clinics, holding congresses even in war-time to promote the application of scientific discovery to the progress of mankind, and arranging the co-ordination of the scientific work of Great Britain, the U.S.S.R. and the United States, strengthens their faith in the triumph of democracy through science. This very cordial message from the Argentine will be received with much satisfaction by scientific workers in Great Britain, who will be encouraged to pursue the course they have set for themselves in helping to rid the world of totalitarianism.

## Illuminating Engineering Society

AT the annual general meeting of the Illuminating Engineering Society on May 12, Mr. W. J. Jones (president) was able to present an encouraging record for the past session. A feature has been the further development of centres and groups, of which there are now eleven, and which are expected to do useful work in studying the lighting requirements of special local industries. The position in regard to the Society's work on A.R.P. lighting, undertaken jointly with the Ministry of Home Security, has for the time become stabilized, but a number of committees are now exploring various aspects of lighting in relation to after-war reconstruction. The "Recommended Values of Illumination" put forward by the Society (I.E.S. Code) has been adopted by the Ministry of Supply, the Admiralty and the Ministry of Aircraft Production, in applying the Factories (Standards of Lighting) Regulations (1941). Many members of the Society are engaged in the task of designing lighting installations for factories engaged on national work. The Society in 1940 initiated the practice of conferring fellowship on those of its members having the requisite technical qualifications. The number of fellows created is now 96—rather less than ten per cent of the membership of the Society. The new president for the forthcoming session is Mr. R. O. Ackerley.

The Illuminating Engineering Society made a practice, in the years preceding the War, of inviting an eminent expert from abroad to deliver an address on the occasion of each annual meeting. This practice is no longer possible in the present circumstances, but its own members have filled the gap. The lecturer on May 12 was Mr. G. H. Wilson, who took for his subject "Street Lighting: Past, Present and Future". Mr. Wilson remarked that the period 1928–38 was one of great technical progress. An outstanding event in 1928 was the erection of fifty model lighting installations illustrating the eight classes of the British standard specification. This led to a recognition of the importance of road surface brightness as a factor in relation to visibility. During the ten-year period two new sources, the sodium and mercury vapour lamps, were introduced. Their unusual spectra raised new problems and their shape and size made necessary considerable changes in the design of lanterns. Attention was also devoted to the problem of the siting of posts, which was discussed in the report of the departmental committee appointed by the Ministry of Transport appointed in 1934. After referring briefly to lessons to be learned from our experience of the low orders of illumination available during the present black-out, Mr. Wilson reviewed after-war problems. He pointed out the possibilities of the new fluorescent lamps, expressing the belief that technical resources are enormous. The future of street lighting, he affirmed, depends largely on the extent to which the scientific attitude of mind is employed in the application of the achievements of research—for example, in connexion with bold experiments in town planning.

## Fluorescent Lighting

A PAPER on this subject, read recently by L. J. Davies, H. R. Ruff and W. J. Scott before the Institution of Electrical Engineers in London, gives a brief history of fluorescent lighting and follows this by a description of a typical mains-voltage tubular fluorescent lamp and the principles of its operation. The new fluorescent lamp combines the high efficiency of the straight electric discharge lamp, with much of the convenience of operation of the incandescent lamp, while possessing, in addition, special characteristics of low brightness, exceptional colour-rendering power, and comparative absence of radiant heat. The 200/250-v., 80-watt lamp and its auxiliaries, marketed in Great Britain in March 1940 to improve factory lighting in blackout conditions, are described in detail and the characteristics and components of the complete unit are explained. This lamp is 5 ft. long and 1½ in. in diameter, taking 0·8 amp. at a lamp voltage of 115; its nominal luminous efficiency is 35 lumens/watt and its mean brightness is 3·3 candles/sq. in. Its high electric power/light conversion ratio is examined together with the conventions whereby these are assessed. The paper concentrates attention upon the practical features of the lamp, but gives a sufficient description of the physics involved to promote an appreciation of both the present characteristics and future importance of this type of light source. The authors conclude that the lamp is satisfactorily fulfilling a present industrial need, and that its quality has been so greatly appreciated that it is undoubtedly the forerunner of a new series of lighting lamps.

## Industry in Scotland

IN the discussion in committee in the House of Commons on May 12 on the estimates for the Scottish Home Department, Mr. T. Johnston, Secretary of State for Scotland, gave a survey of industrial development in Scotland since 1918. He referred to the advisory committee on Scottish industry which has been set up, and expressed the hope that by its means the industrial aftermath in Scotland of 1914–18, due to concentration on heavy industries for export, will be avoided. In the course of the discussion, Sir John Graham Kerr put in a plea for the development of a variety of small light industries. The industrial belt of Scotland has grown in its present position because the sources of power are close at hand. Industry tends to drift to the more populous parts of the country—in Scotland to the south—and one way of stopping this drift is to carry power all over the country. The transport of power is of vital importance. In the form of coal and oil, subject to road or rail transport, power is only distributed with difficulty and at relatively high cost. The newer method of distribution of power through an electric grid might have a tremendous influence on Scotland and its industries, for, by these means, the site of industry is no longer tied to the source of power, and the feeding of small units becomes feasible.

## Austrian Scientific Workers in Great Britain

AN Association of Austrian Engineers, Chemists and Scientific Workers in Great Britain has recently been formed. The main activities of the Association will be to assist members in their professional work and interests, to represent them with the authorities, to promote contact and relations with British colleagues and to form a link with British scientific and technical institutions. Lectures, courses and discussions will be held and will give opportunities for the exchange of views and to discuss matters of mutual interest. It is hoped that the Association may assure that better use is made of the knowledge and abilities of Austrian engineers, chemists and scientific workers who are anxious to assist in the war effort. The acting chairman of the Association is Dr. F. Ehrenfest-Egger; inquiries should be sent to the honorary secretary, Mrs. K. Hilfreich, 133 Hatherley Court, London, W.2. Lectures are being given on the first Monday of each month. Every Monday, commencing June 1, at 7 p.m., a club-room will be open for members of the Association at the Austrian Centre, 69 Eton Avenue, N.W.3, where there will be opportunities to read technical periodicals and to meet other colleagues.

## A Relic of Dr. John Dee

THE sale by Messrs. Sotheby of an interesting relic of the mathematician and astrologer Dr. John Dee, who was patronized by Queen Elizabeth, was recorded in *The Times* of May 5. The relic is a gold disk 3½ in. in diameter weighing 1 oz. 4 dwt. 5 gr., bearing the London date letter for 1589 and engraved with a diagram of the "Vision of the Four Castles" which appeared to his medium Edward Kelley, on the morning of June 20, 1584, at a house in St. Stephen's Street, Cracow, where the two men were staying. The diagram is reproduced in "A True and Faithful Relation of what passed for many Years between Dr. John Dee . . . and some Spirits" published in 1659 by Dr. Meric Casaubon. The disk was bought by the British Museum for £230.

## Institution of Electrical Engineers Awards

THE Council of the Institution of Electrical Engineers has made the following awards of premiums for papers read during the session 1941–42, or accepted for publication: *Institution Premium* to Mr. J. M. Meek; *Ayrton Premium* to Mr. J. S. Forrest; *Fahie Premium* to Dr. W. G. Radley and Mr. E. P. G. Wright; *John Hopkinson Premium* to Dr. A. L. Williams and Mr. L. E. Thompson; *Kelvin Premium* to Mr. E. Colin Cherry; *Overseas Premium* to Prof. K. Aston and Mr. M. V. Kesava Rao; *Extra Premiums* to Dr. A. H. M. Arnold, Mr. G. W. Bowdler, Messrs. G. W. Bowdler and W. G. Standring, Mr. R. Davis, Dr. H. D. Einhorn and Prof. B. L. Goodlet, Dr. Hackett and Mr. A. M. Thomas, Mr. E. A. Richards, Mr. V. Sia, Messrs. R. C. Woods and A. S. MacDonald; *Installations Section Premiums* to Messrs. L. J. Davies, H. R. Ruff and W. J. Scott (Crompton Premium), Mr. R. Grierson; *Meter and Instrument Section Premiums* to Messrs. E. A. Burton, J. S. Forrest and T. R. Warren (Silvanus Thompson Premium), Mr. D. J. Bolton; *Transmission Section Premiums* to Mr. J. W. Leach (Sebastian de Ferranti Premium), Messrs. W. Casson and F. H. Birch; *Wireless Section Premiums* to Mr. O. S. Puckle (Duddell Premium), Dr. D. C. Espley and Mr. D. O. Walter (Ambrose Fleming Premium), Messrs. J. E. Thwaites and F. J. M. Laver.

