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FUNCTIONAL APPROACH TO INTERNATIONAL CO-OPERATION

NO feature of the recent conference of Prime Ministers of the British Empire has been of wider interest than the attention given to the shaping of international organization after the War. No less impressive than the evidence of the unity of the British Commonwealth on essentials, and of its identity of spirit and resolve, is that of willingness to assume the responsibilities of world leadership and to make constructive proposals for the organization and maintenance of world peace. The emphasis placed on the inclusive rather than exclusive character of the British Empire is welcome and wise, all the more when, as in the recent speeches of Mr. Cordell Hull, of Mr. Dewey and others, there is evidence that the United States is giving equal attention to such questions. Furthermore, it is clear that some American periodicals such as *Foreign Affairs* are making a bold attempt to prepare American opinion to face whatever changes may be involved and, to clear away misunderstandings that might hinder Anglo-American co-operation.

An interesting feature of recent discussions has been the tendency to reconsider the verdict on the League of Nations and to look to the reconstitution of such an organization after the War. In December 1942, Mr. Eden, for example, declared that the old League of Nations did not fail because its machinery was faulty but because there was "insufficiently representative force or drive behind it". "There are", said Mr. Eden, "three indispensable attributes for an international organization if it is to have a chance to achieve its purpose. First, it must be fully representative of the Powers that mean to keep the peace ; secondly, it is for those Powers themselves to have the unity and the determination to arrive at great positive decisions ; thirdly, they should have force behind them to give effect to their decisions".

Much the same line of thought has been discernible in Mr. Churchill's recent references to this question, and especially in his statement in the House of Commons on May 24 ; while the picture of international organization which General Smuts gave in his broadcast to the United States last December closely resembles in structure the old League of Nations. Within the wider democratic organization of the United Nations, he said, there would be not only a council and an assembly on the existing League of Nations model, but also a definite responsibility on the four great Powers, the United States, the British Commonwealth of Nations, the U.S.S.R. and China, for maintaining peace, at least while the new world organization is being built up. National sovereignty would be respected, but there would be an international regime of law and order which would guarantee to each State a peaceful life of its own, and the aggressor would be dealt with by international authority as an outlaw.

General Smuts firmly rejected the current criticism in the United States that the League of Nations asked too much of its members. He argued that the contrary is true. The obligations imposed by the League were too indefinite and too slight, and insufficient provision was made for the supply of force. In effect, he accepts the argument consistently used by the French, and supported by several minor European Powers, although opposed by Britain, the United States and Germany.

General Smuts hopes that under the leadership of the four great Powers there will be built a habit of co-operation in the critical years immediately following the War, and that it will become a real basis of future security. In this way would be remedied the lack of will which first wrought the mischief twenty years ago. The question of sovereignty is handled very delicately, but nowhere does he quite put the real issue so plainly as Mr. Wilkie in a recent article, when he urged that the best way for the United States—and other nations—to preserve its sovereignty is to exercise it as part of a world pattern of co-operation.

It will be noted again that General Smuts passed lightly over the common tasks of the new organization outside its police work. Of its economic functions he said little, nor did he touch on the functional proposals which represent one important approach to the organization of peace and are not necessarily inconsistent with his own outline. Nothing in his defence of the old League or in his proposals for a new one holds any support for indulgence by the British Commonwealth once more in an easy peace, which far more than any shortcomings, political or economic, in the Versailles Treaty was our undoing.

That is indeed one of the surest grounds for hope. While the structure of a new world organization is only tentatively outlined, in Great Britain as in America, public opinion is being educated to what is involved and prepared for the demands that must be made upon it for the support of any system of security. Recognition that any structure of peace must be backed by adequate force must be followed by the recognition that adequate force involves the support of adequate measures and policies to provide that force, and a break once for all with the fatal habit of the inter-war years of enunciating principles and proclaiming policies without assuring the means of making them effective.

That means in practice that some joint strategy common to the members of the British Commonwealth and to the other great countries of the United Nations must be planned, and preparations made to put it into execution. Such joint effect should develop the essential habits of co-operation and in time create appropriate organs adapted to the functions they are called upon to perform. Defence, in fact, is only another functional approach to the main problem, and as in so many other fields the most promising line of advance lies in the adaptation to our post-war purposes of the machinery called into being by the dire exigencies of war.

The functional approach to the problem of the organization of world order is not the only way in which new arrangements for international co-operation are already being evolved. The regional approach as exemplified in the Anglo-American Caribbean Commission or the Middle East Supply Centre is fully as important as the functional developments represented by the interim Commission on Food and Agriculture resulting from the Hot Springs Conference, and also the United Nations Relief and Rehabilitation Administration. Both types are consistent with the structure outlined by General Smuts and with the seventeen points of the basic statement on foreign policy of the United States issued by Mr. Cordell Hull in April.

That such proposals do not as yet include any definite provision for either the continuation of the League of Nations as a body 'entrusted with the co-ordination of the activities of a number of international bodies, or the creation of a new international organization entrusted with such functions, does not warrant any hasty conclusions that what is useful in existing organizations will not be incorporated in a new or remodelled organization. Always provided that the fundamental thought and examination are proceeding, it is no bad thing that time should be taken to formulate proposals for the ultimate structure of the permanent world organization to give effect to the points of the Atlantic Charter and subsequent declarations of the United Nations. The growth of a sense of common purposes and ideals and fellowship which is one of the invaluable results of functional organizations is always a slow process, and on this point Mr. M. R. C. Greaves' "The League Committees and World Order" will richly repay further study.

Uneasiness on this point, which is betrayed both in the long report on "The Future Policy, Programme and Status of the International Labour Organization" prepared for discussion at the Philadelphia Conference, and in the Report on the Work of the League of Nations, 1942–43, submitted by the Acting Secretary-General, may well be unfounded. The International Labour Organisation, it is true, has had to establish with each of the new bodies as it is created working relationships equivalent to those which, during the inter-war period, covered a wide field of international co-operation. Nevertheless, it may well be that the distinctive contribution of the International Labour Organisation to the strengthening of the whole structure of international organization now being evolved may best be made, in virture of its tripartite composition, if the Organisation conceives of its activities on functional lines, and pursues policies and activities in accordance therewith. The Organisation possesses no executive authority, and to press too far into such fields as that of full employment, which call for the exercise of authority in economic matters, may well be detrimental rather than beneficial to its influence.

The creation of functional international bodies for handling world social and economic problems is not an end in itself, but a step towards the achievement of a larger objective. Whatever functional bodies may be established will require effective co-ordination in a general pattern of international economic organization, and this general pattern in turn must be integrated appropriately with the general international organization for the maintenance of international peace and security contemplated by the Moscow declaration. This point is rightly emphasized in the report on the future of the International Labour Organisation already mentioned, which points out that it may be reasonably assumed that the constitutions of the functional bodies which are being created will be framed so as to allow effective co-ordination of their activities and the ultimate attainment of the larger objective of a comprehensive and integrated structure of world institutions. Failure to achieve such co-ordination must in fact seriously prejudice the efficient operation of such bodies.

While agreeing that a close and organic relationship must be established between the International Labour Organisation and the general international organization of the future, it is urged that the contribution of the International Labour Organisation to the co-ordination of international public policy must be made in a manner which does not involve renunciation of its tradition of constitutional autonomy, which has contributed much to the vitality of the Organisation and is a major safeguard for its capacity to weather future storms. At the same time, the Organisation is itself potentially an instrument through which there can be achieved both a measure of co-ordination of the specialized agencies now being envisaged and an adequate backing from public opinion for their work. Co-ordination of the different fields of international public policy requires, it must be remembered, not only machinery for taking immediate decisions but also machinery for the general exchange of views on objectives and methods of approach. For the fulfilment of this second function the International Labour Conference appears to be a uniquely appropriate instrument, and its experience may well prove invaluable in working out the precise machinery by which expression is to be given to the democratic ideal in the progress towards world government.

The introduction to the Acting Secretary-General's report on the work of the League makes another useful contribution to this discussion of the future world organization. Its summary of representative views shows that the fundamental ideas on which public pronouncements concur are not greatly dissimilar from the principles of the Covenant of the League of Nations, and that certain general principles are gaining recognition. These principles must be accepted as the bases of continuous co-operation and embodied in definite undertakings. There must clearly be a permanent organization, and whether or not the League of Nations itself is reconstituted we should avail ourselves of its experience over a period of twenty years.

The Acting Secretary-General in his report suggests as a useful method of approach the careful examination of the provisions of the Covenant, to determine for each the principle its authors wished to establish and to ascertain whether that principle can now be considered as generally acceptable. If so, past experience should show whether the means provided in the Covenant to give practical application of the principle are sufficient, and whether it was the power and the will to apply them that were lacking.

One of the problems that will require reconsideration in the light of the lessons of the War is that of joint military action. This is one of the novel features of the plan for world settlement outlined by Ely Culbertson in his "Summary of the World Federation Plan"* of which a more detailed account has since been published in the United States under the title "Total Peace". Mr. Culbertson dismisses as fallacy the idea of reviving the League of Nations, although he regards its work as laying the foundations for a new attempt at world order. Mr. Street's plan for Federal Union is also dismissed as premature in the face of the dominant force of nationalism. Co-operation of sovereign nations must precede world federation of nations and the world parliament of man. Neither of these plans takes into full account the revolutionary changes in science, military weapons, communications and economics of our generation; but Mr. Culbertson's "Summary" does not specify the fatal defects of structure which in his view crippled the League of Nations from the start, and the real defect in the Federal Union proposals is probably one of rigidity rather than of timing.

Meanwhile, the League report itself recognizes that structural defects may hamper the development of an institution, however much we agree with Mr. C. J. Hambro that organic growth in itself can never be made to conform to blue-prints but creates constantly new problems. Moreover, if the means placed at the disposal of an international institution are not commensurate with the aim in view, the loftier the aim the greater the risk that the institution may sooner or later meet with a serious failure, which will shake confidence and make reconstruction difficult. Nevertheless, there is some substance in the suggestion of this League report that what is sometimes regarded as the liability of the old name might prove an asset for the new attempt to establish world order, because use of the old name should discourage any tendency to think that the machine of itself would produce results, whereas they can only come from consistent effort and loyalty and goodwill.

Mr. Culbertson, however, is at one with the League report in holding that the future world organization cannot be left over entirely until peace and order have been re-established. The details may be left, as Mr. Churchill has suggested, to be settled after the formidable foes we are now facing have been beaten down and reduced to complete submission. This view was afterwards endorsed by Mr. Eden, who suggested, as one of the five principles on which world organization should be based, flexibility and growth by practice without attempting to work to a fixed and rigid code or rule; but a prior and clear-cut decision as to the revival of the collective security system and guaranteed mutual help might well aid in solving many thorny problems, such as those relating to frontiers.

Mr. Churchill, in his review of foreign policy on May 24, contemplated a world order and organization equipped with all the necessary power to prevent

* Summary of the World Federation Plan: an Outline of a Practical and Detailed Plan for World Settlement. By Ely Culbertson. Pp. 78. (London: Faber and Faber, Ltd., 1944.) 5s. net.

future wars or the planning of them in advance by restless and ambitious nations. That organization would include a world council of the greatest States for the purpose of preventing war, as well as a world assembly of all Powers. Mr. Churchill would not commit himself to the precise relations of these bodies or to the question of united forces of nations as against world police for keeping the peace. He considered that we should undoubtedly embody in our world structure a great part of all that had been gained for the world by the structure and formation of the League of Nations, but within the limits assigned to it we must make sure that our world organization has overwhelming military power. These points were later elaborated by Mr. Eden as three of four principles of world organization, the fifth being that the Powers included should strive for economic as well as for political collaboration.

That is the main purpose of Mr. Culbertson's quota force principle. This is a new system of composition and distribution of national armed forces, based on heavy weapons. The world police, under this system, would be the only force in the world which is armed with heavy weapons (that is, armoured aeroplanes, capital ships, tanks and heavy guns), the manufacture, transportation and possession of which would be the monopoly of the world federation. This police force would consist of twelve separate armies: eleven national contingents recruited from the citizens of each initiating State, and an international contingent, called the mobile corps, of units or regiments, or their naval and air equivalents, recruited from all member States other than the initiating States, and distributed, in contrast to the national contingents, which will be stationed only in their country of origin, only in strategically located islands purchased by the Government of the World Federation and in leased bases in States which participate in two regions. Quotas assigned to the national contingent are based on the industrial power of the initiating State, the extent of the regional territory it must defend and on the psycho-political factor. The international mobile corps would be stronger than the strongest contingent of any initiating State.

This scheme for a double military system is ingenious, and while some points of detail such as the relative allotment of power as between the U.S.S.R., the United States and the British Commonwealth may be hard to justify, it at least merits serious consideration, and represents something which might well develop naturally from the position in which the United Nations will find themselves after the War. It is certainly not inconsistent with the picture of world organization sketched by General Smuts or by Mr. Churchill. For the rest, it may be said that Mr. Culbertson's plan is based on a regional system, the arguments for which are given in his book, "Total Peace"; this may explain some of the inconsistencies which appear to characterize the plan as presented here. Its fundamental assumptions no less than its specific objects are common ground in most discussions to-day on the form of world organization. The substance of them is to be found in the speeches of Mr. Churchill and of Mr. Eden on May 24 and 25. Mr. Culbertson himself, for example, in furtherance of the establishment of world-wide educational, scientific and economic institutions, would embody within the world federation the present agencies which the United Nations, the League of Nations and the larger peace foundations have already established for these ends.

If on the evidence so far presented some of the details of Mr. Culbertson's plan are not entirely convincing, in spite of the serious study which his suggestion for a double military system deserves, to that extent they attest the wisdom of Mr. Churchill's warning against putting forward formally and in too great detail our own views or solutions in a manner which might prejudice their consideration. The importance of democratic procedure in establishing a permanent organization is sometimes overlooked, but without it the consent of all participating nations is not likely to be easily won or held. Nevertheless, though methods and details may vary, through all these proposals, from whatever source, runs the common thread that, as Mr. Eden said in concluding his speech, only by translating into the period of peace the confidence we have built up among our allies in war can we hope to save the world from a repetition of these conflicts. Given the will, courage and elasticity of mind, firmness of purpose, the ability to learn from the past, to adapt to our present and future needs whatever of value existing institutions may hold, there is no room for pessimism. The constructive thought and imagination displayed in those and other speeches and papers show that mankind is capable of providing himself with the institutions necessary to ensure that the fruits of victory are enjoyed, and that the great heritage of civilization, the rights and liberties of individuals, are not again endangered by nationalistic ambitions.

A SURVEY OF THE UNIVERSE

The Universe Around Us

By Sir James Jeans. Fourth edition, revised and reset. Pp. x+297+32 plates. (Cambridge: At the Oxford University Press, 1944.) 15*s*. net.

IN the decade that has elapsed since the third edition of this well-known book, many advances in astronomy have been made. Sir James Jeans has incorporated reference to many of these in the new edition, and has taken the opportunity to make a thorough revision and to rewrite a large part of the book. Thus, for example, reference is made to the recent discovery of companions of small mass belonging to the systems of 61 Cygni and 70 Ophiuchi; some account is given of Eddington's investigations on the expansion of the universe and the rate of recession of the extra-galactic nebulæ; and the bearing of the new knowledge of the source of stellar energy, derived from the study of thermo-nuclear reactions, on the evolution of the stars is described.

Thus the book continues to provide one of the best accounts available in simple language of the results of modern astronomical research. A very wide field is covered, for an account of modern atomic and quantum theory is included; this is a great help to the general reader. The book is written with Sir James Jeans's usual clarity of style and facility of expression. It abounds in apt and striking illustrations and analogies; to give but one example, the statement that the temperature at the centre of a star is about twenty million degrees conveys little to the average reader, but the remark that a pin-head of matter at that temperature would emit enough heat to kill anyone who ventured within a hundred miles of it helps the reader to realize something of what such a temperature involves.

There are a few places where a little further explanation would have been helpful to the lay reader. Thus, on p. 33 it is stated that "in 1814 Fraunhofer repeated Newton's analysis of sunlight, and found that the spectrum was crossed by a number of dark lines"; a reader who knows nothing about the spectroscope but is familiar with Newton's experiment, in which a coloured band was formed by overlapping images of the sun, may be left wondering why the spectrum is crossed by *lines*. On p. 65, it is stated that the rotation of the galaxy makes it difficult to believe in a local cluster of stars, because such a cluster could not be a permanent structure; the reader may object that some of the photographs of extra-galactic nebulæ appear to show many such local clusterings.

On pp. 25, 26 it is said that Ptolemy argued that the earth could not be moving through space because this would involve a displacement of the nearer stars relative to the background of more distant stars; this is misleading, because in Ptolemy's time and for many centuries after, it was believed that the stars were all fixed to a sphere. A motion of the earth would have involved changes in the angular separations of the stars, of the nature of proper-motions, and it was through displacements of this type that William Herschel was enabled to detect the motion of the sun relative to the stars.

On p. 204, referring to the hydrogen content of the stars, it should be mentioned whether the stated percentage is by mass or by volume. The statement on p. 253 that the swarm of asteroids can be explained *quite simply* as the broken fragments of a primeval planet is not correct, if it is meant to imply that it has been proved that the asteroids originated in this manner. Nor is the statement that there are several families of comets the members of which follow one another round and round in the same orbit (p. 256) correct. The explanation of comets as part of the *débris* left after the birth of the planets shelves the difficult question of the origin of comets; several comets have been observed to disrupt, and traces of them can be detected by showers of shooting stars when the earth meets the orbit of the disrupted comet; there is no evidence of any comet having entered the solar system from outside; it seems that the formation of comets must be a continuing process in the solar system, though there is no satisfactory theory to account for this.

But these are minor defects in a book of absorbing interest, which carries the reader through space and time, discusses the evolution of the stars and of the universe, and concludes with a fascinating chapter on beginnings and endings. The black-out conditions during the War have enabled many people to see the glory of the heavens for the first time, and this has brought about a greatly increased interest in astronomy. This book should do much to stimulate that interest.

