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Science and Peace

IN the remarkable differences between the world to-day and that of a century ago, the good changes and the evil are in the last resort due mainly to the use or misuse of scientific discovery. Men of science are, of course, not the sole agents in the transformation ; their work is applied by others, under the direction of industrialists and Governments, for private gain or for the public good as judged by statesmen and administrators.

Until lately, the average man of science was scarcely more interested in the uses to which his work was put than is the ostrich in the fate of the eggs that she lays. There will always be scientific workers whose interests seem far removed from ordinary human affairs ; but the activities of an increasing proportion of men of science are closely linked with social purposes. Many of these can work with good heart in full confidence that the fruits of their labours increase the health and happiness of mankind ; but there is a darker side to the picture, since through science the horrors and waste of war have been greatly intensified. The result is that some people think that the only way to save civilization is to suppress further advances in scientific knowledge.

Responsibility for the deplorable growth of destructive forces cannot, however, be accepted by men of science except as citizens. Throughout recorded history there has been war, and the captains and the kings have always sought to increase their military strength by the inventions and the science of their time. The change in our day is not fundamental, but lies only in the much greater effectiveness with which the old purposes are pursued, through the application of modern scientific method and resources. From the military point of view, organized scientific effort meets with the same astonishing success as when applied to peaceful ends ; but the result for mankind is to extend enormously the burden of fear and the potential human suffering through war. Indeed, ever since the Great War, this suffering has been not only potential but actual, as recently in Abyssinia and now in Spain.

The Founder of Christianity asked from the Cross forgiveness for those who set Him there, "for they know not what they do". The cross of war laid upon mankind is due, like that other Cross in Jerusalem, to the moral and intellectual frailty of man—to the craft of a few who are deeply evil ; to the folly and weakness of others among the great, who are not lacking in good-will ; to the passions and tumult of the multitude. Perhaps the share of science in this modern crucifixion may be compared with the work of the soldiers who then drove in the nails and raised high the crosses.

Were these soldiers worse than other soldiers ? Was not the Government and nation that made them soldiers, and assigned their tasks to them, more responsible than they for that evil ? In like manner, are those scientific workers who are directly working for military ends to be regarded as worse than other men of science ? Surely the chief responsibility lies with the community at large ; and in proportion to his single-minded simplicity, the militarized man of science, like the common soldier, is to be honoured for the faithful following of his vocation.

Many minds, however, cannot find rest in simple obedience to authority, in simple national patriotism. The principle of the settlement of disputes by war is increasingly questioned by the mind and conscience of man, and not least among men of

science. Many fair-seeming bubbles have been burst, and recognized for the hollow, puffed-out things that they are: the famous Oxford resolution on fighting for king and country was symptomatic of this. Of course, if Britain should become involved in war, the youth of Oxford would fight; but among them and the rest of the soldiers there would be a larger proportion than in any previous war, who from the outset realized the futility, the colossal folly of war. Even in Germany, with all its unchallenged internal propaganda, it is not too easy to resurrect the old 'Heldentum' nonsense, which many German ex-soldiers hoped had been cast into limbo for ever.

Already in the time of the Great War the prostitution of science to warfare was deeply felt by many British men of science, some of whom refused altogether to participate either as soldiers or as scientific workers. The same attitude would again be taken by many, should we be cursed by another war; even in the present time of uneasy peace, the activity in rearmament is renewing in the minds of many men of science the same tension of feeling, the same problems of conscience. Such scruples, sincerely held, claim at least the same measure of public consideration and respect as they received in Britain during the Great War. For such men there is often no choice whether they shall or shall not refuse war-service, scientific or otherwise; like Luther on a famous occasion, "they can no other".

Most men of science will know outstanding examples, among their colleagues, of men who so refused in the Great War, and, by our national wisdom, were spared the penalties that in other countries might have led to their death, by direct act or by obloquy and torment leading to suicide. Many such men have, in the years since then, rendered distinguished service to the community; their work, and that of many lesser men who followed the same course, has enriched the nation, and been rendered with loyal recognition of the toleration extended to them by their fellow-citizens during a time of great national stress.