## Earthquake in Ecuador

REPORTS have been received from Guayaquil, the chief port of Ecuador, that a severe earthquake took place in the interior on the night of May 13–14. Guayaquil itself was severely shaken for some 60 seconds according to human perception, and buildings collapsed. It is estimated that about sixty people lost their lives. Mr. J. J. Shaw's seismograph at West Bromwich recorded a severe earthquake at 2h. 25m. 52s. U.T. on May 14. Further news is awaited. It will be recalled (NATURE, Feb. 11, 1939, p. 238) that several earthquakes shook a considerable area in Ecuador including the towns of Vallechillos and Quito on the night of February 1, 1939. On that occasion the volcano Cotopaxi erupted.

## Earthquakes Registered at Kew

Two strong distant earthquakes have recently been recorded at Kew Observatory. The first, on April 8, 1942, began recording on the vertical component at 15h. 53m. 58s. U.T. impulsively, and on the N and E components at the same time $P$ was emergent. The shock probably originated some 11,000 km. distant, at maximum attained an amplitude of 410 $\mu$ at Kew, and finished recording at 19h. 20m. U.T. The second, on April 13, began recording impulsively on all three components at 07h. 55m. 48s. U.T. from an epicentral distance of 6,130 km., attained a maximum amplitude of 94 $\mu$ and finished recording at 10h. 00m. U.T. All readings are tentative.

Between April 15 and May 5 eighteen earthquakes were registered by the seismographs at Kew Observatory. All these shocks gave rise to small amplitudes with the exception of the earthquake of April 20. This began recording at 08h. 52m. 40s. U.T. and finished recording at 10h. 25m. U.T. The interpretation of this record is at present tentative, though an epicentral distance of 125° is possible.

## The Ipswich Man

THE skeletal remains known as the Ipswich man, which have been housed at the Royal College of Surgeons, London, since they were discovered in Bolton and Co's brickfield, Ipswich, in 1911, have now, through the instrumentality of Mr. Reid Moir, and the kindness of the president and Council of the College, been presented to the Ipswich Museum. Since 1911 a great deal has been discovered regarding the age of these remains, which were thought originally to be older than the Upper Chalky Boulder Clay—a deposit of one of the major glaciations of East Anglia. It now seems clear that the skeleton is referable to one of the prehistoric floors situated in the slopes of the adjacent valley, and examples of flint implements, etc., found in these floors are exhibited with the human bones in the entrance hall of the Ipswich Museum. Though all the geological and archæological aspects of this matter are not yet completely understood, it is highly probable that the Ipswich man lived in the earlier part of Upper Palæolithic times, and is of considerable antiquity.

## Announcements

PROF. E. H. ALTON, registrar and professor of classics in Trinity College, Dublin, who represented the College in the Dail from 1923 until 1937, when the University representation was abolished and he was elected to the Irish Senate, has been appointed provost of the College in succession to the late Dr. W. E. Thrift.

MR. HARRY BREARLEY, the well-known steelmaker of Sheffield, has been elected to honorary membership of the Iron and Steel Institute. Mr. Brearley's recently published autobiography was reviewed in NATURE of April 11, p. 397.

THE British Standards Institution has been recognized by the Government as the sole organization for the issue, in consultation with any Government, professional or industrial bodies concerned, of standards having a national application. In regard to the preparation and issue of codes of practice for building and civil engineering work, the Minister of Works and Buildings is making special arrangements by the appointment of a representative committee with which the Institution is co-operating.

As from the commencement of next session, the name of the Meter and Instrument Section of the Institution of Electrical Engineers will be altered to "Measurements Section". The Section will include within its scope all matters relating to electrical measurement and control, the design and application of the apparatus, and the materials employed in connexion with them.

A DISCUSSION on "Soviet Metallurgy", arranged by the Faculty of Science, Marx House, will be held at the London School of Hygiene and Tropical Medicine, Keppel Street, W.C.1, on May 28 at 7 p.m. The discussion will be based on a paper by Mr. Tom Barker, who took a leading part in the creation of the great Kuzbas Metallurgical Combine in Siberia. Tickets of admission, including a copy of Mr. Barker's paper, can be obtained from Marx House, Clerkenwell Green, London, E.C.1, price 2*s*.

# LETTERS TO THE EDITORS

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

## Cosmic Rays and Magnetic Storms

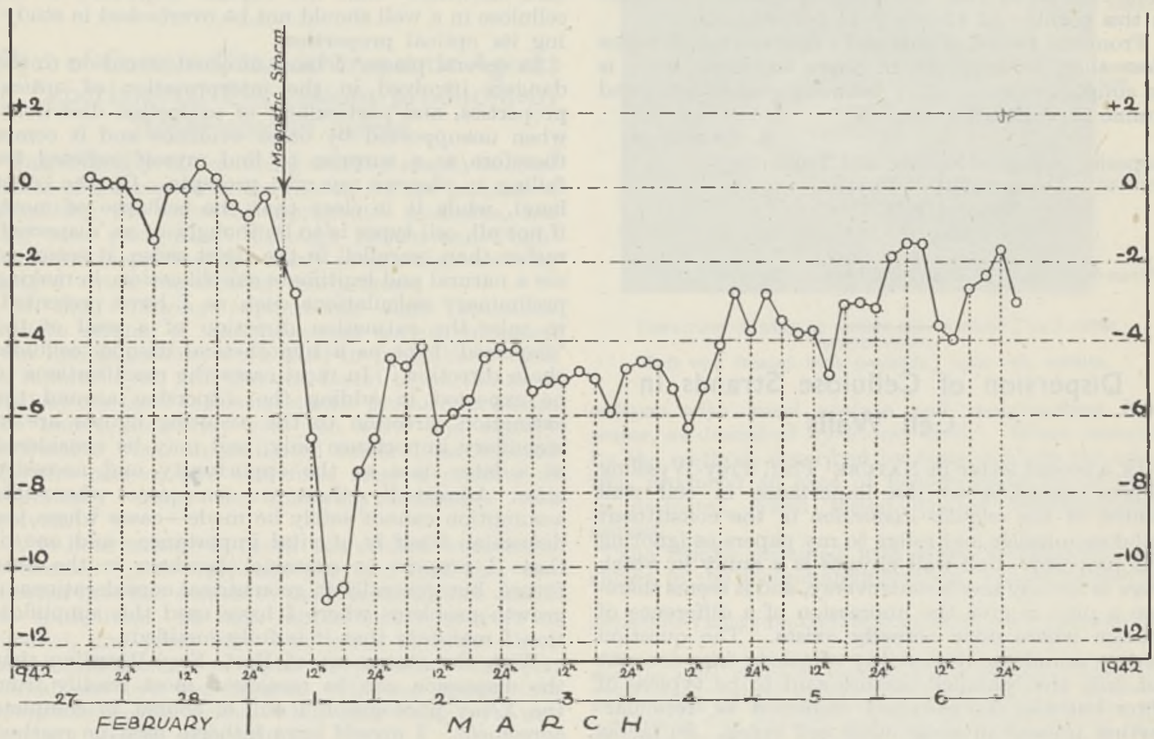
Hourly records of the intensity of cosmic rays have been made during the past year. The apparatus used consisted of a battery of Geiger counters registering about 25,000 threefold coincidences each hour. The circuit used gave a negligibly small number of casual coincidences. No absorbing screens were used, but preliminary experiments with lead showed that the thickness of the counter walls was sufficient to cut out all radioactive rays and the very soft cosmic rays. To count such a rapid rate of coincidences without loss, a Wynn-Williams' scale-of-two counter[1] was used. For this circuit the resolving time is so reduced that, at the counting rate of 400 per minute, the number of particles which is missed is not more than one per thousand. The number of pulses recorded by the telephone counter is photographed automatically every hour.