H. SPENCER JONES.

ELEMENTARY WAVE MECHANICS

Elementary Wave Mechanics

Introductory Course of Lectures. By W. Heitler. Notes taken and prepared by W. S. E. Hickson. (Hectographed.) Pp. ii+88. (Dublin: Dublin Institute for Advanced Studies, 1943.) 5*s*.

THERE are many elementary treatises on wave mechanics. Their multiplicity is due perhaps to the fact that there are many ways of approach to this subject or perhaps because, as in the case of a disease for which many cures are advertised, the right treatment still awaits discovery.

The present work is based on notes taken on a course of lectures given by the author. It contains features which distinguish it from similar works and which make its publication well worth while.

It is clearly the work of a lucid teacher, and this is to be expected of the author of "The Quantum Theory of Radiation". It is the work of a writer who can keep the physical principles of the subject in the foreground and at the same time introduce the reader to the essentials of the mathematical technique, with some pardonable short cuts.

The book covers a representative range of problems, including the treatment of the hydrogen-like atoms and the problem of two electrons. Welcome additional subjects of study in a work of this scope are the perturbation theory, exchange degeneracy, the spin wave function and the helium atom.

The uncertainty relations are presented in a way which is likely to lead the student to think of them as peculiar to the quantum theory. The sense in which this is true is stated by the author (p. 13), where he writes: "Classical mechanics holds for heavy bodies, the uncertainties are a peculiarity of quantum mechanics which applies to light particles". It is, however, advisable that the student should be aware of Rayleigh's conditions of optical instruments, some simple examples of which can be explained from his knowledge of physical optics. This line of approach has some justification in the history of the development of the uncertainty relations and avoids the tendency towards over-emphasis upon the application in wave mechanics.

It is to be regretted that the wave-length of the particle waves is described as being inversely proportional to the particle velocity although this is true in the limited application considered. It is preferable to describe the wave-length as inversely proportional to the momentum, since this is a true statement both in the classical and relativistic cases.

The author attempts to lead the reader very gently to the idea that the particle velocity has its counterpart in the group velocity and not in the phase velocity of the waves. The attempt seems rather laboured and leads to the strange result that the phase velocity is half the group velocity. This may lead to confusion later on when the question has to be further considered.

The work is to be recommended to degree students who require a clear statement of the principles of the subject and a knowledge of how they are applied in practice. The syllabus chosen is excellent and affords a useful guide to teachers of the subject.

With the correction of some misprints and amplifications here and there, the work will become a valuable introductory text-book.

H. T. FLINT.

Catalogue of Union Periodicals

Vol. 1: Science and Technology. Edited for the National Research Council and National Research Board by Percy Freer. Pp. xvi+525. (Johannesburg: University of the Witwatersrand, 1943.)

THIS catalogue covers the periodicals of seventy-seven libraries in the Union of South Africa which have agreed to co-operate in inter-library loaning. While it should primarily facilitate such loans and thus materially aid the war effort in South Africa, it is also an important aid to reshuffling so as to place the most complete sets in the larger centres. The catalogue renders comparatively easy the compilation of a national list of desiderata of those important titles of which no sets or only poor sets exist in any South African library, and besides eliminating unnecessary duplication it should encourage co-operative purchase to fill the lacunæ thus revealed.

The catalogue includes many titles which are too recent to be included in the World List of Scientific Periodicals, and for that reason and also on account of the subject classification it may be found a useful guide to the existence of scientific and technical periodicals, although not for loan purposes, by scientific and technical librarians in Great Britain.

The present catalogue contains more than 6,000 entries as against 3,117 in A. C. G. Lloyd's List of 1927. The abbreviations used in the World List of Scientific Periodicals are used wherever possible and emphasized by underlining. The primary arrangement is by subject, and the sub-arrangement first by language, and second by country. Where no subject is expressed the main entry goes to the name, for example, Carnegie, Faraday, Franklin, Smithsonian, with cross-references from institutes and societies. Other main entries are under distinctive titles, while where no subject is expressed with academies, institutes or societies the main entry goes elsewhere, for example, to institutes, laboratories, museums, etc. Entries closed on September 30, 1941, and participating libraries are now invited to maintain a list of additions and corrections for quarterly publication in South African libraries.

The Weather

By George Kimble and Raymond Bush. (Pelican Books, A.124.) Pp. 188+24 plates. (Harmondsworth and New York: Penguin Books, Ltd., 1943.) *9d.* net.

Meteorology

By Lieut.-Colonel R. M. Lester. (Complete Air Training Course, No. 4.) Pp. 64+4 plates. (London, New York and Melbourne: Hutchinson's Scientific and Technical Publications, n.d.) *2s. 6d.* net.

"THE WEATHER", by Kimble and Bush, must be one of the best nine-pennyworths extant. It is chatty and inconsequential, but when one comes to the end and looks through the index, one realizes that it holds a great deal of solid information; even the reader who follows the suggestion to skip the introduction will miss a great deal. The structure of the atmosphere, instruments, clouds as weather forecasters, depressions and anticyclones are all dealt with simply but effectively; then follow the seasons, 'one-man' forecasting and some oddments like weather cycles, all illustrated by many rhymes from folk-lore, some clear diagrams, and twenty-four excellent cloud photographs. There are one or two errors, such as that a halo grows in diameter as the cloud lowers, but on the whole, readers—and we hope they will be many—will obtain a very good grounding in meteorology.

Lieut.-Colonel Lester's little book is chiefly remarkable for its surprising statements. Its quality may best be shown by a few quotations: "No two snow crystals are ever alike. When one considers the millions of crystals that fall in a snow-storm, this is an amazing feat of nature." "Rainfall is greater at high altitudes. This is caused by the air coming into contact with the cold surfaces of these higher regions." Cyclostrophic effect in wind. "This is the deviation of its flow further from the curved path through the gyroscopic properties of the air mass." "A satisfactory type of rainfall gauge is a brass tube of just over two inches diameter inside an eight-inch cylindrical can." C. E. P. BROOKS.

Quantitative Chemical Analysis

A Student's Handbook. By Prof. Joseph Reilly and Eileen A. Moynihan. Pp. x+116. (Cork: Cork University Press, 1944.) *7s. 6d.*

THIS is a very carefully written and good introduction to quantitative analysis by teachers who have evidently had much experience in presenting the subject to students, and have the ability to write a clear and concise account of all the details of the laboratory work which are essential to success. It includes both volumetric and gravimetric analysis, and the examples chosen are very good. The main emphasis is practical; the chief fault of many books, that of writing an elementary treatise on physical chemistry interspersed with a few practical exercises which are generally quite inadequately described, is avoided. The relevant theory is always given, but in its proper proportion, and the result is a very good practical manual which will make easier the work of teachers who adopt it.

The standard of the book is between the intermediate and final of most university degree courses. An appendix describes a method of phosphate separation in qualitative analysis by means of zirconium oxychloride which is an improvement on the usual procedure by this method.

Martindale's The Extra Pharmacopoeia

Twenty-second edition. In 2 vols. Vol. 2. Pp. xxxiii+1217. *27s. 6d.* Supplement to Vol. 1. Pp. 48. *2s.* (London: The Pharmaceutical Press, 1943.)

THE second volume of Martindale continues to provide modern and reliable information on drugs and their assay, biochemistry, bacteriology, nutrition and therapy in general. The material provided is as up to date as is compatible with the time necessary to compile and produce the book. The sulphonamides and the vitamins, on which much recent work has been done, are well documented, but the short notes on penicillin direct attention to the rapidity of progress in this field.

As a book of reference for the clinician, pharmacologist and research worker "Martindale" is already well established, and the new volume will deserve the reputation of its predecessors.

The supplement to Volume 1 records changes in, and additions to, the British and United States Pharmacopoeias and National Formularies. There are also notes on recent Orders affecting supplies of drugs, new proprietary names and approved names of substances.

JOHN DALTON, 1766–1844

By Dr. J. NEWTON FRIEND
Central Technical College, Birmingham

On July 27, 1844, at the ripe age of seventy-eight, John Dalton passed peacefully away in Manchester. Local feeling was stirred to its depths; it was unanimously agreed that nothing less than a public funeral could express the reverence felt for the memory of so great a man. This was the more remarkable since Dalton was a strict Quaker and as such was opposed to official ceremony. His remains lay in the darkened town hall, where some 40,000 people paid homage before interment took place in Ardwick Cemetery on August 12.

So many biographies and sketches of the life of Dalton have been published from time to time that it would be superfluous to labour the details in these columns. Suffice it to say that Dalton was born, probably on September 5, 1766, of humble parents in a thatched cottage in the secluded Cumberland hamlet of Eaglesfield, some half a dozen miles from Cockermouth. The cottage still stands, the thatch replaced by slate, and a suitably inscribed commemorative tablet has been inserted above the door. Fig. 1 shows the cottage with the tablet. Fig. 2 shows the Friends' Meeting House at Eaglesfield (where Dalton worshipped) as it appeared in 1895. In the foreground stands Mr. Norman, the oldest inhabitant of the village and the only one who then remembered seeing Dalton.

At an early age Dalton showed unusual industry and talent; when a mere lad of twelve, he began to teach the village school in a barn, but discipline was difficult as his pupils included boys and girls several years older than himself and far more interested in each other than in their lessons. Three years later (1781) he joined his brother as usher in the Friends' School, Stramongate, Kendall, run by his cousin George Bewley. The school had been founded in 1698 and continued until some fifteen years ago, when it was closed down. It is still an educational institution, being used in part as an elementary school for senior boys and in part as school and dental clinics*.

Fig. 1.

Dalton appears to have been but an indifferent teacher, too much absorbed in his scientific pursuits to worry unduly about the progress of his pupils. About this time he gave a few lectures also to adult audiences, but Dr. Henry states that he was never an attractive lecturer. He seemed unable to devise really impressive experiments, and failed as often as he succeeded in carrying out even such elementary experiments as he did attempt. Nature seldom puts all her eggs in one basket.

While at Kendall, Dalton discovered that his idea of colour was abnormal—he was colour blind. To him pink appeared as blue, and a waggish friend suggested that this might be the cause of his remaining a bachelor, the pink cheeks of a maiden giving him "the blues".

* I am indebted to the Town Clerk of Kendall for this information.

In 1793 Dalton went to Manchester, where he entered upon a period of great scientific activity. It was here that, in 1803, he announced his Atomic Theory, which revolutionized our outlook on chemistry, rapidly raised him to the pinnacle of fame and earned for him the title of 'father of modern chemistry'. Honours of various kinds were showered upon him, but none affected his rugged character or his genuine native simplicity. In the words of Millington, one of his biographers, "even after all his triumphs and scientific achievements, he was at heart the simple countryman of frugal tastes, speaking the broad dialect of the Cumberland fells".

Dalton's interests, however, were by no means confined to physics and chemistry. In 1844, having been a member of the Manchester Literary and Philosophical Society for fifty years and occupied the presidential chair for the last twenty-seven of these, he presented his fiftieth annual meteorological report, having then made more than 200,000 separate recordings. He once (1801) wrote a book on English grammar. In this he pointed out that, while logically there can only be two tenses, past and future, it is convenient to regard the immediate past and future as the present. In his treatment of gender he was less fortunate; few will accept his dictum that "phenomena" is the feminine version of "phenomenon".

In 1804 Davy invited him to lecture at the Royal Institution. Dalton evidently thought highly of Davy, referring to him as "a very agreeable and intelligent young man", but added that his habits were marred by one serious defect, to wit, he did not smoke! Six years later, Davy suggested that Dalton should offer himself as a candidate for election to the Royal Society, but Dalton did not do so, possibly because the fees were too high for one holding so meagre a purse. He never laid himself out to make money; for him money was merely a means to an end; otherwise he had no use for it. Thus in 1818 he was invited to join the Arctic Expedition of Sir John Ross at a salary of £400–£500 a year. This would have been nothing less than a fortune to Dalton; nevertheless he declined on the ground that he did not wish to interrupt his scientific investigations.

FIG. 2.

In 1822, however, Dalton was elected a fellow of the Royal Society; he also visited Paris, making the acquaintance of Laplace, Thenard, Ampère, Berthollet, Gay Lussac and numerous other savants. Alas, he could not meet Lavoisier. Ten years later he received the degree of D.C.L. at Oxford, along with his famous contemporary Faraday. In 1833 effect was given to a proposal of Charles Babbage, of calculating-machine fame, and he was awarded a Civil List pension of £150 a year, afterwards raised to £300. Meanwhile, Manchester decided to erect a statue in his honour, and that was duly completed. It still stands in the entrance to the Manchester Town Hall. In the same building is a mural painting by Ford Madox Brown depicting Dalton collecting marsh gas for his analyses. In 1834 Edinburgh conferred on him the degree of LL.D. and in the same year he was presented at Court to King William.

In a codicil to his will, Dalton bequeathed all his chemical and philosophical apparatus to his pupil and friend, Dr. William Charles Henry, who in 1854 published his well-known "Memoirs of the Life and Scientific Research of John Dalton".

Turning now to the theory which has made Dalton famous for all time, it may be well to remind ourselves that Dalton was not, and never claimed to be, the first to postulate an atomic theory of matter. Early Hindu literature shows that such a theory existed so long ago as 1200 B.C. Matter was regarded as an aggregation of minute, discrete particles which, though separated from each other by empty space, contrived to attract one another with sufficient force to account for the ordinary phenomena of cohesion. In later years the Greeks held similar views; we do not know if they borrowed them from the Hindus or arrived at them independently; they did, however, go a little further in that they regarded the particles as indestructible and always in motion. Democritus (460–360 B.C.) was an early exponent of these ideas, which at first were by no means popular but later came to be generally accepted.

Although useful to the physicist, the theory did not offer much help to the chemist, largely because it was purely qualitative in character. In the fourteenth century, Al Jildaki (died 1360) observed that "when substances react they do so by definite weights". Had this quantitative conception been suitably linked up with the atomic theory, our Law of Equivalent or Combining Weights would have been anticipated by several centuries. But the time was not ripe and the observation appears to have been forgotten. It required a Dalton to bridge the gulf.

Dalton's Atomic Theory is usually summarized as follows:

(1) The atoms of any one element are all alike and possess a definite and characteristic mass. They differ from the atoms of any other element both in their physical and chemical properties.

(2) Chemical compounds are formed when the atoms of two or more elements unite in simple chemical proportions.

Once these postulates are accepted, the Laws of Definite Proportions, Multiple Proportions and of Equivalent Weights become self-evident.

The exact manner in which Dalton arrived at his theory has been a matter of dispute, into which we need not enter. It may well be that the idea gradually crystallized in his mind as the result of prolonged thought in various directions. Neither need we worry because we now know that the theory, as given above, is not correct in detail. Thus, owing to the existence of isotopy, the atoms of any one element are not of necessity all alike; also, atoms do not always unite in simple numerical proportions when compounds are formed, as witness albumen and gelatin, to which reference is made below. The important point is that the essential feature of the theory is correct. It would not be difficult to re-state it, if such procedure were deemed necessary, to harmonize more closely with modern conceptions of matter. But a theory need not be correct in detail in order to be useful, and it is mainly by its use that we judge it.

One immediate advantage of the theory lay in the possibility it afforded of designing formulæ capable of indicating both qualitatively and quantitatively not merely the composition but also the constitution of chemical entities by means of symbols and formulæ. Hitherto, symbols had been used by the alchemists, more or less as cryptic labels, to denote various substances. Thus a circle denoted gold, a wavy line represented water, and so on. But such symbols gave no indication whatever either of the quantities involved or of the composition of the substances. Dalton denoted atoms by circles, suitably modified to distinguish between different elements. Thus ●○ represented a molecule* of carbon monoxide. As each circle denoted one atom of carbon and oxygen respectively, the formula clearly gave the amount of the gas present and its composition. When several atoms are united in a molecule, it may be possible to arrange them in different ways, yielding compounds with different properties. It is not generally known that Dalton himself realized this. He believed it to be the case with albumen and gelatin, which he formulated as

albumen gelatin

(that is, C_2H_2ON).

* We are ignoring the fact that at first Dalton wrote oxygen as ⊙ and afterwards altered it to ○; also that he used the word atom very frequently where we to-day would write molecule.

The example was unfortunate, for these molecules are far more complex than Dalton realized. This fact, coupled with the introduction of the simpler Berzelian notation, which led to the writing of empirical formulæ, such as CO_2 instead of the structural o●o (or OCO), caused Dalton's observation to be overlooked. Consequently when later chemists discovered true examples of isomerism, Dalton's contribution had been forgotten.

The question now arose as to how the union of atoms could take place. The first suggestion that atoms possess mechanical hooks was very natural at the time but was soon found inadequate. The connexion between chemical combination and electrical forces was then just beginning to be realized, for Nicholson and Carlisle had already in 1800 decomposed water electrically. In his Bakerian Lecture to the Royal Society in 1806, Davy formulated a qualitative electrochemical theory of chemical combination which was improved upon by Berzelius in 1812; but curiously enough, attention was focused more on the combining power of groups of atoms (radicals) than on individual atoms themselves. It was not until eight years after the passing of Dalton that Frankland introduced the conception of an atomic attractive power, that is, valency. The gate was thus opened to an enormous field of research on atomic forces and molecular structure, the confines of which even now appear to recede, like the end of a rainbow, the further we progress.

The foregoing may be regarded as some of the more immediate consequences of Dalton's theory. But the tale is not complete even yet. The atomic theory has migrated into realms undreamed of by its creator, and has opened up avenues of approach to problems the immensities of which appear to grow with each succeeding age.

In his brilliant researches, Faraday showed that electrically charged atoms or groups of atoms can exist in solution; he called them 'ions' and in 1835 enunciated his Laws of Electrolysis. It was inevitable, therefore, that the atomic conception of matter should eventually be extended also to electricity. This was first clearly done by Johnstone Stoney in 1874, who named the 'atom' of electricity an electron, and pointed out that Ne is Faraday's ionic charge for a univalent ion, N being Avogadro's number and e the 'atom of electricity', that is, the electronic charge.