To the main body of men of science, however, military preparations, and scientific co-operation in them, seem in the existing state of the world a necessary, though regrettable, measure. It is important that there should be mutual respect and understanding between this majority, and their colleagues who are extreme pacifists—the larger group standing by the minority, to protect them against victimization, and the minority co-operating wherever possible with their fellows, and respecting in them their loyalty to their duty as they perceive it.

Should this duty end, however, with the performance of their special technical functions? Does there not rest upon men of science as a body a responsibility for the promotion of peace, and peaceful methods of international adjustment, beyond that which already attaches to them as citizens? Their special outlook, education, training and gifts seem to involve this.

In the first place, scientific workers are better able than most men to reflect with knowledge upon the evolution of man, and to realize what immense conquests have already been made over ignorance, weakness and evil. They can in some measure review the progress of man in social organization, and envisage great improvements still to come, realizing that even to-day unity in government may be maintained, as in Canada, despite enduring differences of race, religion, language and traditions among the people. They know that wider loyalties can develop and become paramount over a hierarchy of narrower loyalties. They have, therefore, ground for good hope, since so much has been achieved by steps largely unconscious or groping, that far more can yet be done by conscious and well-chosen effort.

Secondly, they have long been accustomed to international co-operation, without which the state of science itself would still be relatively primitive. They can, therefore, well judge how fruitful would be the adoption of a similar policy in the social and economic sphere, even if military budgets continued as now; but such co-operation would also remove many of the chief causes of international mistrust, and enable much of these budgets to be diverted to happier uses.

Furthermore, their whole professional activity contrasts strongly both in method and success with the military method of settling international problems. The military method is inherently ill-designed to yield good results. Men of science know that the judgment is warped by bitter passions such as prevail in war, and that difficult problems are best attacked by earnest, dispassionate search after understanding and truth, with prolonged patience and resource; they have therefore a special duty to urge the application of these methods to international problems. To attempt to solve the problems by military force, or the threat of it, is to hazard our whole well-being, with no certainty that the better cause will

prevail or the best solution be arrived at: on the contrary, with the certainty that whatever side gains the victory, those who bear the suffering—nowadays the majority of the people—will never reap a reward that will compensate them for their travail and sorrow.

Medical men form a group among scientific workers who have particular reason to hate war and to seek after peace: the wholesale maiming, poisoning and starvation involved in war is in extremest contrast with their lifework for the maintenance of health and the alleviation of human suffering. A campaign to arouse the interest of the medical profession in the promotion of peace has lately been launched, with much devoted work and considerable success: similar efforts have been in progress at Cambridge, Oxford and elsewhere, among scientific workers of other kinds. During the recent National Peace Congress in London, among the several Commissions which considered special aspects of the peace question was one on science and medicine, reference to the proceedings of which appears elsewhere in this issue (p. 993). It is hoped that, as a result of the meetings of this Commission, local groups of scientific workers will be formed in many other places, for the promotion of peace by study, research and propaganda.

We do not overlook the fact that urgent reasons can be given in favour of the present British rearmament, or that there is little likelihood that our vast military preparations will be devoted to selfish aggression. But it cannot be denied that a Europe armed to the teeth will be an unstable Europe, living under the shadow of fear that some spark may produce a disastrous conflagration. What we suggest is that some part of this great effort and expenditure on rearmament would be more fruitful for the peace of Europe and the world, if employed in other ways. The greater part of that expenditure might, indeed, be saved by a more active policy of international co-operation and appeasement.

The main argument against such proposals is that our very safety is threatened by other well-armed nations, so that rearmament is essential for the defence of our lives and ideals. We know ourselves that our intentions are wholly non-aggressive, but can we expect the rest of Europe to welcome a *pax britannica*, imposed on 'the lesser breeds' by means of our great national wealth? Surely the only kind of peace likely to endure in Europe is one that is corporately maintained by the community of European nations, whose instrument of co-operation is ready at hand in the League of Nations. The best means to achieve this cannot now be discussed here, but a determined effort, and indeed lead, in this direction seems incumbent on the British Government to assist movements for the promotion of international peace and thus avoid the impression that recent activities are solely concerned with military measures for national defence in case of war.