The whole apparatus was prepared in the Physical Laboratories at the University of Manchester, and the observations are being made in a room at the top of the Imperial College of Science, South Kensington.

The observations began in February 1941, but the photographic recording started only in April. The first phenomenon to which attention was given was the so-called temperature effect of cosmic rays, not only because of its importance in itself, but also so as to be able to eliminate this influence from the observations and thus to establish more accurately the other time variations. A detailed account of the results of the work will be given elsewhere. It will be shown that the changes from day to day in cosmic rays, when corrected for changes in atmospheric absorption, are well correlated with the variations in height of the atmospheric layer of given pressure at which mesons are supposed to be formed. This is consistent with Blackett's explanation on the basis of the instability of the meson.

When this effect is eliminated from the observations, then the well-known world-wide changes appear which are associated with the disturbances of the terrestrial magnetic field. These changes are being studied in detail.

In this letter an account is given of the remarkable changes of cosmic ray intensity associated with the magnetic storm of March 1, 1941. The figure shows the observed time variation from February 27 to March 6. The decrease of nearly 12 per cent on March 1 is certainly unusually great, and, so far as is known to us, is the largest variation of this kind so far recorded.




Percentage deficiencies of cosmic rays during Feb. 27–March 6.

The points on the diagram represent the mean value of the cosmic ray intensity during a period of three hours. The scale is in percentages from the normal value. The cosmic ray disturbance began shortly before the onset of the magnetic storm. From information kindly given by the Royal Observatory, the latter began at 7h. 27m. on March 1. Allowing for the statistical fluctuations ($\pm$ 0·36 per cent for the three-hour period), it appears that the cosmic rays disturbance began about one hour earlier. The storm subsided rapidly after 1 a.m. on March 1 and finished three hours later. After the first rapid recovery of about half the initial drop, which occurred within twenty hours of the onset, the recovery was very slow and was not complete by the sixth day.

It is remarkable that the magnetic storm was not so intense as some others which have been accompanied by a less violent disturbance in cosmic rays. The extreme range in the horizontal magnetic force $H$ was 512 $\gamma$, equivalent to less than 3 per cent of the $H$ value. Nevertheless, it appears that the bright eruption which began to be observed on the sun 19h. 27m. earlier was one of the most extensive recorded, lasted for an unusually long time and produced a much larger magnetic effect than usual. Also a special feature of the storm was a series of 27 giant pulsations in $H$ and $V$ which lasted from 15h. 5m. to 17h. 3m. on March 1, and are, it seems, unique in the Abinger records. It was just in this interval when the number of cosmic particles reached its lowest value. The mean of the two-hourly observations centred at 16h. and 17h. represents a decrease in this number of $12 \cdot 0 \pm 0 \cdot 44$ per cent.

From the record of this and other storms, it seems clear that, at any rate in these latitudes, there is no simple proportionality between geomagnetic and cosmic rays disturbances.

A. DUPERIER.
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Department of Physics,
London, S.W.7.
May 6.

[1] Wynn-Williams, C. E., *Rep. Prog. Phys.*, 3, 239 (1937).

## Dispersion of Cellulose Strands in Cell Walls

IN a recent letter in NATURE[1] Prof. Frey-Wyssling directs attention to the importance in cell wall studies of the *angular dispersion* of the constituent cellulose micelles and refers to my papers as ignoring this property. Cell wall physics is a study in which there is already much controversy, and it seems therefore a pity to give the impression of a difference of opinion where none actually exists. The question centres round the desirability of taking into account not only the 'parallel' texture said to be typical of fibres but also the so-called 'dispersed' or 'reticular' texture present in some other cell types. So far as I am concerned, the difference between 'parallel' and 'dispersed' textures is one of degree only and actually, far from taking ". . . only parallel textures into account . . .", I have yet to investigate a single cell in which I am convinced that this texture does occur in anything like a strict sense. In some fibres[2] and wood cells[3], for example, where Frey-Wyssling himself supports parallel texture, and in some collenchyma[4], my collaborators and I have shown that the angular dispersion of the cellulose micelles about the extinction direction may be considerable and is probably of importance in explaining certain optical properties of the wall. These latter papers have apparently not been available to Prof. Frey-Wyssling since his citations of my papers cease in 1939.

In the paper from which he quotes, however, I made the first suggestion, later applied also to the conifer tracheid[3], that certain optical properties are to be explained in terms of the angular dispersion of the cellulose matrix. In cross-section the walls of some of the cells referred to (vessels of some ring-porous trees) show optical discontinuities, and concerning the explanation of this fact I remarked: ". . . the observed phenomena may be accounted for either by a variation in chain direction from layer to layer, by a variation in chemical nature, by 'a change in the *dispersion* of a single direction', by a variation in the amount of cellulose present in the crystalline phase, etc. Any one of these possible causes 'may be accepted only when the others have been ruled out'." The sections quoted by Prof. Frey-Wyssling (enclosed in single inverted commas) are therefore misleading. The full quotation, and even more strongly the sense of the whole paper, makes it abundantly clear that here I am questioning not the existence of dispersion or even of changes in dispersion but whether these can explain the phenomena under review. In fact the purpose of this paragraph was to emphasize what I have repeatedly said elsewhere—that the *angular dispersion* of the cellulose in a wall should not be overlooked in studying its optical properties.

In several places[6] I have directed attention to the dangers involved in the interpretation of optical properties, and particularly of extinction directions, when unsupported by other evidence and it comes therefore as a surprise to find myself indicted for failing to observe my own precepts. On the other hand, while it is clear that the cellulose of most, if not all, cell types is to be thought of as 'dispersed' rather than 'parallel' in the strict sense, it seems to me a natural and legitimate simplification, in making preliminary calculations such as I have presented, to take the extinction direction of a wall of the 'dispersed' type as a hypothetical unique 'cellulose chain direction'. In most cases the modifications to be expected in adding the dispersion around the extinction direction to the resulting figures are of secondary importance only, and may be considered at a later date as the opportunity and necessity arise. Admittedly, there are some cases where this assumption cannot safely be made—cases where the dispersion itself is of vital importance—and one of these I propose to examine elsewhere in the near future, but generally in geometrical considerations of growth problems where I have used this simplification I maintain that it is fully justified.

With the statement of Prof. Frey-Wyssling that the dispersion can be measured most readily from the X-ray photograph I am, of course, in complete agreement. I myself have hitherto used no method other than this, but of the available indirect methods I would prefer the fluorescence staining method of Morey[7] to the chlorzinc iodide method of Frey-Wyssling since the latter technique introduces possible changes in dispersion on swelling. I would like, however, to point out further that Prof. Frey-Wyssling is in error when he implies that no X-ray photographs are available of single cell walls. A photograph has already been published of the X-ray diagram of an oak vessel[5] and of what corresponds to single walls of a conifer tracheid, and in each of these photographs the angular dispersion can be measured approximately. Under some conditions, moreover, the dispersion can be detected and measured in some cell types even though bundles of cells rather than individual walls are photographed[4].

There is, therefore, no fundamental difference between Prof. Frey-Wyssling's attitude and mine concerning the 'dispersed' texture. The 'reticular' texture figured so often by Frey-Wyssling is, however, of a somewhat different type and I must confess some hesitation in accepting this as a possibility as it stands. That there occurs in latex vessels and in most cells with primary walls a high degree of dispersion is well established, but this by no means suggests that the cellulose complex is reticular.

R. D. Preston.

Department of Botany,
University of Leeds.
April 22.