But if electricity is atomic, what about other forms of energy? The breakdown of classical methods of calculating the intensity of radiation of a black body at various temperatures led Planck in 1900 to extend the atomic idea to energy in general. He suggested that bodies can only emit radiation in discrete portions or quanta—atoms of energy. This enabled him to draw up an expression that would account completely for the observed distribution of energy in the temperature-radiation spectrum. He was also able to explain the lack of agreement between the classical formulæ of Wien and Rayleigh, and to show that these formulæ represent extreme cases of his own universal expression.

Five years later, Einstein explained photo-electric effects as due to 'atoms of light' or photons; in other words, light waves possess atomic characteristics.

Limitations of space forbid further discussion of these themes. Sufficient has been said, however, to show that Dalton's Atomic Theory has proved one of the most fertile ever propounded. Just as a crystal dropped into a solution may yield a vast crop of crystals entirely unrelated in quantity to the size of the original crystal, so Dalton's theory has been a nucleus around which have collected, and are still collecting, new laws, hypotheses and theories.

MEN AND SCIENCE IN THE SEA FISHERIES*

By MICHAEL GRAHAM

IN 1863–65, Huxley, as a member of a Royal Commission, on fisheries, made a tour; and I cannot do better than quote a report in his own words on one of the places visited—the Isle of Skye. "He would mention an occurrence which made an indelible impression on his mind—the total earnings of one of those peasants, he might say his whole property and everything belonging to him, would not come to more than £5. Certain interested parties in Glasgow . . . had got a law smuggled through the House of Commons, where nobody cared anything about it, by which it was made penal to catch a herring during the three summer months of the year, a time at which herrings were swarming in innumerable millions . . . that meant that [a man] might be totally ruined or might be put in prison for doing this. . . . Now there was not the smallest imaginable reason why that enactment should have been passed. It was a stupid, mischievous and utterly useless thing. . . . That appeared to be one of the worst forms of modern oppression."

I cannot find words to express my admiration for this passage, of which I have given only excerpts. There the man of science, quite sure of his ground, raised his voice against arrangements that were not based on scientific facts; and in this cause he used all the power of the English language to convey meaning and feeling. Reading it to-day has a reviving effect, by contrast with the polysyllabic jargon of good intention, among which so many of our modern aims meander, and are lost.

This visit of Huxley's led to the Act of 1868, by which most of the restrictions on fishing were abolished; and it set the policy of no restriction without scientific justification which has ruled Britain ever since, and affected millions of lives and fortunes. Such is the power of the scientific attitude, in a bold and able man.

A second notable event in the history of British fisheries was an International Fisheries Exhibition and Congress in 1883, presided over by Huxley, who was now president of the Royal Society. The exhibits included a very good one from the United States Bureau of Fisheries, which was at that time pre-eminent in knowledge of life in the sea, owing to the inspiration of Agassiz. This exhibit aroused great interest among British scientific men, of whom Ray Lankester was one, and it led to their signing a memorial calling for organized marine research in Britain. Their feeling formed one of the major trends at the Exhibition.

A second noticeable trend, closely connected, was the demand from Mr. James Alward, and from other good skippers, for research into the biology of the fish themselves; and a third trend, voiced by the

* Substance of a discourse delivered at the Royal Institution on June 9.

fishermen and by the younger men of science, was one of contention with Huxley and the Civil servants on the fundamental problem. Huxley said: "Nothing that we do seriously affects the number of fish. And any attempt to regulate these fisheries seems consequently, from the nature of the case, to be useless." Finding the truth on this third question called for what we may call 'population studies'.

It seems to me that there were very sensible people gathered at that Exhibition, sixty-one years ago; and that the time is overdue for something of a progress report.

Looking first at the branch that was inspired by the American exhibit, we see in the past sixty years an enormous accumulation of knowledge, and an increase of certainty. Many of the thousands upon thousands of species of animals and plants in the sea have been described, named and classified. The majority of these are in the plankton, that is, the free-floating population, mainly of very small animals and plants, which are found principally in the surface and near-surface layers of the ocean, and in the shallower seas in all layers. Even those animals whose habit is to live near the bottom or near the shore, with few exceptions, begin life as members of the plankton. Besides their structure, something is also known of the behaviour of these animals and plants. Although their active motion in the horizontal is very limited, they appear to have some control over the layer at which they swim, and undertake more or less regular daily migrations—generally to the surface at night and to deeper layers by day. The surface and deeper layers of water often move differentially. Thus by the movements of horizontal layers of water, plankton organisms do effect some horizontal movement, almost like balloonists. It also seems that some of them settle on or very close to the bed of the sea at times, leaving it at others. But there is much about the behaviour of plankton that is still unknown.

It is also now known that the major seasonal changes in numbers of the plankton are closely related to the supply of nutrient salts in the water; shortage of nitrate, phosphate and silicate being liable to limit the growth and multiplication of the plant forms. Here too, though, there is much that is unexplained. Knowing also that the supply of these nutrients varies with the great water movements, which in their turn are correlated with major meteorological changes, investigators are reaching out to a statement of correlated fluctuations—in weather, fisheries, agriculture, water and plankton. This much indeed is known, that particular species are found in different water masses, and their presence denotes an incursion of that kind of water. An example is afforded by three species of the arrow-worm, each of which is confined almost entirely to one kind of water—inshore, Atlantic, or mixed coastal.

Turning to the animals that live on the bed of the sea, it is now known that these tend to occur in more or less well-marked ecological communities, depending on the nature of the ground; but that their numbers are liable to great fluctuations according to the variations in the current that carries the larvæ.

As an example of the application of this general marine natural history to fisheries, we may remember that great concentrations of some of the more spiky diatoms have been found to exclude herring from the normal fishing grounds; and that a start has been made in providing guidance to the fishermen accordingly.

Principally, though, the researches that have provided all this knowledge are to be regarded as research for its own sake, rather than as yielding this or that means for directly making a profit; and I would add that without the confidence that the main lines of nutrition, competition and mortality have been brought within human ken, I for one would not feel bold enough to make the recommendations that come from more closely applied researches. Pure research reveals the background, and the background enables the applied research worker to use judgment, which he would otherwise be quite unable to do for lack of perspective—and judgment is the most valuable weapon that can be provided in practical matters.

For a progress report on the first requirement stated at the Exhibition, general marine biology, there is ample material with which to satisfy our predecessors. This is largely to the credit of the laboratory of the Marine Biological Association and of its many relatives. I think that Huxley, Ray Lankester and Alward would all be reasonably pleased.

The second requirement was knowledge of the life-history of the food fishes—cod, haddock, plaice, hake, herring, and other less quantitatively important kinds, such as sole, turbot, whiting, mackerel and sprats. Here progress was slow at first, mainly for two reasons. First, researches were mostly confined to the waters near the shore. Observations in bays and inlets were valuable, but they could not be generalized to the much greater population of fish which the fishermen were taking in the open sea. It is true that occasional voyages were made in commercial trawlers, and these are, and always will be, necessary as part of the work of a fishery naturalist because that is the only way in which the naturalist can sample the world of fishing methods and people. Without intimate knowledge of fishermen's methods and aims, a naturalist cannot read truly the fishery statistics, which can provide him with a wealth of valuable material; and unless he sees for himself what fishermen are doing, he cannot have a true conception of whether they would in practice carry out his recommendations.

But we are now considering the natural history of the fish, and here the restriction to inshore studies, with occasional voyages as the guest of the fishermen, could never produce adequate answers. For example, it was a canon of the earlier investigators that the inshore area provided the nursery for the tiny cod of 2–3 in. length, because in the late summer these could nearly always be found in the rock pools. The truth of the matter was, however, found to be very different. Some young cod are indeed found on the mile or two strip of coastline available to the earlier investigators, but the nursery area for the North Sea, as a whole, includes the Dogger Bank to Fisher Bank area, and all the grounds to the south and east, which amounts to more than a third of the whole North Sea. This information was only obtained by several expensive voyages of a large trawler fitted for research.

There are many other examples of the necessity for large-scale operations to determine essential facts in the natural history of fishes. Nowhere has this been better shown than in the matter of growth. One of the very important discoveries since the Exhibition of 1883 was that the various hard structures in fish—bones, scales or otoliths—showed rings on them corresponding with the age of the fish. This discovery, or rediscovery, was made by Hoffbauer in 1892 for carp, and at the beginning of this century it was being applied to the sea fishes. Not all the scales give correct readings in temperate waters, but most of them do, and the record of winter on the scale is on the whole more reliable where the winters are more severe. With the aid of these structures the average age of all the fish in a sample can be determined. In addition, the growth of the fish in past years can also be found by measuring the growth on the scale.

My seniors have told me how at first they expected to find a characteristic growth-curve for each species—the cod, the herring, the hake and so on. This, however, eluded them; samples of cod from Iceland showed a different growth from those in the North Sea, and similar differences between different regions prevail in every species. Even in nearby regions there are differences: the growth-rate of plaice in the English Channel exceeds that of plaice in the North Sea. Recent work has shown that within the North Sea itself there are divisions: in summer a thermocline, or discontinuity in the water layers, becomes established over the deeper parts of the North Sea, denying the warmth of summer to the bed of the sea; and on grounds in that area the growth-rate of cod and of haddock is about half that in the shallower regions.

Furthermore, the growth-rate is not the same from year to year, but varies inversely with the density of the fish on the ground.

A very interesting experiment was carried out by G. T. Atkinson in the early years of this century. He brought back some plaice from the Barents Sea, from crowded grounds which had only recently been found by the English trawlers. Otoliths of plaice from there showed very slow growth. Those of his captive fish that survived, in his barrel of water on the deck of a trawler, he marked and let go near the Dogger Bank. When the marked fish were again caught it was found that they had enjoyed a new lease of life, and had started to grow with the fast growth characteristic of the ground where they had been liberated. Rather earlier, it had been discovered that removing plaice from the crowded nursery grounds of the Dutch coast to the better and more thinly populated feeding grounds of the Dogger Bank resulted in doubling their growth-rate.

All this variation has meant that determination of average growth-rate of fish became a statistical problem of alarming proportions, and it took many years before naturalists could fairly say what the growth-rate is in many regions, under various conditions. Certainly we have lost the old aim of determining a growth-rate characteristic of each species; but, curiously enough, generalization is possible in another way. In one region, the North Sea, under its normal heavy fishing, we can speak for all the important species together. For cod, haddock, plaice and herring, we can say that a good fish, by housewifely standards, is five years old, a notably large fish is ten years old, and a notably aged (and not very palatable) fish is twenty years old.

In most of these studies the only method by which generalization has been possible has been by use of the statistics of the markets. On our English and Scottish fishmarkets are found fish from every part of the North Sea, from the western waters as far as the Atlantic slope, from the Arctic as far as the ice. These fish are recorded by special men in the markets and the place of origin is noted. When all these returns are put together and summed, a series is provided by which the observations of naturalists in particular localities or seasons can be weighted, and so integrated. In this way it has been possible to trace out the main spawning areas and seasons, the main migrations, the main growth-rate and the main areas of immature but catchable fish. So have commercial statistics made possible the solution of problems in natural history.

I put the absence of good statistics as the second barrier to progress in the early years of the science.

It would not, however, be correct to suggest that inshore researches have contributed little or nothing to knowledge. That is far from being the case. Where those researches have led to the statement of a principle, this has been found of general application. Thus sea creatures tend to move upstream to spawn—this is true for the crabs of the east coast of Britain, for cod, plaice and many other fish in the North Sea, for salmon in rivers, and even for eels in the Atlantic Ocean.

Migration before spawning is connected with a period of helpless drift in the life of the eggs and larvæ, which is part of the life-history that has only been traced out by special voyages of investigation, and of which still too little is known. It is tolerably certain that the fluctuations in numbers of the fish population are determined during that six to ten weeks of life in the plankton, but the factors in the process have not been evaluated.

By this kind of research, repeated for many species of fish in different regions, the position has been reached that we know the outline of the natural history of nearly all the populations of fish in the northern hemisphere that provide the staple sea fisheries—and something of those in the southern hemisphere.

I think that the shades of Huxley, Ray Lankester and Alward would take a good deal of interest in this progress since 1883. The requirement stated for thorough investigation of the life-history of the food fishes has, in fact, been met. The only criticism that I can imagine them making would be that it would have taken thirty years instead of sixty if they had had a hand in it.

I should expect, however, a good deal of trouble with Huxley over the third requirement—what I have called the population problem of fish and fishing. My first statement here would be that, far from the catch being an inconsiderable proportion of the stock of fish in the sea, the industry has now grown until, for heavily fished stocks such as the plaice, cod and haddock of the North Sea, the catch in this century has become about 70 per cent of the fishable stock.

Here I should, rightly, be challenged to produce the evidence for a statement that presumes to know the number of fish in the sea; and I should say that there are two independent lines of evidence which agree in making the catch of that order of magnitude. First, naturalists of all countries have marked large numbers of plaice and cod with numbered buttons, and let them go in the sea again. The same has been done for halibut in the Pacific and for several other species in various parts of the world. Marking fish has been a most fruitful technique; and fishermen have become well used to recognizing marked fish and returning the mark, or both mark and fish, to our officers at the ports. With due precautions and adjustments for lost marks, the percentage of fish liberated that is returned to us through the commercial fishery is an index of the percentage of the stock that the commercial fishery takes. That is the first line of evidence. The second is this. In the 'nineties, Henson and his colleagues started to sample plankton quantitatively, including the floating fish eggs. In the hands of Buchanan Wollaston, particularly, this method has been developed, until an estimate of the total number of eggs liberated in a season can be made. This number of eggs can be converted to the number of mature female fish, and that again to the number of fishable fish of that species in the sea. From the commercial statistics we can estimate the number of fish in the catch; and so we have an estimate of the ratio of catch to stock.

Both these estimates have only been possible for one population of fish, the plaice in the North Sea. However, in the commercial statistics of cod and haddock there are certain signs, namely, the fall in the catch per unit of fishing power, the increasing scarcity of older fishes in the catches, and the recovery that these stocks make when fishing is interrupted, all of which indicate about as high a rate of fishing on haddock and on cod in the North Sea as on the plaice, for which our special technique has given a numerical estimate.

So, the nature of the case, from which Huxley argued, is by now very different from what it was in the past. Already, in those days, the catches of the deep-sea fishing smacks were showing a noticeably smaller weight of flat fish caught per vessel than had ruled formerly, and the same phenomenon has continued since, in every fishery where the fisherman has power at his command, and is not limited by his market. This phenomenon, reduction in weight caught per unit of fishing power, is a very common one all over the world. It undoubtedly shows a reduction in the weight of fish in the sea, and a reduction in the return to the fisherman per unit of effort.

By way of an example let me quote what I believe to be the very earliest statistics to show this phenomenon.

CATCH PER TRAWLER PER ANNUM OF SOLES AND BUTTS

	Tons	cwt.	To the nearest ton
1864	15	5	15
1865	11	15	12
1866	9	4	9
1867	8	10	9
1876	4	14	5
1880	5	1	5
1881	4	19	5
1882	5	15	6

The story is just the same wherever a fishery has developed.

It is evident, therefore, that if a fisherman is to continue to earn a living, he can only do this by increasing his efforts. He must work harder, and invent new and better fishing gear, or use a larger or faster ship. In that way each man can continue in business. But, of course, this reduces the weight of the stock still more.

I have talked of the weight of the stock in the sea being reduced, but in all fisheries where there are good statistics this has not at first meant any reduction in the total weight landed each year. On the contrary, the fishery as a whole grows each year at first. But experience has shown that the process does not continue indefinitely: there comes a stage where an increased effort produces no increase in total weight caught; and in fact there are many fisheries where a decrease has followed.

At the stage when increasing effort produces no greater catch commercial depression sets in, and becomes chronic. I am here only describing known facts, but facts that were unknown in 1881, and not conceived possible by Huxley and by those who thought with him.

I will not here recount the mechanics of this characteristic process—a falling weight per unit of effort, and a total yield that first rises, then rises no more, and sometimes falls: but it is in fact all explicable in terms of the growth-curve of individual fish, and of the rate of mortality by fishing and the rate of mortality from natural causes. These relations have been demonstrated in papers written since 1935, or before, and are thus well established. A broader explanation is probably of more use here. I can put it this way. You can fish at a *high* rate and take a large percentage of stock kept *small* by that high rate, or fish at a *low* rate and take a small percentage of a stock allowed to grow *big* by that low rate. Or you may have your equilibrium at any point in between—or so it seems to me; because I believe that the multifarious systems of animals and plants behind the fish stock can nourish young fish or old using different food chains. The catch is therefore stock × fishing, with one going up as the other goes down. Personally, I do not find it at all surprising that extremes of values of the factors give a less product than intermediate values of both.

If, for the present purposes, it is allowed, on the grounds as it were of symmetry, that moderate fishing will give the greatest yield—and the student who has more time at his disposal may check the precise evidence for that conclusion—then some very serious and far-reaching consequences follow.

Let us look at a commercial consequence first. It is necessary to explain that the great steam-trawling industry of Britain has expanded by adventure—that is, by successive exploitation of new grounds, always farther and farther afield. In the years preceding the War, the far Arctic was the new Eldorado, where the cod was still at an early stage of the fishery, where the more you fish, the more the total catch rises. But on all the nearer grounds, such as the North Sea, Irish Sea, and Atlantic Slope, the total catch has ceased to rise long ago.

Yet the traditional psychology of pursuit remains, and whenever these near-waters fisheries appear promising, more ships, or better gear, or better fishermen, immediately engage.

Experience shows that this may be successful for a little while, as when new trawls were profitable in the North Sea for about three years from 1924 onwards; but of course the new effort drives the stock down to a new low level, and the fishery is unprofitable again.

So the net effect of the psychology of adventure is to give an average equilibrium, governed only by the equation:

Average profit = nil.

This is what happened in the Dreary Thirties—as there is ample independent evidence from account books to prove.

The Great Law of Fishing is that unlimited fisheries become unprofitable. It is clear that the only adequate measure to conserve the fishery is to set some limit to the amount of fishing. The alternative is what we have in fact seen, the history of boys not being so unwise as to join an industry where unexampled hardship is coupled with such unattractive prospects.