Science and Social Responsibility

What Science Stands For

By Sir John Boyd Orr, Prof. A. V. Hill, Prof. J. C. Philip, Sir Richard Gregory, Sir A. Daniel Hall, Prof. Lancelot Hogben. Pp. 132. (London: George Allen and Unwin, Ltd., 1937.) 5*s*. net.

THE future historian of science will not fail to chronicle that the early part of the twentieth century was notable for the gradual emergence of a social conscience among scientific men, which, he will aver, was greatly stimulated by the mis-use of certain scientific discoveries for inhuman ends (for example, poison gas against civilians), and by the recognition that extending application of science to industry did not appreciably improve the status or the prospects of the working classes.

The effects of the so-called impact of science upon social life have for long been made prominent in the columns of NATURE, but it is only of late that they have engaged general attention, for example, at recent meetings of the British Association. Incidentally, the word 'impact' seems ill-chosen, for science has been penetrating, not merely impinging on, the material social fabric for at least a century. At last year's meeting in Blackpool, some notable addresses were given on this theme, of which five are reproduced in the volume before us. Covering as they do a diversity of subjects, the choice of an apt title could not have been an easy one; hence the use of the woolly preposition-verb 'stand for'. If the question implicit in the title had been put to T. H. Huxley, his reply would have been immediate: "For

intellectual honesty and the fearless pursuit of truth". Pasteur would have said that science stands primarily for the alleviation of human suffering; Dr. Herbert Dingle that it stands for the co-ordination of sense observations; and a modern political dictator would see in science merely means of controlling the popular will. Sir Daniel Hall, in his vigorous and outspoken address on "Knowledge and Power", sees in science the means to enrich life, and he is as insistent on the non-material benefits as on the material. Sir Richard Gregory, writing on "Cultural and Social Values", elaborates this view, and strongly emphasizes the part played by science in developing the human mind, intellectually, æsthetically and morally.

Although the material fruits of scientific discovery are by no means ignored—Sir John Orr writes on nutrition and Prof. J. C. Philip on the ubiquity of chemistry—the ethical and educational issues which they raise, to wit, social responsibility for the applications of science and the place of science in education, are treated with the prominence they deserve. Sir John Orr thinks that the State has a moral responsibility for seeing that all its citizens obtain the food which health demands; and all the authors would agree that a one-sided training in letters, languages, or art is a very poor preparation for a modern statesman or ruler of men. A background of science is also essential for adapting our antiquated economic theories to modern actualities; and for encouraging the habit of acting on reason instead of on emotion. As Sir Daniel Hall says, "Everywhere we see individuals and nations responding to crude emotional impulses. . . . One function of an education based upon Science is to . . . teach boys and girls in their earliest formative years that all the world over men and women are . . . very much alike. Diversity exists among the individuals, and civilisation largely consists in giving that diversity full play". Prof. L. Hogben, the Sir Galahad of modern progress ("with manly grace, yet maiden meekness in his face"), demands nothing short of a transvaluation of all educational values.

After reading these interesting and valuable addresses, one is more than ever convinced that in real education for a full individual life and for good citizenship—the two are not incompatible—lies the one remaining hope for civilized humanity. As H. G. Wells so well put it: history is coming more and more to be "a race between education and catastrophe". E. H. T.

Experimental Embryology

Experimentelle Beiträge zu einer Theorie der Entwicklung

Von Hans Spemann. (Deutsche Ausgabe der Silliman Lectures gehalten an der Yale University im Spätjahr 1933.) Pp. viii + 296. (Berlin: Julius Springer, 1936.) 29.60 gold marks.

IN this book, Prof. Spemann has collected the main results of his life-long series of experiments on animal development and of those of his school, and he takes stock of the present position in regard to the problems of form-production. He is careful to warn his readers that this is not a text-book, but a record of a line of scientific thought pursued with the help of experimental methods. It is all the more welcome, for his acute analysis and logical progress from question to (experimentally provided) answer are most acceptable and helpful as guides through a field where isolated data are legion, but general principles few.