[1] Frey-Wyssling, A., Nature, 149, 384 (1942).
[2] Kundu, B. C., and Preston, R. D., *Proc. Roy. Soc.*, B, **128**, 214 (1940); Preston, R. D., and Allsopp, A., *Biodynamica*, No. 53 (1939); Preston, R. D., *Proc. Roy. Soc.*, B, **130**, 103 (1941).
[3] Preston, R. D., *Proc. Leeds Phil. Soc.*, Sci. Sec., **3**, 546 (1939).
[4] Majumdar, G. P., and Preston, R. D., *Proc. Roy. Soc.*, B, **130**, 201 (1941).
[5] Preston, R. D., *Ann. Bot.*, N.S., **3**, 507 (1939).
[6] Preston, R. D., *Proc. Roy. Soc.*, B, **125**, 372 (1938); *Phil. Trans.*, B, **224**, 131 (1934): *Biol. Revs.*, **14**, 281 (1939). Preston, R. D., and Astbury, W. T., *Proc. Roy. Soc.*, B, **122**, 76 (1937).
[7] Morey, D. R., *Text Res.*, **4**, II (1934).

## Effect of Certain Chemotherapeutic Agents on Experimental Eye Lesions produced by *Staphylococcus aureus*

The difficulty of producing uniform staphylococcal[1] lesions of the cornea in the rabbit, for the purpose of estimating the value of therapeutic measures, has long been recognized[1] although Brown and Pugh[2] succeeded in producing a fairly satisfactory ulcer by using a strain found after many trials.

In the following experiments eight coagulase positive strains of *Staphylococcus aureus*, obtained from human lesions, were tried in preliminary tests on rabbits' eyes, and the one found to be the most virulent was used as it consistently produced a lesion. The lesions, however, were too acute and rapidly destructive when an undiluted culture was injected into the cornea, and therefore, after a number of preliminary trials, the following technique was evolved.

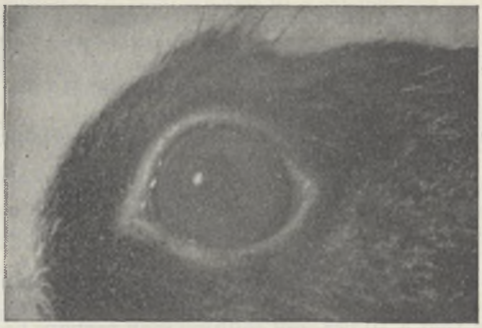
A 24-hour culture in broth was diluted with saline to contain 1,500 organisms per c.c., and this was injected into the cornea to make a small bleb under the epithelium. In each rabbit both eyes were so injected, and great care was taken to make these initial inoculations as equal as possible. In each animal one eye was treated with the appropriate chemotherapeutic agent, while the other eye was similarly treated with saline.

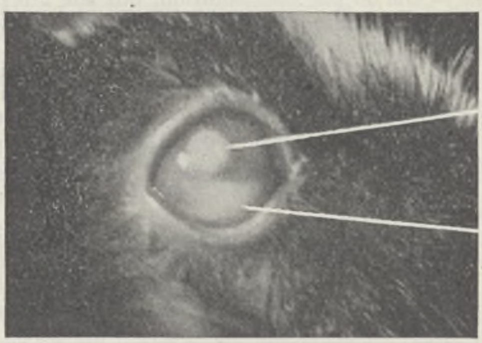
The first treatment was applied one hour after inoculation, and the treatment was usually continued at hourly intervals for the next forty-eight hours, and thereafter at less frequent intervals.

Lesions involving ulceration of the cornea invariably developed in the control eyes, and in 90 per cent of cases this was associated with development of hypopyon. In some 75 per cent of cases the ulcers were large and resulted in either extensive corneal scarring or gross destruction of the eye with or without perforation.

The lesions developed rapidly and by the end of forty-eight hours after inoculation the ulcers were already, as a rule, moderately severe and associated with hypopyon and considerable iritis.

The following chemotherapeutic preparations were used: (1) sodium sulphacetamide (this was supplied to us as a 30 per cent solution, and was used either undiluted or diluted with saline to a 10 per cent or a 2·5 per cent solution); (2) a 15 per cent solution of solubilized sulphathiazole (sulphathiazole sodium formaldehyde sulphoxylate); and (3) solutions containing penicillin, prepared by extracting from fluid




Condition of eyes of rabbit 494 on second day after inoculation.
Left eye, treated with penicillin; right eye, control.

culture with amyl acetate and re-extracting into water, as described by Florey *et al.*[3]. When assayed by the methods described by these authors the solution produced an area of bacteriostasis 21–23 mm. in diameter, when tested against *Staphylococcus aureus*.

The results obtained with these preparations are shown in the accompanying table. Penicillin proved

Effects of Various Treatments on Experimental Staphylococcal Lesions

The main subdivision has been drawn between slight and moderate, since the 'slight' lesions left scars which were small and localized and would not have seriously interfered with vision, whereas the 'moderate', and of course the more severe lesions, produced scarring which was considerably more extensive, and which would have seriously interfered with the function of the eye. The very severe lesions involved gross destruction of the eye. The figures represent the percentage of eyes, in any one group, showing the particular type of lesion.

<table>
<tr><th rowspan="2">Treatment</th><th colspan="5">Lesion produced</th><th rowspan="2">No. of animals</th></tr>
<tr><th>None or trace %</th><th>Slight %</th><th>Moderate %</th><th>Severe %</th><th>Very severe %</th></tr>
<tr><td>Control</td><td>0</td><td>8</td><td>38</td><td>38</td><td>16</td><td rowspan="4">13</td></tr>
<tr><td></td><td colspan="2">8</td><td colspan="3">92</td></tr>
<tr><td>Penicillin</td><td>31</td><td>46</td><td>15</td><td>8</td><td>0</td></tr>
<tr><td></td><td colspan="2">77</td><td colspan="3">23</td></tr>
<tr><td>Control</td><td>0</td><td>31</td><td>44</td><td>6</td><td>19</td><td rowspan="4">16</td></tr>
<tr><td></td><td colspan="2">31</td><td colspan="3">69</td></tr>
<tr><td>30% sod. sulphacetamide</td><td>19</td><td>56</td><td>19</td><td>0</td><td>6</td></tr>
<tr><td></td><td colspan="2">75</td><td colspan="3">25</td></tr>
<tr><td>Control</td><td>0</td><td>33</td><td>33</td><td>22</td><td>11</td><td rowspan="4">9</td></tr>
<tr><td></td><td colspan="2">33</td><td colspan="3">66</td></tr>
<tr><td>15% sulphathiazole</td><td>11</td><td>44</td><td>33</td><td>11</td><td>0</td></tr>
<tr><td></td><td colspan="2">55</td><td colspan="3">44</td></tr>
</table>

to be the most effective in the treatment of these staphylococcal lesions. It is probable that still better results will be produced with more concentrated solutions of penicillin. The result in one animal (494) is illustrated in the accompanying figure.

Sodium sulphacetamide (30 per cent) produced quite obvious beneficial effects.

Because of its known value against staphylococci, we expected very satisfactory results with the concentrated solution of sulphathiazole, but they were unfortunately disappointing.

Weaker concentrations of sodium sulphacetamide were tried in small groups of animals. The results suggest that a 10 per cent solution may perhaps be as effective as the 30 per cent solution, but the 2·5 per cent solution produced only slight benefit in the treatment of these lesions.

The preliminary reports of Florey *et. al.*[3] have already suggested that penicillin may be effective in the treatment of eye lesions. Our experimental results support this conclusion, and also offer evidence that sodium sulphacetamide may be of value in the local treatment of staphylococcal lesions of the human eye.

We should like to thank Dr. Oag, of the Department of Bacteriology, University of Edinburgh, for his advice and for supplying the cultures and dilutions used in these experiments. Mrs. MacNaughtan, of the Department of Bacteriology, University of Edinburgh, very kindly supplied us with penicillin solution. We are indebted to Dr. Prescott of Burroughs, Wellcome and Co. for the preparation of the solubilized sulphathiazole and to Mr. Edwards of British Schering, Ltd., for the supply of sodium sulphacetamide (albucid soluble).

We are grateful to the W. H. Ross Foundation (Scotland) for the Study and Prevention of Blindness, which defrayed the expenses of this investigation.