However, the moment you agree to set a limit to fishing power, you find that you have in practice taken on responsibility for the way of life of the fishermen—and that brings me to the social consequence of the natural law of the stock of fish. Here it seems to me that Huxley would be on my side. He had such a practical and clear-sighted appreciation of arrangements as they affected people, as he showed in his statement of the position of the crofters, in the passage that I have quoted.

No one likes ordering the lives of other people, but control forces us either to order them or to disorder them ; and to order them seems to me to involve fostering all the qualities that we admire in them. Thus we could foster individual enterprise, inventiveness, skill, technical improvement, better conditions, and any other thing that we know to be good.

Thus it seems to me, at any rate, fishery science has in these sixty years since the Exhibition brought in the power of knowledge. I do not suppose that we have more than a first approximation to the truth yet—in many departments of the science—but we have that approximation established beyond any doubt whatever, and we know exactly what has to be done.

In 1868 the scientific attitude liberated the fishery from unnecessary regulations, and allowed it to become great. But the industry has now reached the bounds set by Nature, and science, which allowed the child to grow to manhood, has now the knowledge to say what is proper in a mature industry, dependent on a natural resource.

SYNTHETIC RUBBER PROBLEMS

THE chairman of the Division of Rubber Chemistry of the American Chemical Society, at the spring meeting of the Division in April in New York City, well said that while the achievements of rubber technologists with natural rubber in a hundred years of progress are a subject for just pride, the actual production work with synthetic rubber in less than a hundred weeks is nothing short of a miracle.

Nevertheless, the processing problems involved in using *GR–S*, which is the butadiene–styrene polymer mass-produced in the United States to replace the million tons of natural rubber in enemy hands, are still numerous, difficult, and of serious significance to the available output and service life of the products. There is ample evidence of this in the papers read before the Rubber Division, the great majority being devoted, directly or indirectly, to *GR–S* processing difficulties.

Six to twelve months ago, the rubber manufacturer's primary anxiety was to masticate, plasticize and render self-adhesive or 'tacky' the *GR–S* mixings from which rubber articles are built or moulded. No entirely satisfactory methods are yet available for this work, but provisional practical processes have been made generally known with which we can 'make do' until better are found. The relief from this anxiety has transferred attention to vulcanization difficulties, of which the variability aspect is the worst. A paper by F. E. Rupert and F. W. Gage reveals that atmospheric humidity is a factor of the first importance in the question. Exposure of unvulcanized mixings to increased humidity and increased length of exposure increases the rate of vulcanization by as much as one hundred per cent. Other evidence has been accumulating for some time, all indicating that very exact control of the water content of *GR–S* mixings is of vital practical importance.

Revolutionary ideas on the acceleration of vulcanization were given in a paper by A. A. Somerville in announcing two new copper compounds as being several times as active as conventional accelerators. So little as 0·01 per cent by weight of ordinary 300-mesh copper powder added to a *GR–S* mix will shorten the vulcanization period by half, and the new copper compounds are even more effective. Moreover, instead of the rapid perishing frequently encountered in natural rubber contaminated with copper, *GR–S* articles containing the copper accelerators actually have increased resistance to ageing. Sixteen other metals failed to show these remarkable effects.

Equally novel was a paper read by G. M. Wolf, T. E. Deger, H. I. Cramer, and C. C. de Hilster, on the successful use of a new class of vulcanizing agents —alkyl phenol sulphides, in particular *p*-tertiary-amylphenol disulphide. These agents are claimed to possess two very important advantages over sulphur in that (1) they impart some tackiness to *GR–S* mixings, and (2) they give products with outstanding resistance to deterioration by heat. The heating up of large tyres in use, which is the most serious of the unsolved production problems, since the only remedy at present is to use up the scanty stocks of natural rubber, may at least be ameliorated by the application of these sulphide vulcanizers. The heat build-up problem was touched upon, also, in a paper by G. M. Massie and A. E. Warner, who report that the use of a non-persistent accelerator, for example, a substituted lead dithiocarbamate, much reduces the tendency of *GR–S* to stiffen due to service heat, especially if diphenyl-ethylenediamine is used in the mixing.

A third paper, by A. R. Lukens, was also associated with the heat-stiffening question. It was pointed out that the trouble arises mainly from the presence in *GR–S* tyres of liberal loadings of very fine particle carbon blacks. Such loadings have become customary since the other reinforcing agents often used in natural rubber—zinc oxide, magnesium carbonate, whiting, for example—fail to confer any improvement in *GR–S*. It has now been shown, however, that mineral pigments such as those noted can be prepared in much more finely divided forms than hitherto, with the result that they considerably reinforce *GR–S*, but do not cause heat build-up. Mixings containing lower proportions of carbon black and these new fillers may well be a great improvement on to-day's practice.

Other papers at the meeting dealt with various standard problems, adding much valuable information without conspicuous novelties. Reference may be made in conclusion to an account by R. A. Emmett of an interesting range of raw materials comprising blends of various butadiene rubbers with plasticized polyvinyl chloride. Both vulcanizable and non-vulcanizable plastics have been developed in this range. They possess the good qualities of both components and provide a much-needed economy in rubber in a wide variety of applications.

OBITUARY

Dr. Ida Smedley-MacLean

THE death of Ida Smedley-MacLean on March 2 is a grievous loss to her many friends and has left a blank in her various fields of activity which it will be hard to fill. The British Federation of University Women, which she was largely instrumental in founding, owes much to her for her devoted service and for the breadth of vision she brought to its work. A resolution passed by the fellowship of the International Federation of University Women at a meeting in Washington on May 6 refers to one of the phases of work which she helped to launch and which has grown into a very valuable contribution to original investigation. The resolution reads as follows: "That the Fellowship Awards Committee of the International Federation of University Women records its grief at the grave loss suffered by the International Federation of University Women in the death of Ida Smedley-MacLean who for so many years played a leading part in the establishment and awarding of our International Fellowships, and who in her own work set a high standard in that type of scholarly research which we have tried to foster".

WINIFRED CULLIS.

After graduation at the University of Manchester, Dr. MacLean (then Miss Ida Smedley) worked on certain problems in pure organic chemistry under Prof. H. E. Armstrong, investigating *inter alia* the cause of colour in the dinitrobenzenes. She also acted as demonstrator in chemistry and carried out researches on problems concerning the increase in molecular refractivity of compounds containing a conjugated ethenoid linkage structure. This work resulted in a very interesting communication on the diphenylbutadienes and hexatrienes published in the *Journal of the Chemical Society* of 1908. Shortly after this early work, Miss Smedley was awarded a Beit Memorial Research Fellowship and went to work in the Biochemical Laboratories of the Lister Institute under Arthur Harden, chemist-in-chief. These laboratories had just been constituted by amalgamation of the Laboratories for Pathological Chemistry under J. B. Leathes (later of Toronto and Sheffield), with the Chemical Laboratory under Harden. One must suppose that about this time Miss Smedley developed that intense and lifelong interest in problems of fat metabolism. Study of fat metabolism and fat synthesis had already been actively pursued at the Institute by J. B. Leathes and the school he there established. It was, however, a field in which few then delved. Dr. Hugh MacLean, who later took a great interest in the study of the lipins, arrived at the Institute as one of Harden's assistants about the same time as Miss Smedley, and their marriage took place in 1913.

Dr. Smedley-MacLean perceived early the biochemical significance and importance of fats. Though to-day much is obscure in the biochemistry of fat, her work has made a valuable contribution to our knowledge of the subject, and particularly to those parts of it which deal with the oxidative breakdown of fatty acids *in vitro* and their synthesis by living organisms. Her early investigations in the field led to the deduction of the presence of a decylenic acid in butter fat, a deduction confirmed by isolation at the hands of other workers some years later. In 1912 appeared two papers in the *Biochemical Journal* on a possible mode of synthesis of fatty acid *in vivo*, in collaboration with Eva Lubrzynska. Laboratory experiments on the condensation together of such simple compounds as aldehydes and pyruvic acid led to the isolation of longer carbon chain substances of a fatty nature. The work crystallized itself in the hypothesis that pyruvic acid was a very probable starting point for the synthesis of fatty acid in the body. It is a hypothesis which merits to this day the most serious consideration by students of the subject. Even now we do not know the actual steps by which fatty acids are built up from carbohydrates *in vivo*. Of added interest and significance in this connexion are the later investigations of other workers on the importance of pyruvic acid in carbohydrate transformations.

With the War of 1914–18, Dr. Smedley-MacLean's energies were diverted to other pressing problems. With Dr. Chaim Weitzmann she worked on the problem of producing acetone on the large scale from starch by fermentation, a project which was eminently successful. With the end of the War, and in spite of greatly increased domestic responsibilities, Dr. Smedley-MacLean again threw herself with great energy into the well-loved work, and many important and interesting papers on the mode of synthesis of fat and carbohydrate in yeast appeared at intervals until about 1939. While seeking a possible laboratory model for the study of the biochemical oxidation of fatty acids, she discovered that hydrogen peroxide in the presence of a cupric salt as catalyst is extraordinarily powerful in its oxidizing action on fatty acids. Within a short time the higher fatty acids may be largely broken down to carbon dioxide, a chemical transformation very difficult or impossible to perform in any other way. This field she cultivated with success up to some few weeks before her death.

From about 1935 onwards interest centred on the fat-deficiency disease of rats discovered by Burr and Burr in 1929 (*J. Biol. Chem.*, **82**, 345; 1929). At the Lister Institute the physiological aspects were more closely studied in conjunction with Miss Hume and Miss Henderson-Smith, while the biochemical side was left to Dr. Smedley-MacLean and myself. Her delight and fascination with this work never left her. Besides many other interesting and significant points uncovered, the position of linoleic acid as the probable precursor of arachidonic acid and other very highly unsaturated acids was established. The nature of the fatty acids stored under conditions of disease and cure was also investigated. Finally, the structure of arachidonic acid itself (originally discovered by P. Hartley in 1909 in the same laboratories) was put forward as a result of investigation on a very small quantity of material. The suggested structure was fully confirmed by subsequent work with larger quantities in the United States.

Much has been omitted from this brief note, but it is hoped that sufficient has been said to indicate the late Dr. Smedley-MacLean's comprehensive and intense interest in the biochemistry of fat. I worked with her for many years, and I retain the sense and knowledge of her masterly grip of her field. She sought out the facts, made very sure of them and then held to them amid much cross-fire. She saw the significant correlations between her facts very clearly and rarely went beyond. As a teacher and colleague she never failed to inspire, and there must be many like myself who look back on their "Lister" days with intensely happy memories.

LESLIE C. A. NUNN.

NEWS and VIEWS

Engineering Science at Oxford: Dr. Alexander Thom

DR. ALEXANDER THOM, who has been appointed to the chair of engineering science in the University of Oxford, graduated in engineering in the University of Glasgow in 1915, and after a varied experience in civil engineering construction both abroad and in Great Britain was engaged in aircraft design in the later years of the War of 1914–18. In 1921 he was appointed lecturer in engineering in the University of Glasgow, and from that time until the beginning of the present War, Dr. Thom was in charge of the subject of aeronautics and also of the specialist courses in civil engineering. This somewhat unusual combination was of considerable value to the teaching in both subjects, and large numbers of engineering students in all branches have benefited in their scientific training from the lectures in aeronautics given by Dr. Thom. His tenure of the post was marked by a large volume of research work in aerodynamics, comprising exhaustive studies of the flow of a fluid past a cylinder, stationary and rotating, employing both a small wind tunnel and, for an experimental investigation of the pattern round a cylinder at low Reynolds numbers, a small channel using water and oil. In the course of his studies he developed an arithmetical method of solution of the equations of flow for both ideal and viscous fluids. The results of his researches, which have throughout been characteristic of the originality of his mind, have been embodied in numerous papers contributed to Reports and Memoranda of the Aeronautical Research Committee, the Royal Society and the scientific and technical Press. Shortly before the outbreak of the present War, he was given leave of absence from his university duties to engage in work at the Royal Aircraft Establishment, Farnborough, and has latterly been in charge of new developments in the Aerodynamics Department.

Twenty-fifth Anniversary of the Genetical Society

AT its annual meeting on July 21 the Genetical Society completed the twenty-fifth year of its activity. A gathering was called on June 25, 1919, under the chairmanship of William Bateson, at which it was agreed to found the Society, and the first meeting was held on July 12 of that year. The original list comprised eighty-seven members, and the first president was Mr. A. J. (later Lord) Balfour, who held office until 1930. The Society's eighty-two meetings have been mainly devoted to communications on fundamental genetics, including also addresses by such foreign visitors as Drs. T. H. Morgan, H. J. Muller, A. H. Sturtevant, C. B. Bridges, R. Goldschmidt and Ø. Winge. Interest has, however, not been confined to this narrower field, but, as Bateson intended, has also covered plant and animal breeding, human and medical genetics and, of course, cytology and evolutionary theory. Visits have been made on a number of occasions to plant and animal breeding centres and exhibitions, as well as to Kew Gardens, East Malling and Rothamsted Research Stations, the Lister Institute and the Gardens of the Zoological Society. Joint meetings have been held with the Society for Experimental Biology and the Entomological Society, and doubtless this kind of activity will increase in the future as the applications of genetics become more widely appreciated. The Society also sponsored the Seventh International Congress of Genetics held at Edinburgh in 1939.

Before the War, three or four meetings were held annually. This programme was interrupted in the early years of the War; but since 1941 a return has been made to the pre-war arrangement. The present aim is to hold one symposium and one paper-reading meeting during the winter and spring months, and to visit some appropriate research centre in the summer. The present membership numbers 146, of whom twelve are overseas. Dr. C. D. Darlington is now president and Dr. E. B. Ford and Mr. W. J. C. Lawrence are joint secretaries.

Education of the Pharmacist

AT the British Pharmaceutical Conference held in London on July 11, the chairman, Mr. H. Brindle, gave an address on the "Education of the Pharmacist". He reviewed the present system of pharmaceutical education and commented on the possible effects upon it of the Norwood Report and the Education Bill. The present qualifications in pharmacy include two diplomas granted by the Pharmaceutical Society: that of the chemist and druggist, and that of the pharmaceutical chemist. In addition, several universities have established degrees in pharmacy. All the diplomas and degrees are registerable qualifications for practice. The chemist and druggist diploma is mainly utilized as the qualification for retail practice, but Mr. Brindle hopes that the higher diploma and the degrees will be in greater demand. Retail pharmacy needs its share of the best brains and the most highly trained pharmacists. It offers opportunities certainly comparable in all respects with those of the other branches of the profession, such as hospital, manufacturing and research.

The Pharmaceutical Society in the past has demanded the university entrance standard for its preliminary examination and Mr. Brindle hopes that this will be maintained. The Norwood Report, however, foreshadows certain complications for retail pharmacy because of the recommendation that the general age for entering a university will in future be eighteen plus. This must lead to an alteration in the present system of apprenticeship. He suggested that apprenticeship has largely outlived its usefulness, and if it is to be retained it might with advantage occur after qualification. The university degrees in pharmacy are now well established, and the graduates are proving their value in ever-widening scientific fields. Mr. Brindle hopes that the post-war educational world will provide facilities for brilliant students unhampered by financial handicaps and that university education will be free to all who show themselves capable of benefiting by it.

Veterinary Practice by Unregistered Persons

IN accordance with the recommendation made by the Loveday Committee on Veterinary Education in Great Britain, the Minister of Agriculture and Fisheries and the Secretary of State for Scotland have appointed a Committee to inquire into the extent and effect of veterinary practice in Great Britain by persons who are not registered veterinary surgeons, and to make recommendations as to any measures which may be desirable to limit or regulate such practice. The Committee is constituted as follows: Sir John Chancellor (*chairman*); Mr. A. C. Brown; Sir Daniel Cabot, chief veterinary officer, Ministry of Agriculture and Fisheries; Mr. J. W. Salter Chalker, chairman of the Diseases of Animals Committee of the National Farmers' Union; Mr. Charles Dukes, general secretary of the National Union of General and Municipal Workers; Prof. James Gray, professor of zoology in the University of Cambridge and member of the Agricultural Research Council; Mr. C. M. Holmes, vice-president of the Association of Unqualified Practitioners and Animal Castrators; Mr. W. F. Holmes, member of Council of the Kennel Club; Mr. Robert Hobbs, member of Council of the Royal Agricultural Society of England; Mr. W. D. Jackson, past president of the National Farmers' Union and Chamber of Agriculture of Scotland; Sir Louis Kershaw, member of the Loveday Committee on Veterinary Education; Lieut.-Colonel P. J. Simpson, member of Council of the Royal College of Veterinary Surgeons. The Secretary to the Committee is Mr. G. H. Higgs, of the Ministry of Agriculture and Fisheries, to whom communications should be addressed at 99 Gresham Street (First Floor), London, E.C.2.

Biological Standardization

BIOLOGICAL standardization is the theme of the *Bulletin of the Health Organisation* (VI, **10**, No. 2; 1942–43. Geneva (London: Allen and Unwin, League of Nations Publications Dept. 4*s*.)). The issue contains two articles on the biological standardization of heparin and on a provisional international standard for this substance, and other articles on standard preparations for the assay of the three gas-gangrene antitoxins, on the complexity of the tetanus toxin and on the variable interactions of tetanus toxins and antitoxins. The rest of the issue is occupied by seven articles from the Department of Biological Standards of the National Institute for Medical Research, London. The first of these deals with recent changes relating to international standards for certain sex hormones and for pituitary posterior lobe, due to exhaustion of stocks of the original preparations which had served as international standards for these hormones and the consequent need for their replacement by other samples. The other six articles deal with replacements of the substances of the international standards for the œstrus-producing hormone, for male hormones, progesterone and pituitary posterior lobe and with the international preparation of desiccated ox anterior pituitary gland and the international standard of prolactin.