One of the most interesting of these principles is that known as 'double assurance'—the existence of two mechanisms which function independently to produce one and the same structure. Advanced to cover the case of the operculum of Anuran larvæ which can be perforated by an arm-bud and also without an arm-bud, the notion of double assurance has recently fallen out of favour since the realization that the perforation is brought about by other structures (the atrophying gills). But in regard to the lens, which in certain frogs can be induced by the eye-cup but can also develop without an eye-cup, Prof. Spemann shows that a duplicity of formative methods undoubtedly exists: one involving diffusion of a presumed chemical substance from the eye-cup, the other involving 'determination' *in situ* of the lens before closure of the neural folds. The question how such independent sets of processes are correlated and initiated in ontogeny, or have evolved in phylogeny, is not the least of the fascinating problems which this work raises. Special attention is devoted to fundamental definitions such as 'potency' and 'determination', and also to the results of biochemical analysis of the mode of action of the organizer.

Perhaps one of the most important sections of Prof. Spemann's book is that in which he reviews the hypotheses of 'gradients' and 'fields' that have been advanced to account for certain aspects of development. These he has difficulty in accepting; in particular: that the graded rates of tissue-activity

postulated by the gradient hypothesis can be the specific causes of the movements of cell-groups that result in gastrulation and form-production ; that the long adaptive history of the presence of certain highly differentiated structures in the head-region of animals can be condensed developmentally into a matter of differential oxygen supply to different parts of the egg ; that quantitative differences of rate of activity can result in qualitative differences of structure ; and so on.

To consider these objections here would take too long, and it must suffice to direct the reader's attention to them. Some of them can probably be met ; for example, it seems that yolkless cells cleave faster than yolk-laden cells, not directly because of greater activity of their protoplasm, but indirectly because the greater activity has oxidized and prevented the deposition of yolk in them. For the rest, it is probable that Child and his followers, in wanting to avoid appealing to *chemical* localizations to account for the *earliest* phases of differentiation, and being therefore driven back on to *physical* regional differences, have assumed too lightly that these differences are manifested in rates of metabolism or protoplasmic activity. It is, of course, the peculiar position of the gradient hypothesis that it is the only concept so far framed which covers the outstanding phenomena of development and regulation, but that it is exceedingly difficult to devise or perform experiments to prove its validity satisfactorily.

Without doubt, however, the greatest debt in which Prof. Spemann has placed his readers is for the admirably clear, and, it may be added, delightfully sympathetic, account he has given of the discovery and properties of the amphibian organizer. Many other authors have written about it in their books, and now at last here is a book dealing with these fundamentally important matters with the first-hand knowledge of their discoverer. The Silliman Lectures, on which the book is based, were delivered in 1933 ; but the book is brought completely up to date and includes the advances made during the past few years. G. R. DE BEER.

German Physics

Deutsche Physik

Von Philipp Lenard. In 4 Bänden. Band 1 : Einleitung und Mechanik. Pp. xv+249. (München : J. F. Lehmann, 1936). 10 gold marks.

PROF. P. LENARD is known to fame in two capacities, as the great experimenter whose work on phosphorescence and electronics is fundamental for modern physics, and as the politician, the stalwart fighter against the Jewish evil, of whom Herr Rosenberg has said : "As a thinker, however, Professor Lenard has taught that all knowledge is not the same, but that souls of alien race produce bodies of knowledge of a quite different spiritual content". The present book, which is the first of a series designed to present the elements of physics to young students, shows him likewise in two capacities, as a profound political thinker and patriot on one hand, and as physicist on the other. The two are spatially separated, the one activity being confined to the preface and introduction and having, apparently, very little influence on the rest of the book, which has something in common with the excellent little work of the non-Aryan Warburg.