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[1] McDonald, R., and Pettit, H., *Arch. Ophthal.*, **21**, 817 (1939).
[2] Brown, A. L., and Pugh, J., *Arch. Ophthal.*, **16**, 476 (1936).
[3] Florey, H. W., *et al.*, *Lancet*, ii, 177 (1941).

## Microbial Synthesis and Autolysis in the Digestive Tract of Herbivora

BOTH the rumen in ruminant and the cæcum in non-ruminant Herbivora support an abundant iodophile microflora : that is, an association of taxonomically diverse species exhibiting, in consequence of the deposition within them of bacterial starch or granulose, the common characteristic of giving a blue reaction with iodine.

I have established by direct microscopic observation that the iodophile micro-organisms make their appearance on the adoption by the young animal of a vegetable diet, progressively displacing the aniodophile species prevalent in the early phases of milk-feeding ; and, furthermore, that they play a determinant role in the decomposition of starch, pectins and cellulose [1,2,3].

Thus the decomposition of these substances in the rumen or cæcum is accompanied by the deposition within the micro-organisms of an iodophile polysaccharide. The opinion has been advanced, moreover, that the products of synthesis as well as the initial products of decomposition may be utilized by the host animal[3,4,5,6]. In conformance with this opinion is the fact that very few iodophile micro-organisms are found in the fæces[3,6]. It can be demonstrated that several agents may be concerned in their elimination. Thus *in vitro* experiments on centrifuged suspensions of iodophile micro-organisms from the rumen of cattle have shown that they are not acted upon by peptic and only slowly affected by commercial tryptic preparations. Observations, however, on fresh rumen contents demonstrated that they are ingested and digested in large numbers by the ciliates present (Ophryoscolecidæ and Isotrichidæ), thus confirming Westphal's results with *in vitro* cultures of rumen protozoa[7]. It was found, moreover, that the ciliates, unlike the iodophile micro-organisms, were readily acted upon by enzyme preparations. Similarly, examination of post-mortem material showed that in the abomasum the majority of the rumen Protozoa had been digested (cow and sheep). In the cæcum of the horse a digestion of bacteria by ciliates (Cycloposthidæ, Paraisotrichidæ, Butschilidæ, etc.) had also been recorded[6]. In the guinea pig, however, the Entodiniomorpha are not represented and the small flagellates present (Hexamita, Trichomonas, etc.) do not play an important part in the removal of bacteria[5]. Finally, in the adult rabbit, Protozoa may be altogether absent[4]. In these cases other agencies must be invoked to account for the observed elimination of iodophile micro-organisms. An intensive action of digestive enzymes is precluded by the position of the cæcum and colon. It had been conjectured, therefore, that in these conditions bacterial autolysis might play an important part[6]. This hypothesis has now been tested as follows :

Cæcal contents of a guinea pig were removed and a portion suspended in 10 per cent formalin (control). The remaining portion was diluted with saline saturated with chloroform and divided into two fractions. One of these was heated to 100° C. for 2 min. Both were incubated at 37° C. for 12 hr. The formalized control showed an abundant disintegration of starch grains which were entirely surrounded by iodophile micro-organisms. In the boiled fraction the iodine reaction of the iodophile micro-organisms was unchanged, whereas in the unboiled fraction it had entirely disappeared. Since the reaction of the grains was unimpaired the absence of free amylase could be inferred. The changes witnessed were, therefore, attributed to autolysis. Identical results were obtained in parallel experiments performed with rumen contents of cattle.

It is clear, therefore :

(*a*) That several agencies may co-operate to a varying degree in different Herbivora in the elimination of the iodophile micro-organisms, namely :
(1) Ingestion and digestion by Protozoa.
(2) Action of digestive enzymes.
(3) Bacterial autolysis.

(*b*) That the mode of elimination in ruminants is more complex than in non-ruminants.

(*c*) That in the simplest instance elimination may be effected by bacterial autolysis alone.

Any effort, therefore, to elucidate the complex problems concerned in the effective utilization of a particular diet must inevitably take account of the interadjustment of the processes enumerated, that is, of the actual mode of transference of the available polysaccharides from the vegetable materials ingested to the tissues of the animal. No mention has been made in this communication of the protein syntheses integral to the maintenance of a microbial population in the cæcum or rumen; nor of the initial nitrogen sources from which they may be able to proceed. A detailed study of some microbiological aspects of the problem is, however, now being made in close collaboration with the biochemical investigation of *in vitro* incubations of rumen contents in progress at the Hannah Dairy Research Institute[3,8].

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[1] Baker, F., and Martin, R., NATURE, 141, 877 (1938).
[2] Baker, F., *Sci. Prog.*, No. 134 (Oct., 1939).
[3] Baker, F., NATURE, 149, 220 (1942).
[4] Baker, F., and Martin, R., *Zent. Bakt.*, Ab. II, 96, 18 (1937).
[5] Baker, F., and Martin, R., *Zent. Bakt.*, Ab. II, 97, 201 (1937).
[6] Baker, F., and Martin, R., *Zent. Bakt.*, Ab. II, 99, 400 (1939).
[7] Westphal, A., *Zent. Parasitenk.*, 7, 71 (1934).
[8] Owen, E. C., Smith, J. A. B., and Wright, N. C., NATURE, 147, 710 (1941).

## Spore Dispersal in the Mucorales

THE problem of spore dispersal presented in my last communication[1] is now partially solved, as a result of spore-blowing experiments and the observation of cultures grown in cemented glass cells with coverslip surfaces.

The chief fact which emerges is that the Mucor sporangium is primarily a water dispersal mechanism, and that Mucor spores, for the most part, become air-borne only after preliminary separation in water, and then chiefly on the surface of soil dust particles and hyphal fragments.

The sporangial membrane in Mucors breaks up and eventually disappears in contact with water. In Mucors with thin walls, usually described as 'diffluent' (for example, *M. hiemalis*), this process is rapid, and if the sporangium remains untouched in moist air it gives place to a conspicuous sporangial drop. The stalk on losing turgidity collapses and usually brings the drop against a solid surface, whereupon it spreads out rapidly owing to surface tension. The word 'burst' is inappropriate to describe this process. On drying down the spore masses remain firmly stuck together with mucilage which, however, is readily dissolved and the spores separated in water.

Growing on organic matter in soil the sporangiophores can be seen to form a network linking the soil particles, against which the sporangia are inevitably brought into contact at some stage, with the presumed result that the spores are dispersed by the soil water when present.

Spore-blowing experiments on this type of Mucor show them to be complete *spore retainers* in air. The function of the sporangiophore is thus seen to be that of ensuring some preliminary dispersal of the spore masses, rather than that of raising the sporangium into the air. Its sensitiveness to light in the early stages of growth may also have the effect of counteracting the down-washing of water, and raising the spore masses so as to keep them in the upper layers of the soil, where both aeration and organic matter provide more favourable conditions for the growth of Mucors.

However, when air-dried soil, which has been sterilized and then soaked in a suspension of spores of *Mucor hiemalis*, is subjected to gentle blowing, many colonies are obtained, some of which arise from visibly single spores, but the majority from soil particles, and natural soils also yield scattered colonies of diffluent-walled Mucors. The occurrence of air-borne infections by Mucors of this type is thus explained, and also their frequent presence in dust.

Some species, however, notably *M. rouxianus*, exhibit a subsidiary form of air dispersal, in that the old dry mycelial network is extremely brittle, and when blown upon fragments, yielding scattered colonies each of which is found to arise from a spore mass, or even a single spore, attached to a hyphal fragment. This is referred to as *hyphal spore dispersal*.

*Mucor racemosus* also fragments in this way, dispersing chlamydospores as well as attached sporangiospores. A few spore masses, without hyphæ, are also dispersed, and the stalks do not collapse so completely, so that some direct air dispersal is possible. Here the wall survives longer in contact with water, and the spores have been seen to pass out into a superficial drop. The sporangial drops are less conspicuous, as surviving pieces of wall may render them relatively opaque. Probably most of the Mucors with thick ('fragmenting') walls are of this type, which remains chiefly water-dispersed.