Status of Statisticians

THE report of the Committee of the Royal Statistical Society on the Status of Statisticians, appointed by the Council on July 22, 1943 as amended and adopted by the Council, has now been issued. The present position in Great Britain is regarded as unsatisfactory in some respects. First, an employer requiring the services of a statistician on his staff has no common standard among the qualifications which he can accept as a certificate of proficiency, and the report, in confirmation of this point, notes that in recent discussions on the Society's report on official statistics, the Treasury representatives indicated how useful it would be to departments, in considering appointments to responsible positions in statistical branches, if approved statistical qualifications were in existence. The position is also unsatisfactory to an employer, because there are no generally recognized definitions or descriptions of the various types of statistician. It is equally unsatisfactory to the employee, because there is no recognized status in his profession and no generally approved method of distinguishing between a genuinely accomplished man and a mere quack, or even a rank impostor. It is also unsatisfactory to an employee not to have any standards by which he can judge the level of his own attainment, or to which he can work. The situation is unsatisfactory for the general public, which is affected more than it realizes by bad statistical work.

The Committee considers, therefore, that there is a strong case for instituting some method of determining the professional status of statisticians. It believes that the universities must continue to be the main source of training in statistics, and it would welcome any extension of the facilities already provided, although it does not think that the universities can provide adequate tests of proficiency in statistics for all who are likely to require them; also, existing examinations are not sufficient to provide all the requisite professional qualifications. Accordingly the Committee proposes a scheme under which power would be sought by way of a Supplemental Charter to enable the Council of the Royal Statistical Society to confer on approved candidates a diploma in statistics and to issue a certificate to those who pass Part 1 of the examinations. This would provide specifically for those who may be termed statistical computers or junior statisticians. Candidates for the certificate or diploma should not be limited to fellows of the Society, and those for the diploma should be required to pass Parts 1 and 2 of the prescribed examinations, a suggested syllabus for which is appended to the report, and also to show that they had had satisfactory experience of statistical work over a period of not less than two years. A candidate for the certificate or diploma should be exempted from the whole or any section or sections of the examinations if he has passed examinations approved by the Council. While so far as concerns the scientific aspects of the Society's work, no change is suggested in the present system of election of fellows, in the qualifications required, or in their title, status and privileges, additional by-laws would be required to regulate the award of the certificate and the diploma.

Astronomy in the U.S.S.R.

POST-WAR astronomical research in the Soviet Union is being planned on a great scale. Nine of the nineteen Soviet observatories were in territory that was overrun by the Germans and have been destroyed or seriously damaged. Most important of these was the Pulkovo Observatory, near Leningrad, which was completely destroyed by air and artillery bombardment. Most of the equipment and the valuable library of the Observatory were removed in time to safer places. The Pulkovo staff has continued astronomical research work at Tashkent, Abastumani and Alma-Ata. Prof. Belyavsky, director of the Observatory, states that it has been decided that reconstruction is to commence immediately and that the instrumental equipment will be reinstalled at Pulkovo at the earliest possible moment, to make possible the resumption of work in fundamental astronomy. More powerful equipment is to be constructed in the U.S.S.R. or obtained from abroad. The Engelhardt, Nikolaeff and Tashkent Observatories will also carry on fundamental observations.

The *Moscow News* has reported the decisions of an astronomical conference held in Moscow in September last. A great astrophysical observatory is to be established with headquarters at Simferopol in the Crimea. There will be three observing stations, one in the Crimea at an altitude of 2,000 metres, a solar station at an altitude of 3,500 metres, and a station somewhere in the southern hemisphere. The equipment will include a 120-in. reflector, two 80-in. reflectors, two 16-in. double astrographs, one 50-in. and one 30-in. Schmidt telescope, solar towers and a coronagraph. The training of astronomical staff has continued during the War; some sixty or seventy astronomers and astrophysicists will be required for staffing the new institution. Information has been received that the international latitude station maintained by the U.S.S.R. at Kitab, Uzbekistan, has continued to function regularly throughout the War.

Library Service in South Africa

IN his presidential address to Section F of the South African Association for the Advancement of Science delivered on June 29, 1943, on "Libraries and Science" (*South African J. Sci.*, **40**, 81; November 1943), P. Freer referred particularly to the difficulties of book selection in scientific literature. He urged that this is not a matter for librarians alone, and that librarians should also do more to secure the writing and publishing of authoritative but simple and readable books on important subjects. After pleading for more care and co-operation in the discarding of surplus or unwanted stock, Mr. Freer indicates measures that are required for the full efficiency of the inter-library loan system. Until the master-catalogue of non-fiction in the libraries of the Union of South Africa is completed, as well as the publication of a new edition of the Lloyd's List of Scientific and Technical Periodicals, with a complementary volume covering the humanities, book-buying and discarding are both unscientific. As regards the imperfections and limitations of present abstracting and indexing services for scientific and technical periodicals, Mr. Freer notes that only forty-one of the three hundred abstracting and indexing periodicals in existence in 1937 were available somewhere in South Africa, and these periodicals only covered one third of the scientific papers concerned. He emphasized the need, for the advancement of science, of a highly trained staff and augmented resources to diminish the gap between the 6,000 titles in the Catalogue of Union Periodicals and the 15,000 acknowledged as being of international importance. Plans for post-war reconstruction contained no suggestions for a national library system, but in addition to national libraries, regional libraries and organization would be required. Finally, he referred to the contribution which science could make to the improvement of the printed book itself and its preservation.

Mexican Institute of Nutrition

ACCORDING to an annotation in the September issue of the *Boletín de la Oficina Sanitaria Panamericana*, the newly organized Institute of Nutrition in Mexico includes the following sections: bromatology, economic, social and dietetic departments, clinical medicine and a clinical laboratory, and maternal and child welfare. In the near future, the bromatology section will undertake studies of the chemical composition of legumes and maize, and the economic section will undertake an investigation of the state of nutrition of about seven hundred families in an area of Mexico City. The clinical laboratory will determine the vitamin content and blood count of the families studied. The maternal and child welfare division has already started work on the vitamin content of the blood of pregnant women at different periods of pregnancy, and a training centre has made plans for courses for the selection of dietitians and experts in nutrition.

An Automatic Vibration Analyser

ONE of the potential causes of failures in an aeroplane power-plant or the aeroplane structure itself is excessive vibration. While every effort is made to reduce the number of modes of vibration which might be excited to objectionable amplitudes, it is desirable to measure the power-plant and aeroplane vibration characteristics in flight to ensure that there are no vibrations of sufficient magnitude to cause fatigue failures in any parts. In the Bell Laboratories an automatically tuned wave analyser has been developed to meet the needs of the Pratt and Whitney Aircraft Division of United Aircraft Corporation, which desired apparatus that when used in conjunction with suitable vibration pickups would measure the amplitude of vibration at a frequency equal to any predetermined multiple of engine revolution per minute, even when the engine speed is continuously changed. This analyser can be used with a recorder to draw curves of the amplitude of vibration of selected orders of frequency as a function of engine speed. An article by F. G. Marble (*Bell Lab. Rec.*, **22**, No. 8; April 1944) describes the analyser and its use.

Illuminating Engineering Society

DURING the past few years the membership of the Illuminating Engineering Society has approximately doubled, it has formed new centres and groups throughout Great Britain, and its activities have greatly increased. In addition, it has in prospect a programme of still greater activity and of new developments in the post-war period. In order to cope with these developments the Council is now contemplating the appointment of a full-time paid secretary who, it is hoped, will eventually take full charge of its administration. In the meantime, however, the Council hopes to continue to benefit for some time to come from the services of its honorary secretary, Mr. J. S. Dow, who has been associated with the Society since its inception in 1909.

Institution of Electrical Engineers: Radio Section

ON the recommendation of the Wireless Section Committee the Council of the Institution of Electrical Engineers has decided to change the name of the Section to "Radio Section" and to modify Rule No. 1, which deals with the scope of the Section, to read as follows: "The Section shall include within its scope all matters relating to the study, design, manufacture or operation of apparatus for communication by wave radiation, for high-frequency and electronic engineering, or for the electrical recording or electrical reproduction of sound".

ERRATA.—In the communication "Physico-Chemical Properties of the Surface of Growing Plant Cells" by Prof. H. Lundegårdh and G. Stenlid in *Nature* of May 20, p. 618, Fig. 1 and Fig. 2 should be interchanged. The absorption maximum of the flavonone is in two places in the text erroneously assigned to 2550 A. instead of 2850 A.

LETTERS TO THE EDITORS

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

A Notation for Organic Compounds

DR. A. R. RICHARDS[1] proposes a form of 'chemical shorthand' to designate the commoner hydrocarbons and their simple compounds. This he claims may avoid the practice of coining names such as 'triptane' for '2,3,3-trimethylbutane'; in practice it will not have this effect. Once a substance leaves the laboratory and enters the plant, it *must* have a short distinctive title by which all and sundry can refer to it; and in 'triptane' such a word has been found. What we must most sedulously avoid is the unwarrantable intrusion of such coined names into systematic nomenclature. G. C. Foster, so far back as 1865[2], pointed out that all sciences have two distinct requirements of nomenclature—a convenient *general* language and a systematic or 'legal' language. The former serves for the ordinary everyday transactions of science and manufacture and will, "in the main, take care of itself; and at any given period it usually contains a large admixture of terms—once technical, but now no longer used for purposes of accuracy—which, like fossils in a rock, tell of the successive changes by which the existing state of things has been brought about". The strictly legal or premeditated language of organic chemistry is for cataloguing and identifying substances with absolute precision and expedition.

At present we have a choice in the exact delineation of an organic structure between an ideograph (the structural formula) and a so-called 'systematic' name. The ideograph is space-consuming, has no sound equivalent and cannot be indexed in list form, since a series of structural formulæ has no intrinsic basis of ordered arrangement; on the other hand, it is immediately intelligible to the eye and easily remembered. 'Systematic' names are cumbrous, often ambiguous, very long to print, and have never attained more than a measure of popularity with practical chemists. They impose an intolerable burden on the memory, being related to a large group of 'trivially' named nuclei, many of which are virtually unknown. Further, they are often unpronounceable owing to the use of various types of brackets, subscripts, dashes and the like which not only complicate printing but also have no simple vocal equivalent [for example, pronounce the following: Spiro [3-naphtho [1,2] triazole-2,2′-3′-ox-2′azatricyclo [3.3.3]. nona-1′(8′),5′(9′)6′-triene] and then jot down its structure (Ring Index No. 3179)].

Most chemists actually remember the structure or ideograph and associate the idea of a name; after a time many structures and names become so closely associated as to be almost indissoluble in the memory; but such are only a small fraction of those ordinarily used, while the rules and exceptions of systematic nomenclature are tedious both to learn and to apply.

Ordinary systematic nomenclature has failed to give that degree of precision and ease of reference required by modern developments of the science, and I have developed during the past few years a system of ciphering for organic compounds which provides a method of reference and classification capable of providing a unique cipher for every structure. Such ciphers have a logical system of enumeration, use only the capital letters, θ, and the Arabic numerals, without subscripts, brackets or dashes and are, moreover, capable of being 'interpreted' on a punched card system; the 'sorting' of cards into categories can be readily accomplished and a form of mechanical indexing and reference is thus achieved. In addition, the machine can automatically compute, from the card, the empirical formula of the compound.

Space does not permit the discussion of details of this system, which will be published elsewhere shortly, but my purpose in writing will be served if attention is directed to (*a*) the inevitability of short or trivial names for substances in common use; (*b*) the undesirability of allowing such names further to complicate our systematic nomenclature; (*c*) the desirability of any universally adopted cipher system being complete, international and mathematically adaptable.

G. MALCOLM DYSON.

Research Laboratories,
Genatosan, Ltd.,
Loughborough.
June 24.

[1] *Nature*, **153**, 715 (1944).
[2] *Phil. Mag.*, **29** (April 1865).

A Silicified Member of the Cyclanthaceæ from the Tertiary of the Deccan

NEW discoveries of fossil plants, while frequently solving problems of geographical distribution, often throw up fresh problems that baffle solution. Recent work on the silicified flora of the Deccan Intertrappean Series (which with all deference to the 'official' view of the Geological Survey of India we regard as Early Tertiary and not Late Cretaceous[1]) has brought to light genera which are either identical or very closely related to living types now confined to parts of South America. A year ago, one of us reported the occurrence in these beds of well-preserved silicified sporocarps essentially of the Regnellidium type, which he referred to a new genus, Rodeites[2], closely allied to the Brazilian water-fern *R. diphyllum*. We now record the occurrence in the same locality (Mohgaon Kalan, in the Central Provinces) of a new silicified member of the Cyclanthaceæ, a family now living only in tropical America. Of this family we had hitherto no definite fossil record. The leaf-impressions from the Eocene of Sézanne, which Saporta[3] referred to a new genus, Ludoviopsis, may equally probably belong to palms.

We recently suggested[4] that the silicified stem from the Deccan, described by K. P. Rode in 1933 under the name *Palmoxylon Sahnii*[5], is probably not a true palm but an extinct member of the Cyclanthaceæ. A detailed anatomical examination of the vegetative organs of this plant, of which two large specimens were found by one of us at the same locality in 1941, has now proved that our surmise was correct. In its main anatomical features the fossil shows a close resemblance to Cyclanthus. Thus the root structure is very similar to that of the modern genus; in both forms the pith is fibrous and there are ten or more protoxylem groups. The fibrovascular bundles of the stem, which Rode described in some detail, also resemble those of Cyclanthus in the crescentic arrangement of the xylem vessels and in the peculiar compound (? branched) nature of the bundles. As in Cyclanthus, too, the leaf sheaths contain a row of large secretory canals.

The flowers and fruit of the fossil are unknown, hence the exact affinities are still rather obscure. But the anatomical resemblances with Cyclanthus are set off by the arboreous habit of the plant which, as stated last year, "must have grown rooted in shallow water, with the stems rising in a clump, somewhat like those of a bamboo"[4]. These differences, coupled with the distance in time and space which separates the living and the extinct forms, suggests that the fossil should at least provisionally be referred to a new genus of the sub-family Cyclantheæ.

The generic name Cyclanthodendron is proposed. This new genus provides (like Rodeites) another interesting link between the Early Tertiary flora of the Deccan and the modern flora of South America.

B. SAHNI.
K. R. SURANGE.

Department of Botany and Geology,
University of Lucknow.
April 19.

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[3] Saporta, *Mém. Soc. Géol. France*, **8**, 338, pl. IV, figs. 1, 3 (1868).
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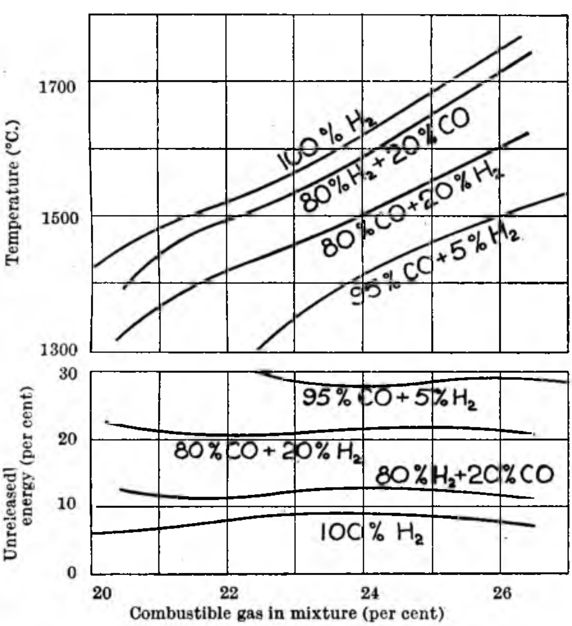
Unreleased Energy in Flame Gases

WE recently reported[1] the results of temperature measurements made by means of very fine quartz-coated platinum wires in the flame gases resulting from the combustion of hydrocarbon–air mixtures in a specially constructed burner. From these measurements estimates were made of the proportion of the heat of combustion which was unreleased in the flame gases for the purpose of increasing their temperature. The unreleased energy, we believe, is due partly to latent energy held in stable form in some of the newly formed tri-atomic molecules and partly to abnormal dissociation resulting therefrom. It varied from about 10 per cent to rather more than 20 per cent of the heat of combustion.



Similar measurements have since been made using mixtures of air with hydrogen and with carbon monoxide plus hydrogen in various proportions. The curves in the top portion of the accompanying graph show these measurements. It will be seen that for any given mixture strength the hydrogen flame gas temperature is more than 200° C. higher than the carbon monoxide (with 5 per cent hydrogen) flame gas temperature—and this in spite of the fact that the calculated hydrogen flame gas temperatures are more than 100° C. lower than the calculated carbon monoxide flame gas temperatures. The measured temperatures have not been corrected for radiation loss, but even after making full allowance for this, the hydrogen temperatures are of the order of 150° C. below and the carbon monoxide (with 5 per cent hydrogen) temperatures of the order of 500° C. below the corresponding calculated temperatures.

Curves giving estimates of the unreleased energy are shown in the lower portion of the graph. In the hydrogen flame gases this is of the order of 8 per cent of the heat of combustion and in the carbon monoxide (with 5 per cent hydrogen) flame gases it is nearly 30 per cent. There is little doubt that in flame gases resulting from the combustion of carbon monoxide with a much smaller proportion of hydrogen it would be considerably greater than 30 per cent.

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[1] *Phil. Mag.*, **34**, 816 (1943).

Rayleigh Quenching of Active Nitrogen

ARISING out of work[1] on the disappearance of the nitrogen afterglow by heating the activated gas at various pressures, it may be of interest to record results which indicate limits for the production of a like phenomenon observed by Lord Rayleigh[2], namely, the quenching of the afterglow due to cooling by liquid air. With a condensed discharge in a Crookes' tube connected in series with a long glass spiral *S*, and a small bulb containing powdered iodine, the activated gas excited the characteristic iodine luminescence even when the afterglow in the observation spiral *S* was feeble; both disappeared when *S* was cooled by liquid air, in agreement with Rayleigh's observation[2]. That this phenomenon is, however, restricted to low pressures is shown by the fact that when the gas pressure was increased progressively from 0·1 mm. the Rayleigh quenching in *S* tended to be less marked; was uncertain near 30 mm. and not observed above 40 mm.