The preface is a clear exposition of Prof. Lenard's views on the harm done to physics by the Jewish people. The author states that every race has its own kind of physics : in the past there was a physics of the Arabs ; to-day one can perhaps speak of a physics of the Japanese. There is no Negro physics, but recognition of the fact that there is a Jewish physics has been delayed simply because Jews write not in one language, but in many. "Jewish physics is then, a delusion and a sign of degeneration from the fundamental Aryan physics. It was necessary to stress this here with emphasis, for only when the fundamental opposition between Jewish and Aryan physics has become clear can the full appreciation of the value of the latter come into being again after having been lost so long." Einstein's theory of relativity is cited as particularly pernicious, as is also the Jewish division of physics into classic and modern, a piece of trickery designed to delude the German student. The preface, in fact, shows Prof. Lenard's competence in a field in which he is now an acknowledged master, for he has been entrusted with the task of reporting upon the question "Jewry in Science" for the Reichsinstitut für Geschichte.

The book itself consists of an introduction and a treatise on mechanics : it is being followed by three other volumes, dealing with the rest of physics. In the introduction, Prof. Lenard insists that all natural laws are simple, and that for an understanding of the fundamentals of physics very little mathematics is necessary. He shows himself particularly skilful in expounding the laws of mechanics, including such matters as the dynamics of rotating bodies, with a minimum of

mathematical analysis: even those students who have mastered the conventional exposition in the ordinary text-books of rigid dynamics will probably find that Lenard's method of presentation helps them to realize what it is all about. It is, however, hard to understand why the editor of Heinrich Hertz and the admirer of Maxwell should speak as deprecatingly of theoretical and mathematical physics as does Lenard. He is probably thinking on one hand of those physicists who conceal their lack of real understanding behind a mist of formulæ, and on the other hand of Faraday, who discovered basic quantitative laws without any mathematical knowledge; but there are theoretical physicists who discover new truths, while Faradays cannot be expected even once a generation. Lenard's book shows how far a man with a real grasp of physical laws can go without mathematics, how clearly many fundamental matters, even the elements of hydrodynamics, may be expounded almost without symbols. It is the work of a great lecturer and teacher. At the same time the simplicity is sometimes a little misleading—to say that "the measure of inertia is called mass. This is the definition of the conception 'mass'" is scarcely a fair solution of the difficulty.

The book is in the best German tradition, as exemplified, for example, by Riecke, from whose book some of the illustrations are, apparently, taken. It does not touch upon recent work, but limits itself to physics as known at, say, the end of the past century. This is possibly not at all a bad thing in a book intended to ground the student in the really fundamental parts of physics; but at the same time the interest could have been widened by reference to some more recent matters, such as the lift of an aeroplane wing in the section on hydrodynamics.

As a record of the methods of one who, besides being an investigator of the first rank was also one of the best teachers of his time, the book has a claim on the attention of all who have to present the facts of physics, German or even non-Aryan, to the student. E. N. DA C. A.

International Co-operation in Telephony

Comité Consultatif International Telephonique (C.C.I.F.)
Proceedings of the Xth Plenary Meeting, Budapest, 3rd–10th September 1934. Translated into English by the Technical Staff of the International Standard Electric Corporation. Pp. 660. (London: International Standard Electric Corporation, Ltd., 1936.) 25*s*. net.

IT is quite impossible in a short review to give an adequate account of the material, highly compressed and specialized in scope, which appears in the present volume of 660 pages. For the information in the field covered, that of telephonic communication between countries, it provides a bargain for those who can understand. There is so much that it takes the translators some two years to turn the five French volumes of the original into one volume in English. A detailed discussion is therefore not called for, especially since the material is not the work of individuals but of a large number of committees, the members of which may be located in several countries and do their work through the post.

The important thing about the Comité Consultatif International Telephonique (C.C.I.F.), which deals with telephony, and its parallel organizations, the C.C.I.T. and C.C.I.R., which deal with telegraphy and radio respectively, is that they form an excellent example of what can be achieved internationally if the problem to be solved—here telephonic communication at the lowest cost and with the highest speed—can be stated clearly and given to investigators who are prepared to find a solution which is acceptable to the majority. The interesting thing about the system is that it has no power of compulsion on its members to carry out any instructions. It sets up a sub-committee to report on the data of a particular aspect of the general problem; the report may say that research must be done, and this is allocated to the particular telephone authority which has the facilities for carrying it out. When this is done and further considered, the C.C.I.F. makes its recommendations, and it is up to the participating administrations to carry out these recommendations so far as is desirable at the time in order to co-operate with similar organizations in other countries.