*Absidia glauca* is a *spore-mass-shedder* with relatively little hyphal dispersal. Despite such adaptations to air dispersal as stoloniferous growth, rigid sporangiophores, and a columella which collapses into a cup, liberating the whole spore mass, it forms sporangial drops, scatters no single spores in air, and seems to be readily water-dispersed. It is thus intermediate in type.

*Rhizopus nigricans*, however, exhibits advanced adaptation to air dispersal. The sporangiophores, borne aloft on 'stolons' and firmly 'rooted' on their substrate, remain rigid when dry, and the collapse of the columella into a bell-shaped cap exposes the spores to air movements for long periods. Such sporangiophores, grown in a glass cell in 1939, are still erect and capable of shedding viable spores after three years. The rough angular spores are not easily wetted, and do not 'clump' closely in water, so that they dry out rapidly, and are not stuck together in mucilage. This type, however, sheds no spores when moist, but broadcasts spore masses of varying size, and single spores, when quite dry. It is therefore called a *dried-spore-shedder*.

Finally, in conidial types such as Cunninghamella we have *spore shedders* which scatter spore masses from the fresh, moist colony, and vast numbers of single spores from the dry, and are comparable in efficiency of air dispersal with many Hyphomycetes.

The Mucorales are thus brought into closer relationship with the other groups from which they have been somewhat isolated. Once it is realized that the Mucor sporangium is a water-dispersal mechanism, it can bear some comparison with that of the soil Oomycetes, especially in those species which liberate akinetes, and at the same time the various conidial forms, some of them approaching those of the Plectomycetes, become intelligible as adaptations to air dispersal.

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[1] NATURE, 143, 286 (1939).

# SEA WAVES: THEIR GROWTH AND SUBSIDENCE

By P. J. H. UNNA

THE effect of tidal streams on swell was discussed in NATURE of February 21, p. 219, the standard for comparison being the slack-water state. Estimates depended on the fact that, in swell, waves retain their identities as individuals. But wind-forced waves cannot do so, even when there is no change in stream, because, with speed proportional to root of length, their periods lengthen as they grow—see below—and in so far as this affects the issue the arguments for swell may not hold good. But stream, tidal or otherwise, can also influence rate of growth, for which no slack-water standard is available, the laws of growth being still unknown. It is therefore necessary to improvise.

It seems that any system of wind-forced waves consists of at least three series: the primary and most conspicuous waves, the smaller secondary ones which ride upon them, and the long low waves[1,2], more akin to swell, that are masked by the primaries. It may be assumed that, under stabilized conditions, steady wind of given strength gives rise to primary waves of given average length and height, according to fetch and to strength and direction, or absence, of stream. But the only data available are two independent sets of observations, which, when combined, give figures for primary waves due to the strongest winds ordinarily met with. These figures are merely tentative, but suffice to show the general trend, and to illustrate the principles involved.

First, there are Stevenson's equations[3], intended to give greatest wave height according to fetch:

$H_{max.} = 1 \cdot 5F^{\frac{1}{2}} + 2 \cdot 5 - F^{\frac{1}{4}}$ for fetches from 1 to 39 miles,

$H_{max.} = 1 \cdot 5F^{\frac{1}{2}}$ for fetches from 40 to 120 miles,

$F$ being fetch in miles. Secondly, taking Wilton's[4] theoretical limit of 0·13 for steepness as applicable to new-born waves, the more reliable recorded measurements, including many of those summarized by Gaillard[5], indicate that for wave-lengths up to 400 ft. the greatest steepness ordinarily met with in the open may be expressed as:

$S_{max.} = (7 \cdot 5 + 0 \cdot 3H)^{-1}$,

that is, $L = 7 \cdot 5H_{max.} + 0 \cdot 3(H_{max.})^2$.

There is no guarantee that all the measurements on which these equations are based relate to slack water, as will be assumed here. And the equations will be taken as the history of growth of a definite wave system under steady wind, although the strongest winds over short fetches are unlikely to persist over long ones. The following equations result:

$$\frac{dP}{dF} = 9H(2 \cdot 5L^{\frac{1}{2}} + 0 \cdot 15L^{-\frac{1}{2}}H^2)\,(0 \cdot 75F^{-\frac{1}{2}} - 0 \cdot 25F^{-\frac{3}{4}})$$ up to 39 miles,

$$\frac{dP}{dF} = 25L^{\frac{1}{2}} + 1 \cdot 5L^{-\frac{1}{2}}H^2$$ from 40 to 120 miles,

$P$ ($=EG$) being power, that is, energy per sq. ft. of water surface multiplied by its speed to leeward. The resulting values are as in the table below.

| $F$ | $H$ | $L$ | $S = \frac{H}{L}$ | $\frac{dP}{dF}$ | Hours before stability of regime | $\frac{d(EL)}{dF}$ | Waves lost, % per mile of fetch |
|---|---|---|---|---|---|---|---|
| 1 | 3·0 | 25 | 0·120 | 173 | ½ | 935 | 8·7 |
| 4 | 4·1 | 35 | 0·114 | 162 | 1½ | 1,040 | 4·0 |
| 7 | 4·8 | 43 | 0·112 | 165 | 2 | 1,200 | 2·8 |
| 10 | 5·5 | 50 | 0·110 | 175 | 3 | 1,350 | 2·1 |
| 15 | 6·3 | 59 | 0·107 | 183 | 4 | 1,550 | 1·5 |
| 20 | 7·1 | 69 | 0·103 | 197 | 5 | 1,800 | 1·2 |
| 30 | 8·4 | 83 | 0·101 | 211 | 6½ | 2,150 | 0·9 |
| 40 | 9·5 | 98 | 0·097 | (244) | 8 | (2,700) | 0·8 |
| 50 | 10·6 | 113 | 0·094 | 282 | 9½ | 3,350 | 0·65 |
| 100 | 15·0 | 180 | 0·083 | 360 | 16 | 5,450 | 0·35 |
| (200) | 21 | 290 | 0·07 | | (25½) | | (0·2) |
| (300) | 26 | 400 | 0·065 | | (34) | | (0·13) |
| (400) | 30 | 500 | 0·06 | | (41) | | (0·1) |

The figures for $dP/dF$ only relate to power effectively used in raising the primary waves. More is required for the other series, viscosity, turbulence, breaking, and to give leeway to the water near the surface. Nevertheless, they indicate that the wind acts more efficiently as the waves lengthen, and so, except at quite an early stage, by pressure rather than by friction, the area of application of the pressure speeding up as the waves move faster. Later, well beyond the limit of Stevenson's equations, diminution in steepness and in relative speed of wind and waves will cause the pressure to get less, and $dP/dF$ to do the same.

The figures for time show how long the wind has to blow before the sea attains a steady maximum for a given fetch. They are based on the speed of the energy of the primaries.

*EL*, the total energy of a wave, per foot of crest, tends to concentrate near the crest just before breaking, and should therefore be an index of striking force against a breakwater. Values are given for $d(EL)/dF$ to show how, with steady wind, an extra mile added to a long fetch increases maximum striking force much more than when added to a short one. They are based on the derived expressions,

$(60H^2 + 4 \cdot 8H^3 + 16LH)\,(0 \cdot 75F^{-\frac{1}{2}} - 0 \cdot 25F^{-\frac{3}{4}})$ for short fetches,

$(60H^2 + 4 \cdot 8H^3 + 16LH) \times 1 \cdot 125H^{-1}$ for longer fetches.

Owing to lengthening of period, when waves increase in length from $L_1$ to $L_2$, one wave in every $\sqrt{L_2} \div (\sqrt{L_2} - \sqrt{L_1})$ has to disappear. In other words, one wave in every $2L.\frac{dF}{dL} + 1$ vanishes in each mile of fetch. The figures given for percentage loss show that the waves become more permanent with greater fetch, and presumably they then behave more like swell with change of stream. However, it is with these longer waves that stream has least effect on swell.

But stream may have consequences which do not arise with swell, for not only may it affect rate of growth, but also it may alter effective fetch. For example, when there is a local wind in the open, and so not blowing off a weather shore, speed over the ground becomes irrelevant, and yet stream may modify the fetch. Travel of the centre of a cyclonic disturbance is an analogous case. It increases fetch in one semicircle, by mere coincidence the one called 'dangerous' for quite a different reason, and reduces it in the other[6]. The speed of the centre in relation to that of the wave energy is the relevant factor.