A possibly insufficient cooling of the activated gas at high pressures was minimized as follows. The nitrogen was first streamed through a long spiral of glass, which enclosed the Crookes' tube; while both these were well immersed in a bath of liquid air, the gas was subjected to the discharge. A distinct afterglow was produced in *S* even when it was cooled by liquid air, and was accompanied by luminescence in the iodine bulb. Both these effects were observed also at smaller pressures, at which under normal conditions (that is, when not cooled by liquid air before and during activation by the discharge) the Rayleigh quenching occurred. Addition of one more spiral cooled by liquid air, just before the one surrounding the discharge tube, making the total cooling path about three yards in length, had little effect on the results. A high gas pressure and low temperature activation, therefore, are unfavourable to the occurrence of the Rayleigh quenching.

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April 5.

[1] Joshi and Purshotham, *Proc. Ind. Acad. Sci.*, **19**, No. 4 (1944).
[2] Strutt, R. J., *Proc. Phys. Soc.*, **23**, 66 (1910); *Proc. Roy. Soc.*, A, **85**, 219 (1911). Trowbridge, *Phys. Rev.*, **23**, 279 (1906).

Constitution of some Binary Oxide Systems

THE constitution of the oxide systems FeO–MnO, FeO–MgO, CaO–MnO and MgO–MnO have been studied by the X-ray diffraction method. Preparation of the powder samples was carried out *in vacuo* at temperatures up to 1350° C., followed by slow cooling to 1150° C., from which temperature the samples were rapidly cooled.

The X-ray patterns showed that the four systems are single phase. It was possible to make accurate lattice dimension measurements for all samples. The result on the FeO–MnO system is in agreement with that of Andrew, Maddocks and Howat[1] and of McCaughey[2], but in contradiction to that of Benedicks and Löfquist[3] and Hay, Howat and White[4]. The FeO–MgO system has been reported to be a single phase system by Bowen and Schairer[5], following the use of thermal and optical methods. References have not been found to any previous work on the CaO–MnO and MgO–MnO systems.

Full details of the work will be published later.

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[1] Andrew, Maddocks and Howat, *J. Iron and Steel Inst.*, **2**, 283 (1931).
[2] McCaughey, Amer. Open-Hearth Proc. 1938, Discussion, p. 169.
[3] Benedicks and Löfquist, "Non-Metallic Inclusions in Iron and Steel" (1930).
[4] Hay, Howat and White, *J. West Scot. Iron and Steel Inst.*, **41**, 97 (1933–34).
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A Molecule-building Principle

IT has long been realized in the field of molecular spectra that there is need of some form of molecule-building principle equivalent to the *Atombau* principle of Bohr. This would enable the electron configuration and term type of at least the ground-state of any molecule to be predicted from its position in the Periodic Table. Mulliken[1] attempted to trace the change in electron configuration from molecule to molecule for the lighter members of the Table, but his pioneering work was hampered by the absence of many necessary spectrum data, and no progress has since been made. In particular, very little knowledge has been obtained on the configuration of the heavier diatomic molecules.

I have been studying for some time the spectra of the heavy fluorides of the series, AuF, HgF, TlF, PbF and BiF, and it is now possible to describe their ground-states in terms of electron configurations. These are shown in Table 1, along with the corresponding iso-electronic oxides and their probable configurations.

TABLE 1.

Molecule	AuF	HgF	TlF	PbF	BiF
Type	$^1\Sigma$	$^2\Sigma$	$^1\Sigma$	$^2\Pi$	$^3\Sigma$
Config.	π^4	$\pi^4\sigma$	$\pi^4\sigma^2$	$\pi^4\sigma^2\pi$	$\pi^4\sigma^2\pi^2$
Molecule	AuO	HgO	TlO	PbO	BiO
Type	$^2\Pi i$(?)	$^1\Sigma$	$^2\Sigma$	$^1\Sigma$	$^2\Pi$
Config.	π^3	π^4	$\pi^4\sigma$	$\pi^4\sigma^2$	$\pi^4\sigma^2\pi$

Iso-electronic molecules connected by dotted line.

It is evident that a building principle is operating in which the configuration of a given molecule retains the structure of the preceding with the addition of an electron to the lowest available orbital.

It is of interest to note the probable configuration and term type of the molecules AuO (AgO and CuO) and BiO, the spectra of which are still in a state of confusion. More information can be derived for the $^2\Pi$ BiO state by examining the doublet separations of the $^2\Pi$ states arising from $\pi^4\sigma^2\pi$, given in Table 2.

TABLE 2.

CF	NO	SiF	PO	GeF	AsO	SnF	SbO	PbF	BiO
(60)	121	161	221	(920)	1027	2317	2272	8260	

Bracketed values are predicted in forthcoming publication.

This suggests that an electronic interval of the order of 8,000 cm.$^{-1}$ should be sought for among the many bands of this molecule.

A fuller report of this work will be published elsewhere.

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[1] *Rev. Mod. Phys.*, **4**, 1 (1932).

Torulopsis utilis and the Citric Acid Cycle

THE ordinary strain of *Torulopsis utilis* (Henneberg) deriving from Haehn[1] can easily grow on a great many substrates under aerobic conditions (cf. Fink[2]). Nevertheless, there are still many substrates to which the Torulopsis yeast can be accustomed. Among others, I have studied some of the acids belonging to the so-called citric acid cycle of Krebs[3].

In my experiments the standard Torulopsis yeast was cultivated on ethyl alcohol, ammonia and salts, including the sulphates and phosphates of potassium, magnesium and calcium. These salts being of technical quality, it is probable that they contained other elements necessary for the growth of the yeast, for example, iron.

Cultivations were carried out under vigorous aeration. The substrate, except in the earliest stages of adaptation to a new substrate, was added in small portions, using a time schedule, so that the concentration of substrate might be as small as possible.

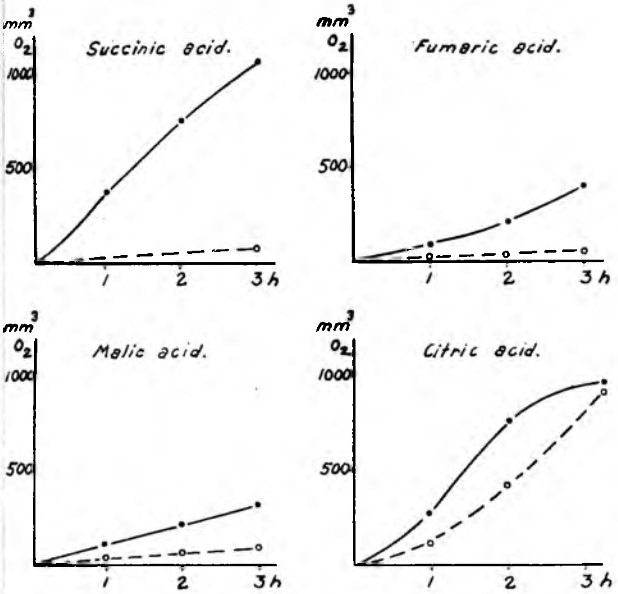

Fig. 1. UPTAKE OF OXYGEN BY *Torulopsis utilis* IN THE PRESENCE OF VARIOUS ACIDS. THE YEAST WAS ADAPTED TO SUCCINIC ACID. EACH WARBURG CUP CONTAINED 20 MGM. OF FRESH YEAST, 1 ML. 0·2 *M* KH_2PO_4, 0·3 ML. 0·1 *M* ACID, NEUTRALIZED TO *p*H 4·5 AND WATER TO 2·4 ML. THE EXPERIMENTS REPRESENTED BY THE BROKEN LINES CONTAINED ALSO 0·2 ML. 0·1 *M* MALONIC ACID.

It was found that the standard yeast grown on ethyl alcohol could not initially use succinic acid but that it could be adapted rather easily to that substrate in the presence of ammonia and salts. It was much more difficult to adapt the yeast to malic, fumaric or citric acid, which also were not attacked initially. The yeast cultivated on succinic acid did not at first show any noteworthy new properties as detectable by adding the acids just mentioned to a suspension of yeast and determining the oxygen uptake in the Warburg apparatus. By cultivating the yeast twice more on succinic acid using the same technique, it was found, however, that the yeast had changed. It was now able to attack all four of the acids just mentioned, but not maleic, *trans*-aconitic or malonic acid. Moreover, on adding malonic acid and one of the acids succinic, malic, fumaric or citric simultaneously, it was found that the oxygen uptake was to a great extent inhibited, which had not formerly been the case (Fig. 1).

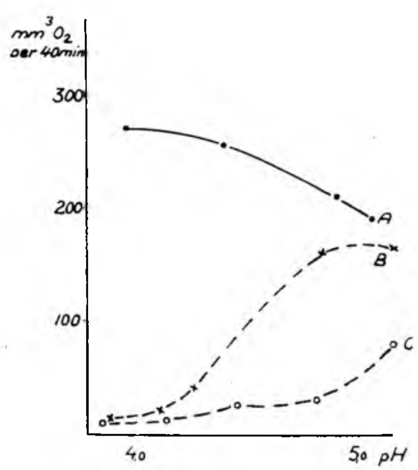

Fig. 2. UPTAKE OF OXYGEN DURING THE FIRST 40 MIN. OF THE EXPERIMENT BY THE SAME YEAST AS FIG. 1. EACH CUP CONTAINED YEAST, KH_2PO_4 AND WATER AS BEFORE AND 0·3 ML. 0·1 *M* SUCCINIC ACID. *B* ALSO CONTAINED 0·1 ML. AND *C* 0·2 ML. 0·1 *M* MALONIC ACID.

To ensure that the *Torulopsis utilis* had not been contaminated with any other kind of yeast, samples were spread on agar plates and found to be identical with the original yeast and very pure.

The activity of the Torulopsis yeast towards succinic acid (and towards the other acids) is very dependent on the *p*H of the suspension, being greater at acid *p*H (Fig. 2).

The property of the yeast of being able to attack succinic acid, etc., had not altered its behaviour towards other substrates such as pyruvic, lactic or acetic acid or ethyl alcohol or glucose, nor did malonic acid inhibit the breakdown of any of them. The inhibition brought about by malonic acid in the case of citric acid is merely a period of induction which lasts about one hour.

The observations described above suggest that although the enzymes of the citric acid cycle are possibly not normal constituents of the Torulopsis yeast, they can be synthesized under the conditions given. It seems justifiable to suppose that a particular enzyme effecting the breakdown of succinic acid is built up during adaptation. The action of this enzyme can be inhibited by malonic acid. The citric acid cycle does not seem to play any considerable part in the breakdown of pyruvic acid in this yeast.

Experiments have also been made to cultivate the yeast, adapted to succinic acid, on citric acid. The yeast grew well though not so fast as on, for example, ethyl alcohol or glucose. All the carbon of the substrate could be accounted for as yeast substance and carbon dioxide, indicating that no intermediates had been accumulated.

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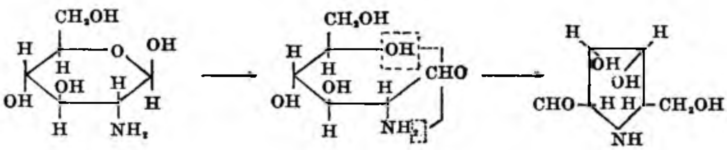
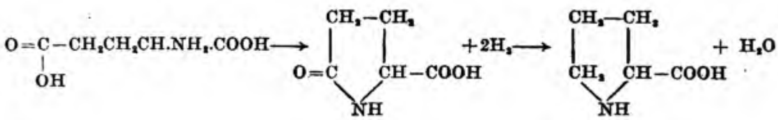
Porphyrin Metabolism in Idiopathic Porphyria

IDIOPATHIC porphyria is especially interesting as the urinary pigment uroporphyrin III associated with this disease resembles closely in its chemical structure the protoporphyrin of hæmoglobin. Urinary uroporphyrin may provide here an index of the rate of protoporphyrin formation in the body, and it seemed important to obtain information on this point, and to observe if uroporphyrin III excretion is augmented by administration of substances which, on a theoretical basis at least, could function as protoporphyrin precursors.

With the excellent co-operation of a porphyric of mild, chronic type, investigations were made on the effect of administration of *d*-glucosamine on porphyrin excretion. It was considered that 2-amino-sugars such as 2-amino glucose or 2-amino arabinose might condense *in vivo* to form pyrrole and porphyrin derivatives which would be excreted. 2-amino glucose may be represented as a possible source of a simple pyrrolidine derivative, following a reaction similar to the internal condensation of glutamic acid to form the lactam, pyrrolidone carboxylic acid, which by reduction yields pyrrolidine carboxylic acid, or proline.



open-chain intermediate



proline

Animal experiments[1] have shown that, following administration of *d*-glucosamine hydrochloride, the amino sugar is collected to a relatively large extent by the liver and kidney, where it is slowly decomposed. Kawabe[1] found that incubation with liver tissue *in vitro* for 7 hours caused the decomposition of 28 per cent of added *d*-glucosamine hydrochloride.

Accordingly, a control experiment on myself with 20 gm. *d*-glucosamine hydrochloride having produced no ill-effects, 20, 30 and 40 gm. were given by mouth on three consecutive days, the hæmoglobin value being 90 per cent. No increase of uroporphyrin was observed either immediately or during the succeeding two months, but large quantities of a reddish-brown, green fluorescent pigment appeared in the urine. This pigment was soluble in ether and chloroform, exhibited urobilin-like absorption at 520 mμ, but after reduction with ferrous hydroxide or sodium amalgam failed to give a colour with Ehrlich's aldehyde reagent. It was therefore neither urobilin nor the porphobilin of Waldenström and Vahlquist[2].

That this pigment was not produced by acid treatment of *d*-glucosamine in the urine was proved by addition of amino sugar to normal and porphyric urines, which were then subjected to the same isolation procedure. Pigments so obtained showed no fluorescence.

Two months after the above experiment, the circulating hæmoglobin level had fallen to 50 per cent, with red blood cells numbering 2,070,000 and colour index 1·16. To improve this condition a concentrated liver extract containing iron was given by mouth over a period of seven days (30 ml. per day), when it had to be discontinued. The effect of this treatment was extremely adverse, as it was followed immediately by an attack of migraine, vomiting, sleeplessness and severe constipation lasting three weeks, during which the urinary uroporphyrin excretion reached a peak value of 26·7 mgm. per litre.

There was no evidence of increased blood destruction as indicated by urobilinogen output, and it was concluded that the raised uroporphyrin excretion was associated at that time with increased rate of erythropoiesis and porphyrin synthesis.

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[1] Kawabe, K., *J. Biochem.*, **20**, 243 (1934).

[2] Waldenström, J. and Vahlquist, B., *Z. Physiol. Chem.*, **260**, 189 (1939).

Bacteria Responsible for the Loosening of Wool on Sheepskins

THE sweating process of fellmongering is widely used in Australia, especially for treating merino sheepskins. It depends on the natural loosening of the wool that occurs when the skins are exposed to the air and kept moist for some days. The wool can then be easily removed by 'pulling'.

Investigations of the sweating process in this laboratory[1] have been greatly facilitated by the development of two new experimental methods:

(1) A physical method of following the progress of wool loosening. This involves measurement of the pull in grams weight to remove a staple of wool of which a 2·5 cm. length after scouring and drying weighs 1 mgm. The staples are pulled manually by means of a clip attached to a 250-gm. spring balance, and the average of six values determined for any particular sample of skin is referred to as the 'depilation load'. The depilation load must be practically zero before the wool can be pulled easily.

(2) A method of sterilizing sheepskin and removing the sterilizing agent to permit investigation of the wool-loosening activity of pure cultures of bacteria. The skin sample is immersed for 17 hr. in 0·05 *M* sodium metabisulphite adjusted to *p*H 2, the residual sulphurous acid and its salts are oxidized by immersion for 7 hr. in 0·3 *M* hydrogen peroxide, and the *p*H value at the skin surface is increased to approximately 8 by immersion for 17 hr. in 0·1 *M* sodium bicarbonate. No visible skin changes are produced in the course of this treatment and only a slight normal fall in depilation load was observed during subsequent storage for three days at 25° C. However, rapid wool-loosening occurred after inoculation of the skin with washings from a non-sterile skin or with some pure cultures of the bacteria listed below. This method was only adopted after it had been shown that the acetone-benzol drying and heating method of Chambard and Azémar[2], X-ray irradiation, and treatment with antiseptics such as chlorine, iodine and mercuric chloride were unsatisfactory owing to the production of undesirable changes in the skin tissues, tendering of the wool, or difficulty of removing the sterilizing agent.

By sterilizing sheepskin as described above and measuring the depilation load during incubation at 25° C. after inoculation with pure cultures of bacteria, it was shown that of the seventy-five species and strains of bacteria occurring on sheepskins, comprising forty-seven aerobes and twenty-eight anaerobes, only four were capable of reducing the depilation load to zero within 41 hr. So far as could be ascertained from Bergey's classification[3], they are: (1) An atypical strain of *Proteus vulgaris* (No. 7). (2) An unidentified species of Achromobacter (No. 42). (3) *Flavobacterium estero-aromaticum* (No. 52). (4) An unidentified species of Flavobacterium (No. 28). (The numbers in brackets refer to the system of classification used in this laboratory.)

Confirmation that only four out of the seventy-five species and strains isolated possess wool-loosening activity was obtained by inoculating sterile fœtal lambskin instead of sterilized adult sheepskin. The skin samples used were removed aseptically from almost full-term fœtuses, as described by Ellis[4].

Observations on the normal bacterial flora of wool roots during sweating have shown that *Proteus vulgaris* (No. 7) is probably the principal sweating bacterium, for although it occurs in relatively small numbers on the wool at the beginning of sweating, it is present in almost pure culture on the wool roots when wool loosening is complete. Sometimes the unidentified species of Achromobacter (No. 42) is also present at the completion of sweating, but then usually in equal numbers with No. 7. The flavobacteria occur rarely on wool roots during natural sweating.

Proteus vulgaris (No. 7) may be identical with *Bacillus pilline* isolated from sheepskins by Villon[5], and with the Streptococcus of Schmitz-Dumont[6], since its morphology varies under certain conditions. Similarly the bacilli "d" and "e" described by Wood[7], and "No. 1" and "No. 6" described by Chambard and Azémar[2], may be identical with No. 7 and No. 42 respectively isolated from Australian sheepskins. Owing to the incomplete descriptions, they cannot be identified with certainty.

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[1] To be described in detail in *Bull. Coun. Sci. Ind. Res.* (Aust.).

[2] Chambard, P., and Azémar, J., *J. Int. Soc. Leath. Trades Chem.*, **16**, 27 (1932).

[3] Bergey, D. H., "Manual of Determinative Bacteriology" (Baillière, Tindall and Cox, London, 1939).

[4] Ellis, W. J., *J. Coun. Sci. Ind. Res.* (Aust.), **16**, 173 (1943).

[5] Villon (1894), cited by Chambard and Azémar (see ref. 2).

[6] Schmitz-Dumont (1897), cited by Chambard and Azémar (see ref. 2).

[7] Wood, J. T., *J. Soc. Chem. Ind.*, **18**, 990 (1899).

Display and Bower-building in Bower Birds

THE recent note on this subject by Marshall[1] is of considerable interest in directing attention to the life-history of a remarkable group of birds. Of particular significance is the apparent correlation between the colours of the objects used by the male for decorating the bower and the more conspicuous colours of the female. If this correlation exists, it can have obvious epigamic value. Marshall studied especially the satin bower bird (*Ptilinorhynchus violaceus* (Viellot)), which prefers blue and green-yellow objects for decoration, and these colours can admittedly be correlated with the blue eyes of the female and the green-yellow of her plumage. But the spotted bower bird, a speckled brown bird with a bright mauve-red neck-tuft, shows preference for green and white objects, the white objects often consisting of bleached bones. Sometimes, with this species, green berries which have been placed in the bower turn red or yellow and are then promptly rejected. Yet another species, Newton's bower bird, with brown and yellow plumage, collects white flowers only. From the facts so far known, therefore, the correlation between choice of colours and plumage does not apparently extend to other Australian bower birds.

The oil-droplets in the cones of the avian retina form inter-occular filters the colours of which vary with the colour and proportion of the oily constituents, and hence there is reason for believing that the spectral sensitivity of birds will vary from species to species. In other words, we are led by physiological reasoning to suspect specificity in relation to colour-awareness in birds, and from this it is a temptation to assume that such specificity must necessarily bear a relation to the plumage colours of the species. So far, however, few convincing experimental findings have been produced to support this view, and the bower birds, representing as they do a *natural* experiment, will amply repay further close and accurate study, the results of which will far outweigh in value any artificial studies on domestic types like the hen and pigeon.

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[1] *Nature*, **153**, 685 (1944).

'Soil' Mechanics

HAVING read with interest the correspondence in recent issues of *Nature* on the suitability of the expression 'soil' mechanics, I am moved to express the hope that the writers will be more successful than I have been in securing the adoption of a better term.

I remember that the use of the expression 'soil' mechanics dates from an earlier time than that of the First International Congress at Harvard in 1935 (as stated by one correspondent) and that I then made strong representations to friends in the United States about its misleading nature. In reply, I was told that it was already too late to effect a change. Then came the Congress, which served to make the term more widely known and established. Thus it appears that, because somebody had mistranslated Terzaghi's original German expression, we were to be condemned for all time to suffer an inevitable muddle in nomenclature.

When, in 1940, I served as the geological member of the appropriate panel of the Committee on Earth Pressures of the Institution of Civil Engineers, I again protested vigorously against this use, or misuse, of the word 'soil', and suggested in its place 'earth', if the alternative and more precise expression 'unconsolidated rocks' (not 'unconsolidated deposits', which is too restricted in scope) were adjudged too cumbrous. But once more I failed, for 'soil' mechanics was said to be too firmly rooted in engineering literature to be eradicated.

Although we are not warranted in any circumstances in defending the slipshod use of words, the trouble in the present instance lies deeper than the simple question of nomenclature. In my experience, the result of the adoption of the expression 'soil' mechanics is that engineers and chemists who have occasion to consult the literature of 'soils' in this connexion are directed (by way of librarians or bibliographies) to pedological literature and not to the appropriate geological sources of information on unconsolidated rocks—sometimes with unfortunate results.

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REACTION OF HUMAN SERUM ALBUMIN WITH HÆMATIN AND HÆM

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IT was shown by Heilmeyer[1] that the pigment in the blood serum of certain cases of pernicious anæmia accompanied by a severe "hæmatin jaundice" or "hæmatinæmia" differs from free alkaline hæmatin in that the absorption band is at about 620 mμ instead of at 610 mμ. As this pigment was also obtained by Heilmeyer[2] *in vitro* by adding hæmatin to blood serum, he concluded that "in serum the hæmatin combines with proteins which are only split off on adding an excess of concentrated alkali. Thus in serum we are concerned with the spectrum of a protein-hæmatin and not that of alkaline hæmatin".

The same pigment was discovered independently by Fairley and Bromfield[3] in the plasma of patients suffering from blackwater fever, which is one of the most dreaded complications of malaria. They described this pigment under the name of pseudomethæmoglobin, showing that it differs from ordinary methæmoglobin (1) in having its absorption band at about 623 mμ instead of at 630 mμ; (2) in not being easily reduced by Stoke's solution; and (3) in not reacting with hydrogen sulphide, sodium azide, sodium fluoride or hydrogen peroxide. Fairley and Bromfield have also shown that it is formed when oxyhæmoglobin is incubated at 40° C. for 24–72 hours with human plasma. In 1938, Fairley[4] obtained this compound by adding alkaline hæmatin to human or simian blood sera; he failed, however, to obtain it with the sera of other animals. Since of all the protein fractions of human serum only albumin would give this compound, Fairley changed its name from pseudomethæmoglobin to methæmalbumin. On reduction with sodium hyposulphite a compound was formed which Fairley[4,5] described as hæmalbumin, characterized by two diffuse absorption bands at 573 mμ and 530 mμ which after standing a few minutes were replaced by one very diffuse band at about 574 mμ.

In addition to cases of blackwater fever, where the presence of methæmalbumin was confirmed by Foy and Kondi[6], this pigment was found by Fairley and Bromfield in cases of nocturnal hæmoglobinuria, incompatible transfusion and pancreatic cyst fluid (see Fairley[4]). It is interesting to note that in blackwater fever it is found in the plasma, where it is accompanied by oxyhæmoglobin, but not in the urine, which contains oxy- and methæmoglobin.

The main object of the present investigation was to determine the nature of the compound described by Heilmeyer and Fairley. For reasons which will be given later on, the ferric and ferrous forms of this compound will be referred to as hæmatin-albumin and hæm-albumin respectively.

Preparation of Hæmatin-albumin

Hæmatin-albumin was prepared as follows: 20 mgm. hæmin are dissolved in 2 ml. 0·1 *N* caustic soda and made up to 25 ml. with water. To 1 ml. of this solution are added 2 ml. of human serum, human plasma or the albumin fraction prepared from this plasma. The absorption spectrum of the compound thus obtained, together with those of alkaline hæmatin and acid and alkaline methæmoglobin, were determined with the Hilger-Nutting spectrophotometer. Fig. 1 shows that the absorption spectrum of alkaline hæmatin in human serum bears a much greater resemblance to that of free alkaline hæmatin than to those of methæmoglobin, especially as it should be compared not with acid but with alkaline methæmoglobin. As albumin seems to be the only serum protein which gives this compound, hæmatin-albumin is a more appropriate name for it. Moreover, as Fairley and Bromfield[3] have already shown, this compound, unlike methæmoglobin, does not combine with hydrogen sulphide, sodium azide, sodium fluoride or hydrogen peroxide. The absorption spectrum of hæmatin-albumin also differs greatly from that of a parahæmatin, which can be obtained by treating hæmatin with denatured serum proteins.

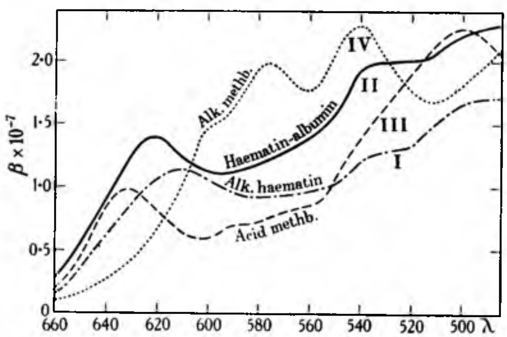

Fig. 1. Absorption spectra of: alkaline hæmatin (I); hæmatin albumin (II); acid methæmoglobin (III); and alkaline methæmoglobin (IV). In all figures the ordinate represents the absorption coefficient $\beta = \frac{1}{cd} \ln \frac{I_0}{I}$, where c is concentration of hæmatin in gram-atom of iron per ml.; d is length of tube in cm.; I_0 and I are intensities of the incident and transmitted light respectively. The abscissa represents wave-length in mμ.

Hæm-albumin. When hæmatin-albumin is reduced with sodium hyposulphite, the colour of the solution turns from reddish brown to a deep red and its absorption band at 623 mμ is replaced by two bands, α at 570 mμ and β at 540 mμ, the α-band always being much stronger than the β-band. This compound can also be obtained by the addition to hæm of human serum albumin, but not by the addition of any other serum protein fraction. The absorption bands of hæm-albumin, as shown in Fig. 2, are much stronger than those of the free hæm, but much more diffuse and lying nearer the red end of the spectrum than the sharp bands of hæmochromogens obtained from hæm and denatured serum proteins. When human plasma or serum is added to hæm, the first change which is usually observed is the appearance of a very small amount of a hæmochromogen with its characteristic bands; these, however, soon disappear and are replaced by the bands of hæm-albumin. The very small concentration of hæmochromogen which occasionally persists obliterates with its α band the clear space separating the α and β absorption bands of hæm-albumin. It is difficult to say whether this hæmochromogen is formed by the native serum albumin itself, as a transient intermediary stage leading to the formation of hæm-albumin, or by traces of a denatured protein present in the solution.

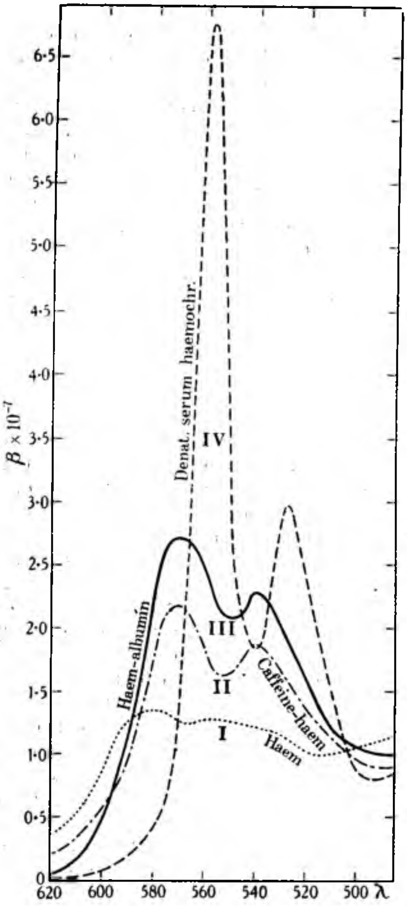

Fig. 2. Absorption spectra of: hæm (I); caffeine hæm (II); hæm-albumin (III); and denatured serum hæmochromogen (IV); β as in Fig. 1; the heights of II and III vary to a certain degree according to preparation.

Nature of Hæm-albumin

The only hæm derivative which bears a strong resemblance to hæm-albumin is caffeine-hæm, which has recently been described (J. Keilin[7]). Both compounds are of the same colour, and the similarity of their absorption spectra is clearly shown in Fig. 2. In the case of caffeine-hæm, it has been shown[7] that caffeine is linked not to the iron but to the porphyrin alone of the hæm molecule, which suggests that in hæm-albumin the protein is also combined with the porphyrin alone. This is strongly supported by the facts that caffeine[7], native globin (Hill and Holden[8]) and "serum proteins" (Haurowitz and Waelsch[9]) form with the free alkaline porphyrins characteristic and spectroscopically recognizable compounds, while among the serum proteins only albumin will react in this way. Finally, by diffusion in gelatine gel and by cataphoresis methods, Gildemeister[10] was able to show that the endogenous coproporphyrins I and III are strongly bound only to the albumin fraction of serum.

Reaction of Hæm-albumin with Carbon Monoxide

On passing carbon monoxide through a solution of hæm-albumin, its absorption spectrum changes to that of carbon monoxide hæmochromogen, which differs from that of carbon monoxide hæm or carbon monoxide caffeine-hæm in the position and relative height of the two absorption bands (Fig. 3). In hæm-albumin treated with carbon monoxide, the protein therefore appears to combine with the iron of hæm.

It is not surprising that hæm may fail to give a hæmochromogen and yet may form a carbon monoxide hæmochromogen under the same conditions, since it is known that the iron of carbon monoxide hæm has a much greater affinity for nitrogenous compounds than the iron of free hæm. It is, however, impossible to say whether the protein component of this carbon monoxide hæmochromogen is the native serum albumin or a denatured protein present in very small concentration.

It can be said in conclusion that proteins may form with hæm (Fe^{++}) three types of compounds: (1) Hæmochromogens: usually given by a denatured protein which is linked with the iron of hæm. (2) Hæm-albumin: given by a native protein linked only to the porphyrin of hæm and forming a compound analogous to caffeine-hæm. (3) Hæmoglobins: where the protein is combined with both the porphyrin and the iron of hæm.

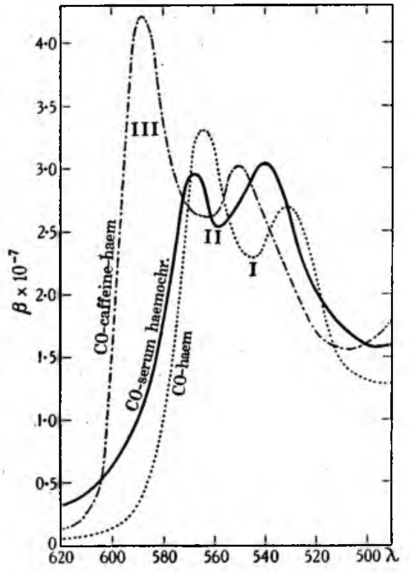

Fig. 3. Absorption spectra of: carbon monoxide hæm (I); carbon monoxide serum-hæmochromogen (II); carbon monoxide caffeine-hæm (III); β as in Fig. 1; the heights of I and III vary according to their dispersion, but the form remains the same.

I wish to thank Dr. E. F. Hartree for the curves of acid and alkaline methæmoglobins and for the preparation of a pure sample of human serum albumin.

[1] Heilmeyer, L., *Deutsch. Arch. Klin. Med.*, **173**, 128 (1932).

[2] Heilmeyer, L., "Medizinische Spectrophotometrie" (Fischer, Jena, 1933): English translation: "Spectrophotometry in Medicine" (A. Hilger, 1943).

[3] Fairley, N. H., and Bromfield, R. J., *Trans. Roy. Soc. Trop. Med. and Hyg.*, **28**, 307 (1934); **31**, 139 (1937); **31**, 372 (1938).

[4] Fairley, N. H., *Nature*, **139**, 588 (1937) and **142**, 1156 (1938).

[5] Fairley, N. H., *Brit. J. Exp. Path.*, **21**, 231 (1940).

[6] Foy, H., and Kondi, A., *Trans. Roy. Soc. Trop. Med. and Hyg.*, **32**, 49 (1938).

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[8] Hill, R., and Holden, H. F., *Biochem. J.*, **20**, 1326 (1926).

[9] Haurowitz, F., and Waelsch, H., *Hoppe-Seyl. Z.*, **182**, 82 (1929).

[10] Gildemeister, H., *Z. Exp. Med.*, **102**, 58 (1937).

COLLAPSE OF DETERMINISM

AT the thirty-first meeting of the Indian Science Congress, which met in Delhi in January 1944, the Congress president, Prof. S. N. Bose, discussed "The Classical Determinism and the Quantum Theory", showing how "physicists have gained knowledge but lost their faith". This was supplemented in the Section of Mathematics and Statistics by B. M. Sen, president of that Section, in his address entitled "The Fundamental Equations of Quantum Mechanics". It is difficult to do justice to this without mathematical symbols, so the following article will be based mainly on Prof. Bose's more general treatment.

Classical physics may be said to have begun with Newton. His laws of motion and theory of gravitation gave an explanation of planetary motion which was so satisfactory that it seemed to provide an infallible means of predicting the motion of the solar system. Laplace went so far as to assert that if a sufficiently vast intellect knew the mass, position and velocity of every particle of the universe at any one instant, and the forces acting on them, then "nothing would be uncertain for him; the future as well as the past would be present to his eyes".

It was difficult to fit the phenomena of light into this scheme, since the discovery of interference had shown that light moves like waves rather than like particles. But Maxwell overcame this difficulty by developing Faraday's ideas about the ether. Newton's absolute space was no longer to be regarded as empty, but as a medium possessing energy and momentum, capable of being strained and transmitting waves. This theory was later extended to explain also X-rays, radio signals, and the γ-rays emitted by radium. Maxwell's electromagnetic theory was extended by Lorentz into a theory of electrons. The modifications in the Newtonian scheme had left it stronger than before. Almost everywhere was seen the reign of exact laws. It was true that in thermodynamics physicists had to make shift with laws that applied only to large aggregates, but no one doubted that these were derivable by averaging from exact equations which were too numerous to be conveniently considered individually. The derivation of these averages contained the term probability or chance, but it was always pointed out that really there was no such thing, and that every occurrence could be predicted if all the circumstances were known. It was characteristic of the eighteenth century, the 'age of reason', that Voltaire should assert that "Chance is a word void of sense: nothing can exist without a cause", and Linnæus that "Nature does not proceed by jumps".