Consideration of the effect of stream on rate of growth raises the rather puzzling problem of the hairpin bend. A river takes a hairpin bend, and the wind blows up one reach. Alternatively, it may be blowing up an arm of the sea with a narrow entrance, up the Solent from Hurst would be a case in point. Given, say, a 3-knot weather-going stream, the waves will fail to stem the tide until they attain a 5-ft. length, and their energy will fail to do so until they become 20 ft. long. Presumably waves do form near the weather shore when it starts to blow, but it looks as if they must be soon dispersed by breaking. The solution may be that those which form to leeward, and so in their early life are independent of speed over the ground, attain the lengths necessary for survival before they are swept back to the weather shore. In that case there should be waves of appreciable length where there is almost inappreciable fetch, after the wind has been blowing long enough for the conditions to be stabilized. It would be interesting to know what actually does happen.

But assuming that a stable regime has been attained, it is possible to roughly estimate the change in the demand for wind energy, and so the surplus available for increasing the sea, that a weather-going stream entails. Wave power over the ground is $E(G + C)$, $C$ being stream in ft./sec., and negative for weather going. Let $E_1G_1$ apply to a given position, and $E_2G_2$ to another position a mile to leeward. The increase in wave power within that distance, that is, over a strip of water a mile long and a foot wide, will be $E_2G_2 - E_1G_1$ in slack water, and $E_2G_2 - E_1G_1 + C(E_2 - E_1)$ with stream $C$. That would only be strictly correct if the wind could be so adjusted that $E_2$ and $G_2$ might retain their slack-water values. Then $C(E_2 - E_1)$ would be the change which $C$ entails in the demand for power, but it may also be taken as a rough estimate of the surplus available for increasing the sea when the stream has made to the weather without change in wind, a surplus additional to any due to greater speed of wind over water. To put this in another way, $\frac{d(EC)}{dF} = 16HC.\frac{dH}{dF} = 18C$ ft.-lb. per sq. ft. per sec. per mile, for Stevenson's highest waves with long fetches. The corresponding expression for short fetches would be $(18 + 34F^{-\frac{1}{2}} - 18F^{-\frac{1}{4}} - 10F^{-\frac{3}{4}})C$, giving $24C$, $19C$, $17{\cdot}5C$, $16{\cdot}8C$, $16{\cdot}3C$, $16C$, $15{\cdot}8C$ for the fetches in the table.

To take an example, the value of $18C$ for a 3-knot stream is 90, a constant surplus for weather going, and deficiency for lee going, stream comprised in the increasing slack water figures for $dP/dF$ for the longer fetches in the table. Thus, as with swell, stream seems to have less proportionate effect on longer waves; and from the way in which the sea steepens when the stream turns to the weather, it looks as if the surplus is used in heightening rather than in lengthening them, and so in intensifying rather than in speeding up their energy. But this line of reasoning for the case in which a weather shore has to be taken into account has obvious defects, and it is only put forward in the hope that someone may be able to explain what really happens.

Now as to subsidence. The rate at which swell subsides owing to the viscosity of the water can scarcely be measured, but it has been calculated by Stokes[7], who, taking as examples waves 2 in. and 240 ft. long, has shown that it is much slower for the longer waves. Unfortunately, his formula gives the daily loss in height for the 240-ft. waves as only $\frac{1}{4}$ per cent, and this seems much too small. In fact, a steepish swell only 100 ft. long should subside into an almost flat calm after a couple of days, whereas by formula it would only lose a tenth of its height a week. The probability is that other effects preponderate. Thus a short sea is generally local, and always so in narrow waters. Therefore the tail of its energy will soon pass on, and leave a calm behind. Again, waves lose height because their energy only travels half as fast as they do, and the shorter waves will lose height more quickly on this account. Compare two hypothetical swells, with 6,000 waves, 400 ft. and 100 ft. in length. They will cover 400 and 100 miles to start with. The middle waves of each will start to lose height, for the reason mentioned, when they have travelled through these distances, that is, after 15 and $7\frac{1}{2}$ hours, as the case may be.

The modulus of viscosity is based on frictional action between layers shearing one over the other. On the other hand, water movement under wave action involves two-dimensional distortion, what were elementary cubes in calm being distorted, approximately, from upright to horizontal oblongs, cubical form being passed midway. If the work done against viscosity were proportional to the linear distortion of the cubes, the loss in wave height per second would be absolute, and not a percentage one, and proportional to $L^{-\frac{1}{2}}$, while the distance through which the waves would have to travel before they completely flatten down would be proportional to $LH$. That seems more in accordance with ordinary experience.

Perhaps an actual example will be more convincing. The period of the surf on the Guinea Coast, 10–12 sec., is about double the ordinary period of waves in the Straits of Dover. The surf probably originates in energy that has travelled from the Southern Ocean, a journey of at least a week, while hours rather than days would be the measure of the time of calming in the Straits.

But to return to the original question of length and height in relation to wind and fetch. Determination of the empirical laws should go some way towards solving the problems that surround wind-forced waves. Measurements taken at lee shores with various but preferably small exposures, during steady winds of various strengths, might give some clues, and they could easily be made where the conditions are suitable. In any event, waves affect so many things, ships—breakwaters, erosion and littoral drift—that further knowledge as to their behaviour can scarcely fail to pay.

[1] Vaughan Cornish, "Waves of the Sea", 87.
[2] Jeffreys, H., *Proc. Roy. Soc.*, A, 107, 189.
[3] Stevenson, Thomas, "The Design and Construction of Harbours" 2nd ed., 22–26.
[4] *Phil. Mag.*, (6) 23, 1055 (1913).
[5] Gaillard, "Wave Action", 32, 76.
[6] See also Vaughan Cornish, "Waves of the Sea", 121.
[7] Stokes, "Math. and Phys. Papers", 3, 42 (with footnote).

## NEW SEISMOGRAPH STATION AT LOGAN, UTAH

THE installation of a seismograph at Utah State Agricultural College was made possible through a gift from the estate of the late Thomas E. Oldham, an English-born resident of Logan, who died in 1938 ("The Oldham Seismograph station at Utah State Agricultural College, Logan, Utah", by J. Stewart Williams, *Bull. Seis. Soc. Amer.*, 32, No. 1, January, 1942). The name is a very happy one in view of the history of seismology, though the late Thomas E. Oldham of Utah bears no known relationship to Thomas Oldham of the Geological Survey of India (1816–1878), and R. D. Oldham his son (1858–1936), who first recognized the longitudinal and transverse character of the two types of preliminary waves of an earthquake.

The seismograph room is in the basement of the main college building, the approximate geographical location being latitude 41° 45′ N., longitude 111° 47′ W. The station is equipped with two horizontal component Wood-Anderson seismographs bought with the gift, and an accelerograph installed by the United States Coast and Geodetic Survey. The seismometers are mounted on concrete blocks 28 cm. high, keyed into the surface of a pier, resting in well-cemented gravels of the Bonneville delta, and standing at a height of 4,772·3 ft. above sea-level. Installation of the accelerograph was completed in July 1939 and the Wood-Anderson instruments began operation on January 26, 1940. Since October 1, 1940, all seismograms have been checked by the United States Coast and Geodetic Survey, and the results will be published in the monthly seismographic report.

The north-south seismometer has a static magnification of 716·8, the east-west seismometer one of 697·9. The period for both is maintained at 6·0 seconds, the damping ratio at 20 : 1. The recording drum has so far been run at 15 mm./m. The seismometers are in protective coverings which are also provided with water cans in an attempt to eliminate temperature variations near the instruments. The accelerograph, lent and maintained by the United States Coast and Geodetic Survey, is a No. *M*-45, with a 12-in. recorder, and has three components.

The site appears to be a particularly happy one, especially for the accelerographs, since Utah appears to be the most seismically active of the Rocky Mountain States. This latter may be related to the zone of faulting that marks the eastern margin of the Great Basin. One centre of activity has been in Washington and Iron Counties, adjacent to the Hurricane fault, and another centre lies in Sevier County between the Tushar and Sevier faults. Nearly one third of the earthquakes of Utah have occurred close to the Wasatch fault.