These beliefs were rudely shaken by the quantum theory. In 1900 Planck, puzzled by the phenomena of heat radiation, made the revolutionary suggestion that the emission of energy is discontinuous. Soon afterwards Einstein made a similar suggestion about the photo-electric effect. Bohr explained the spectra of hydrogen and other atoms by postulating that an electron must pass discontinuously from one orbit to another. These postulates were empirical, put forward so as to lead up to the results of experiment. They succeeded in this, but at the cost of abandoning the established laws of mechanics and electrodynamics. What seemed a greater break with tradition was called for by the theory of relativity, but in fact this theory does not reject causality. It is true that it denies the existence of absolute time and absolute space, but it sets up a new set of absolute laws in space-time which are independent of all axes of reference. Einstein's success in explaining the slight discrepancy in the motion of the planet Mercury, and in predicting the apparent displacement of stars during a solar eclipse, struck the imagination of the public, but really it is quantum theory that clashes fundamentally with classical physics.

At one time there were hopes of a reconciliation through Schrödinger's wave mechanics, but it turned out that the waves in question are only mathematical fictions. They are excellent as a means of calculation, as they can be treated by the familiar methods of differential equations instead of by the unfamiliar matrices of Heisenberg or Dirac's algebra of observables. However, all three methods are equivalent, and all lead to the same startling conclusion, Heisenberg's Principle of Uncertainty. This shows that if we measure the position and momentum of a stream of electrons, the more accurately we determine the position the less accurately can we determine the momentum, and conversely. Some think that this merely expresses the obvious truth that every experiment interferes to some extent with the phenomena we attempt to measure. Others go further, and interpret the principle to mean that we cannot predict the motion of a single particle exactly, owing to the slight variation of the forces on it caused by atomic structure, although we can obtain statistical laws which hold for the average motion of a large number of particles. But a third interpretation goes so far as to claim that the existence of causality is disproved. Von Neumann claims to have demonstrated that the results of the quantum theory cannot be obtained by averaging any exact causal laws.

Some philosopher-physicists welcome these conclusions, as giving us a hope of escape from the tyranny of an iron law of causation, and assuring freewill to mankind as well as to electrons! However, the majority of physicists regard causality as essential to science, and are hoping to establish a unified theory that will once again reunite all our knowledge into exact causal laws. While the matter is still unsettled, we should do well not to insist on imposing our preconceived ideas upon Nature, but let them evolve and adjust themselves to our growing, if somewhat unmanageable, knowledge of reality.

H. T. H. PIAGGIO.

THYROXINE: ITS BIOSYNTHESIS AND ITS IMMUNO-CHEMISTRY*

THE suggestion that thyroxine might be formed in Nature from tyrosine through the stage of diiodotyrosine was made at an early stage of the elucidation of the chemistry of thyroxine, and was made more probable when the constitution of the latter was finally determined. Over a number of years several pieces of evidence, all indirect in character, were brought forward in support of this biogenetic hypothesis, which thus came to be generally accepted.

Recently two lines of direct evidence have become available which seem to place the matter beyond doubt. In the first place, the transformation of diiodotyrosine into thyroxine has been effected by purely chemical methods of a character which make

* Abstract of the Croonian Lecture of the Royal Society delivered by Dr. C. R. Harington, F.R.S., on July 13.

it possible to formulate a theory of the chemistry of the process involved. Secondly, by the application of modern biochemical technique, the actual synthesis of thyroxine from diiodotyrosine has been demonstrated in surviving thyroid tissue *in vitro*. The latter type of experiment incidentally offers an opportunity for the analytical study of the action of substances such as thiourea, which inhibit thyroid activity supposedly by interfering with the biosynthesis of the hormone.

Accepting the mechanism of biosynthesis of thyroxine as being satisfactorily established, we are left with two outstanding problems. Is thyroxine itself the actual circulating thyroid hormone, and, if so, by what mechanism does it exercise its effect in the periphery ? To the second of these questions no answer can yet be given. Evidence regarding the first is conflicting, and in the attempt to obtain a definitive answer an approach has been made along a new line which raises matters of some general interest. The method is based on the theory, deduced from the known facts of immunological chemistry, that an antigen of which the determinant group is a physiologically active substance should give rise to an antiserum capable of inhibiting the characteristic activity of this substance. Application of this idea to the problem of thyroxine involved the development of a new technique for building up artificial antigenic complexes. Such a complex containing thyroxine as the determinant group has proved to be able to give rise to an antiserum which can inhibit the physiological action both of a protein containing thyroxine, such as thyroglobulin, and of thyroxine itself. The latter observation, together with extension of the experimental method to an entirely different compound, favours the hypothesis that thyroxine itself is in fact the actual circulating thyroid hormone.

INCIDENCE OF RICKETS IN GREAT BRITAIN

DURING the six weeks from mid-January to the end of February 1943 the British Pædiatric Association carried out a combined clinical and radiological investigation into the frequency of rickets in twenty-three areas of Britain and Ireland*. Out of a total of 5,283 children aged 3–18 months, 106 only were reported to show radiological evidence of rickets. Dr. Percy Stocks, who analysed the returns, concluded that the incidence of rickets, diagnosed radiologically, was 2·5 per cent before six months, 4 per cent during the first year and negligible after the first year. The incidence was highest in Ireland and lowest in South England. In Watford and St. Albans no case was detected even by clinical methods. There is no evidence of any increase in the incidence or severity of rickets during the War. In some of the cities of north England, older children with deformities due to severe rickets may still be seen, whereas the cases found in this investigation were slight and free from any gross deformity. It may be inferred that severe rickets was more common a few years ago.

Two points stand out. First, the number of cases diagnosed by clinical methods was nearly ten times the number diagnosed radiologically. Even the three radiologists who examined the films differed in their interpretations. Obviously, we do not yet know what constitutes evidence of slight rickets. But this disagreement is itself evidence of the mildness of the disease, for there is no mistaking severe rickets.

Secondly, 85·5 per cent of the babies that were considered to have rickets on X-ray evidence had had some form of treatment with vitamin D. On this point the report is disappointing. Examiners were supposed to find out what preparation was used, when dosage began, and the daily dose given ; but analysis in terms of the duration of vitamin D prophylaxis and the dosage was not attempted, and the nature of the preparations used is not mentioned in the report. It is little use saying that "scientific evidence has clearly shown that adequate vitamin D administered in an adequate dosage and in a suitable form will prevent rickets", and that "a common cause of rickets is the popular cod liver oil-and-malt", without inquiry into the reasons why 77 out of 4,317 babies who were given vitamin D got rickets. The fashion of handing over the results of an investigation for mechanical analysis by a statistician who may not appreciate the importance of the problems that may be involved has its disadvantages.

* Reports on Pub. Health and Medical Subjects, No. 92. "The Incidence of Rickets in War-time". Pp. 36. (London : H.M. Stationery Office.) 9*d*. net.

MODERN ASPECTS OF INORGANIC CHEMISTRY

THE presidential address by Prof. R. C. Ray to the Chemistry Section of the Indian Science Congress at Delhi dealt with some aspects of modern inorganic chemistry. After mentioning that research in inorganic chemistry had declined towards the close of the nineteenth century, mainly because of the very rapid development of organic chemistry and the rise of physical chemistry, he pointed out that there are very many new aspects of the subject now being developed.

Prof. Ray went on to describe some interesting developments. Compounds of the inert gases with water, boron fluoride and phenol have been obtained, and compounds with metals such as mercury, and iodine, sulphur and phosphorus are described. A subject which has been fully studied, particularly by Stock, Wiberg, Bauer and others, is the chemistry of the hydrides of boron and related compounds, which are also of great interest in relation to the electronic theory of valency. In this field, Indian workers have made important contributions, particularly Prof. Ray and his pupils, who have also worked on the chemistry of co-ordination compounds, glass and hydrides of metals such as nickel.

Nickel forms two hydrides, NiH and NiH_2, and cobalt forms analogous compounds, the heats of formation being comparable with those of the salt-like hydrides of the alkali and alkaline earth metals. The heats of formation of hydrides of some rare-earth metals, zirconium, tantalum and titanium, generally regarded as interstitial compounds, are also of the same order, this suggesting that there can be little difference in the nature of the chemical bond in such substances as zirconium hydride and barium hydride, with nearly equal heats of formation.

Prof. Ray considers that there has probably never been a time when the prospects of inorganic chemistry were so promising as they are to-day, when new methods in physics, physical chemistry and organic chemistry are available.

EXCAVATIONS AT TRES ZAPOTES, MEXICO*

AN account has now been issued of the first season's work of the joint expedition of the National Geographic Society and the Smithsonian Institution to Tres Zapotes. The site is a large one, and the remains consist of earth mounds with little or no masonry. The deposits are tentatively assigned to three periods, Middle Tres Zapotes *A* and *B* and Upper Tres Zapotes. This nomenclature depends on the existence of a Lower Tres Zapotes horizon, discovered during the work of the second season.

The Middle *A* and *B* deposits were not found in superposition, but there is little doubt about their relative age. On one site, Upper Tres Zapotes was found overlying Middle *A*, but separated from it by 6 ft. of sterile deposit.

Middle Tres Zapotes *A* is associated with large numbers of hand-made, solid clay figurines of archaic type, and although there is always a possibility that they are a late survival in a marginal area, the balance of the evidence favours an early date. The Middle *B* period contains uncremated burials in large ollas. It is marked by the first appearance of painted pottery, and there are contacts with many other cultures, conspicuous among which is the Maya Old Empire. The Upper period contains cremated burials in covered bowls, and the pottery is of great complexity. A curious feature of the site is the paucity of Aztec remains.

The work described was admittedly exploratory and many problems are left unsolved. Some of the results of the work of the second season were already available when it went to press, so it seems a pity that publication was not delayed until they could be incorporated and a fuller picture given. The illustrations are good and clear, but should as a matter of routine have been provided with a scale in every case. The same criticism applies in a greater degree to maps 2 and 3, and to the section which is, rather curiously, designated "Map 6".

G. H. S. BUSHNELL.

* Smithsonian Institution: Bureau of American Ethnology. Bull. 139. An Introduction to the Ceramics of Tres Zapotes, Veracruz, Mexico. By C. W. Weiant. Pp. xiv+144+78 plates. (Washington: Government Printing Office, 1943.) 40 cents.

FORTHCOMING EVENTS

Tuesday, July 25

QUEKETT MICROSCOPICAL CLUB (at the Royal Society, Burlington House, Piccadilly, London, W.1), at 7 p.m.—Exhibition of Specimens and Discussion.

APPOINTMENTS VACANT

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

LECTURER IN EXPERIMENTAL PHYSIOLOGY—The Registrar, The University, Sheffield (July 28).

LECTURER IN THE ELECTRICAL ENGINEERING DEPARTMENT—The Registrar, Technical College, Sunderland (July 29).

JOHN RANKIN CHAIR OF GEOGRAPHY—The Registrar, The University, Liverpool (July 31).

W. H. COLLINS PROFESSORSHIP OF HUMAN AND COMPARATIVE PATHOLOGY—The Secretary, Royal College of Surgeons of England, Lincoln's Inn Fields, London, W.C.2 (July 31).

SENIOR LECTURESHIP IN THE DEPARTMENT OF METALLURGY of the University of the Witwatersrand—Dr. W. Cullen, 4 Broad Street Place, London, E.C.2 (July 31).

PRINCIPAL OF THE MID-ESSEX TECHNICAL COLLEGE AND SCHOOL OF ART, Chelmsford—The Chief Education Officer, County Offices, Chelmsford (August 5).

ASSISTANT LECTURER IN METALLURGY—The Acting Registrar, The University, Leeds 2 (August 12).

ASSISTANT PHYSICIST to the Sheffield Radium Centre—The Secretary, Sheffield Radium Centre, Royal Infirmary, Sheffield 6 (August 12).

BIOLOGIST at the West Midland Forensic Science Laboratory at Birmingham—The Establishment Officer, Room 320, Home Office, Whitehall, London, S.W.1 (August 12).

LECTURER IN PHILOSOPHY—The Very Rev. the Dean, Christ Church, Oxford (October 15).

LECTURER IN PHYSIOLOGY—The Principal, Nonington College of Physical Education, Bromsgrove.

ASSISTANT PHYSICIST (with experience of the Physics of Radiotherapy)—Mr. S. Clayton Fryers, General Infirmary, Leeds 1.

SCIENTIFIC ASSISTANT (Science Degree) and TECHNICAL ASSISTANT (Arts Degree)—Imperial Bureau of Animal Health, Veterinary Laboratory, Ministry of Agriculture, New Haw, Weybridge, Surrey.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Department of Scientific and Industrial Research. Index to the Literature of Food Investigation. Vol. 15, No. 1, June 1943. Compiled by Agnes Elisabeth Glennie, assisted by Janet Lang Hall Keuneman. Pp. iv+86. (London: H.M. Stationery Office.) 4*s*. 6*d*. net. [266

Fitzwilliam Museum, Cambridge. Annual Report for the Year ending 31 December 1943. Pp. 10. Friends of the Fitzwilliam Museum. Thirty-fifth Annual Report for the Year 1943. Pp. 4. (Cambridge: Fitzwilliam Museum.) [266

British Society of Animal Production. Report of Inaugural Meeting, 6th January 1944. General Topic: Cattle Breeding Policies. Pp. 38. (Edinburgh: Secretary-Treasurer, British Society of Animal Production, c/o Imperial Bureau of Animal Breeding and Genetics.) 2*s*. [266

National Physical Laboratory. The Natural Lighting of Houses and Flats with Graded Daylight Factor Tables. By T. Smith and Miss E. D. Brown. Pp. 22. (London: H.M. Stationery Office.) 4*d*. net. [266

Burton-on-Trent Natural History and Archæological Society. Local Records for 1943. Edited by H. J. Wain. Pp. 24. (Burton-on-Trent: The Museum.) 1*s*. [276

Medical Research Council. Emergency Report No. 5 of the Industrial Health Research Board: A Study of Variations in Output. By S. Wyatt, assisted by R. Marriott, W. M. Dawson, D. E. R. Hughes and F. G. L. Stock. Pp. iv+16. (London: H.M. Stationery Office.) 4*d*. net. [276

Lister Institute of Preventive Medicine. Report of the Governing Body, 1944. Pp. 16. (London: Lister Institute.) [276

Nuffield College. Problems of Scientific and Industrial Research: a Statement. Pp. 64. (London: Oxford University Press.) 2*s*. net. [276

A Racial Survey of the British People. Lecture delivered to the Free German Institute of Science and Learning, London, on March 11, 1944. By Dr. R. E. G. Armattoe. Pp. 8. (Londonderry: The Author, 7 Northland Road.) 1*s*. 6*d*. [47

Combine Harvesting in the North, 1943. (N.I.A.E. Publication No. 502/44.) Pp. 20. (York: National Institute of Agricultural Engineering.) 6*d*. [47

Notes on Drying Paints by Radiant Heat (Infra-Red). Pp. 20. (Slough: Imperial Chemical Industries, Ltd.) [47

Other Countries

Bulletin of the American Museum of Natural History. Vol. 82, Art. 8: Geographic Variation in *Rana pipiens* Schreber of Eastern North America. By John Alexander Moore. Pp. 345–370+plates 61–66. (New York: American Museum of Natural History.) [306

Indian Forest Leaflet No. 57: Furnace Heated Veneer Drying Kiln suitable for the Seasoning of Veneers for the Manufacture of Plywood. By M. A. Rehman. Pp. ii+6. 6 annas; 7*d*. Indian Forest Leaflet No. 60: A Short Note on the Beedi Leaf Industry. By Jagdamba Prasad. Pp. iii+12+2 plates. 6 annas; 7*d*. Indian Forest Leaflet No. 61: Kiln Drying Schedule for Seasoning of Veneers. By M. A. Rehman and S. M. Ishaq. Pp. ii+7. 6 annas; 7*d*. Indian Forest Leaflet No. 64: The Growing of *Cryptostegia grandiflora* as a War Time Emergency Plantation Crop. By A. L. Griffith. Pp. iii+10. 4 annas; 5*d*. (Dehra Dun: Forest Research Institute.) [47

Indian Forest Bulletin No. 121: Tests on the Suitability of Indian Woods for the Manufacture of Textile and Jute Mill Accessories, Part 1: Substitutes for Persimmon and Cornel for Cotton Mill Shuttles. By M. A. Rehman and Chheda Lal. Pp. 9. 4 annas; 5*d*. Indian Forest Bulletin No. 122: Tests on the Suitability of Indian Woods for the Manufacture of Textile and Jute Mill Accessories, Part 2: Care and Seasoning of Wood for Bobbins, Picker Arms, and Jute Mill Rollers. By M. A. Rehman. Pp. 7. 4 annas; 5*d*. (Dehra Dun: Forest Research Institute.) [47

Indian Central Cotton Committee: Technological Laboratory. Technological Bulletin, Series A, No. 57: Technological Reports on Trade Varieties of Indian Cottons, 1943. By Dr. Nazir Ahmad. Pp. ix+84. 1.8 rupees. Technological Bulletin, Series A, No. 58: Technological Reports on Standard Indian Cottons, 1943. By Dr. Nazir Ahmad. Pp. iv+103. 1.8 rupees. Technological Bulletin, Series A, No. 59: Spinning Tests on Mixtures of Staple Fibre with Indian Cottons. By Srinagabhusahana and Dr. Nazir Ahmad. Pp. 27. 12 annas. (Bombay: Indian Central Cotton Committee.) [47

Indian Forest Records (New Series). Silviculture, Vol. 4A, No. 4: Yield and Stand Tables for Sal (*Shorea robusta*, Gaertn.f.) High Forest. By A. L. Griffith and Bakhshi Sant Ram. Pp. ix+171–287+4 plates. (Delhi: Forest Research Institute.) 2.14 rupees; 4*s*. 4*d*. [47

U.S. Department of Agriculture. Technical Bulletin No. 869: Strains of the European Corn Borer in the United States. By K. D. Arbuthnot. Pp. 20. (Washington, D.C.: Government Printing Office.) 10 cents. [47