## FORTHCOMING EVENTS

*(Meetings marked with an asterisk are open to the public)*

### Wednesday, May 27

PHYSICAL SOCIETY (COLOUR GROUP) (at the Royal Photographic Society, 16 Princes Gate, London, S.W.7), at 2.30 p.m.—Discussion on "The Relative Merits of Spectrophotometry and Colorimetry" (to be opened by Miss Dorothy L. Tilleard).

INSTITUTE OF CHEMISTRY (LONDON AND SOUTH EASTERN COUNTIES SECTION) (at 30 Russell Square, London, W.C.1), at 6 p.m.—Dr. H. Baines: "Recent Advances in Photographic Theory".

### Thursday, May 28

BRITISH PSYCHOLOGICAL SOCIETY (INDUSTRIAL SECTION) (at the National Institute of Industrial Psychology, Aldwych House, Aldwych, London, W.C.2), at 1.20 p.m.—Miss May Smith: "Fatigue, a Revision of Past and Survey of Present Problems".

### Friday, May 29

ROYAL INSTITUTION OF GREAT BRITAIN (at 21 Albemarle Street, London, W.1), at 5.15 p.m.—Mr. Seton Gordon: "Wild Life in the Western Highlands".*

BRITISH INSTITUTION OF RADIO ENGINEERS (LONDON SECTION) (at the Federation of British Industries, 21 Tothill Street, London, S.W.1), at 7 p.m.—Mr. O. S. Puckle: "Time Bases".*

INSTITUTE OF PHYSICS (MANCHESTER AND DISTRICT BRANCH) (in the Physics Department, University of Manchester), at 7 p.m.—Dr. J. McG. Bruckshaw: "Physics in the Search for Oil".*

### Saturday, May 30

NUTRITION SOCIETY (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 11 a.m.—Conference on "Problems of Collective Feeding in War-time".

## APPOINTMENTS VACANT

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

METALLURGIST for research work in connexion with problems attending colliery wire ropes and other coal-mining appliances—The Chief Executive Officer, Safety in Mines Research Laboratories, Portobello Street, Sheffield 1 (May 29).

SPEECH THERAPIST (WOMAN)—The Director of Education, Education Offices, Woodlands Road, Middlesbrough (May 30).

TEACHER OF MATHEMATICS and a TEACHER OF PHYSICS—The Principal, South-West Essex Technical College and School of Art, Forest Road, Walthamstow, London, E.17 (May 30).

LECTURER IN SCIENCE—The Principal, Domestic Science Training College, Knighton Fields, Leicester (May 31).

ELECTRICAL ENGINEER AND MANAGER—The Town Clerk, Town Hall, St. Helens, Lancs. (endorsed 'Appointment of Electrical Engineer and Manager') (June 1.)

SENIOR LECTURER (MAN OR WOMAN) in the SCIENCE DEPARTMENT of the City of Leeds Training College, with special qualifications in BIOLOGY—The Director of Education, Education Offices, Leeds 1 (June 1).

CHIEF LABORATORY STEWARD—The Principal, Technical College, Doncaster.

SENIOR WOMAN LECTURER IN BIOLOGY—The Principal, Southlands Training College (London), at Highbury, Atlantic Road, Weston-super-Mare.

MISTRESS TO TAKE CHARGE OF THE GENERAL SCIENCE WORK—The Headmistress, Day Technical School for Girls, Fort Pitt, Chatham.

## REPORTS and other PUBLICATIONS

*(not included in the monthly Books Supplement)*

### Great Britain and Ireland

South-West Essex Technical College and School of Art. Annual Report, Session 1940–41. Pp. 23+4 plates. (London: South-West Essex Technical College and School of Art, Walthamstow.) [45

Proceedings of the Royal Irish Academy. Vol. 47, Section A, No. 6: Inherent Relations between Random Variables. By Dr. R. C. Geary. Pp. 63–76. 1*s*. Vol. 47, Section B, No. 13: R. Liffey Survey, 4: The Fauna of the Submerged "Mosses" in an Acid and an Alkaline Water. By Winifred E. Frost. Pp. 293–369+plate 6. 5*s*. Vol. 47, Section B, No. 14: The Exploration of some Caves near Castletownroche, Co. Cork (Studies in Irish Quaternary Deposits, No. 4). By A. M. Gwynn, G. F. Mitchell and A. W. Stelfox. Pp. 371–390+plate 7. 1*s*. 6*d*. Vol. 48, Section B, No. 1: The Distribution of Phytoplankton in some North-West Irish Loughs. By W. H. Pearsall and Edna M. Lind. Pp. 24. 1*s*. (Dublin: Hodges, Figgis and Co., Ltd.; London: Williams and Norgate, Ltd.) [45

### Other Countries

Smithsonian Institution: United States National Museum. Bulletin 161: The Foraminifera of the Tropical Pacific Collections of the *Albatross*, 1899–1900. Part 3: Heterohelicidae and Buliminidae. By Joseph Augustine Cushman. Pp. v+67+15 plates. (Washington, D.C.: Government Printing Office.) 20 cents. [274

Ministry of Finance: Survey of Egypt. Survey Paper No. 45: Map Projections in Practice. By J. H. Cole. Pp. iii+43. (Giza: Survey of Egypt.) [304

U.S. Department of Agriculture. Circular No. 632: Rice-Field Insects. By W. A. Douglas and J. W. Ingram. Pp. 32. (Washington, D.C.: Government Printing Office.) 10 cents. [15

Indian Central Cotton Committee: Technological Laboratory. Technological Bulletin, Series A, No. 53: Technological Reports on Trade Varieties of Indian Cottons, 1941. By Dr. Nazir Ahmad. Pp. vii+103. 1.8 rupees. Technological Bulletin, Series A, No. 54: Technological Reports on Standard Indian Cottons, 1941. By Dr. Nazir Ahmad. Pp. ii+115. 1.8 rupees. (Bombay: Indian Central Cotton Committee.) [115

Bureau of Education, India. Education in India in 1937–38. Pp. vii+109. 2.8 rupees; 4*s*. Education in India in 1938–39. Pp. vii+138. 3 rupees; 5*s*. (Delhi: Manager of Publications.) [115

Indian Forest Records (New Series). Botany, Vol. 3, No. 2: Some Additions to the "Botany of Bihar and Orissa". By H. F. Mooney. Pp. v+63–120. 2 rupees; 3*s*. Botany, Vol. 3, No. 3: Five New Indian and Burmese Flowering Plants—*Ceropegia borii* M. B. Raizada, *Bauhinia lyrata* M. B. Raizada, *Neptunia robertsonii* M. B. Raizada, *Capparis chamoensis* M. B. Raizada, and *Phlogacanthus pochinii* M. B. Raizada. By M. B. Raizada. Pp. iii+121–128+5 plates. 14 annas; 1*s*. 3*d*. Botany, Vol. 3, No. 6: The Relict Vegetation of the Shillong Plateau, Assam. By Dr. N. L. Bor. Pp. v+152–195. 2.2 rupees; 3*s*. 6*d*. Silviculture, Vol. 4, No. 4: Investigations into Artificial Regeneration Details of the Dry Fuel Forests of Madras. By A. L. Griffith. Pp. iv+161–224+4 plates. 2.8 rupees; 4*s*. (Delhi: Manager of Publications.) [115

Forest Research Institute, Dehra Dun. Leaflet No. 6: Note on some Experiments on Cork Substitutes. By Dr. D. Narayanamurti. Pp. ii+4+1 plate. Leaflet No. 7: A New Type of Veneer Drying Rack. By M. A. Rehman and Sultan Mohammad. Pp. ii+4+2 plates. (Dehra Dun: Forest Research Institute.) [115

Herbertia. Vol. 8: Daylily edition. Edited by Hamilton P. Traub. Pp. 186 (27 plates). (Orlando, Fla.: American Amaryllis Society.) 3.25 dollars. [115