As contrasted with the world of politics, the countries lose little time in adopting recommendations for the improvement in international telephony. Further, the principles adopted by the C.C.I.F. soon find themselves prevalent in the domestic sphere of the individual countries, since for telephony the greatest measure of uniformity in practice makes for greater general effectiveness in what we now regard as the indispensable method of communication. If engineers were given other problems within their competence to solve, the world would be a better place. L. E. C. H.

Flora of West Tropical Africa

the British West African Colonies, British Cameroons, the French and Portuguese Colonies south of the Tropic of Cancer to Lake Chad, and Fernando Po. By Dr. J. Hutchinson and Dr. J. M. Dalziel. Prepared at the Herbarium, Royal Botanic Gardens, Kew, under the supervision of the Director. (Published under the authority of the Secretary of State for the Colonies.) Vol. 2, Part 2. Pp. 293–651. (London: The Crown Agents for the Colonies, 1936.) 8*s*. 6*d*.

THE publication of the second part of vol. 2 completes this "Flora". The part contains the account of the Monocotyledons, which are represented by thirty-five families. The arrangement, as in the previous parts, follows Dr. J. Hutchinson's "Families of Flowering Plants". An artificial key to the families which are dealt with in this part is provided, and an index to families, genera and synonyms contained in the entire work occupies forty-three pages at the end. A short account of the principal vegetational types, accompanied by a map, is given. The illustrations, many of which occupy full pages, are by Mr. W. E. Trevithick and Miss S. Ross-Craig.

The most important family from the economic point of view is the Gramineæ, the key to the tribes and genera of which is by Mr. C. E. Hubbard. 137 genera, and 503 species of grasses are included. The account of the Orchidaceæ is by Mr. V. S. Summerhayes and is of especial interest as Schlechter's classification of the Angræcoid genera is used for the first time in any flora. The remaining families have been dealt with by Dr. Hutchinson, with the exception of one or two genera: *Cyperus* Linn. by Miss E. A. Bruce, *Pycreus* Beauv. with Mr. B. L. Burtt, and *Sorghum* Pers. with Mr. J. D. Snowden.

The number of new species is considerable, whilst many names published by Dr. A. Chevalier without description have been adopted, and will be validated by Latin diagnoses in future numbers of the *Kew Bulletin*.

The accounts of certain families such as Zingiberaceæ, Marantaceæ, and Palmæ have suffered from the imperfect nature of the specimens which were available, whilst the paucity of the material of a number of the species is at once noticeable on glancing through the pages: many have only once been collected within the area covered.

Commonwealth Bureau of Census and Statistics, Canberra

Official Year Book of the Commonwealth of Australia. No. 29, 1936. Prepared by Dr. Roland Wilson. Pp. xxxii+1015. (Canberra: Commonwealth Government Printer, 1936.) 5*s*.

THIS useful yearbook follows the same arrangement and embraces the same scope as previous issues: in statistics and to a less extent in description it gives a full account of nearly every aspect of Australian life, with further chapters on the territories of the Commonwealth including Papua, New Guinea and Nauru. History also finds a place in the detailed chronological table of events and a full reprint of the Commonwealth Constitution Act. Two excellent features are notable in the statistical matter: one being the comparable figures for other countries, and the other the figures for recent years in the Commonwealth.

As usual in every issue, there are certain distinctive features. Vital statistics, which are always treated fully, this year include analyses of the causes of death and also details of the dwellings of the population at the time of the census. There is also a special article on Australian population mortality.

The list of books and other publications dealing with Australia has been revised and is classified under subject headings. Maps show the distribution of rainfall and the location of the various artesian basins.

Mercury Arcs

By Prof. F. J. Teago and J. F. Gill. (Methuen's Monographs on Physical Subjects.) Pp. vii+104. (London: Methuen and Co., Ltd., 1936.) 3*s*. net.

THIS little monograph formed the basis of a course of lectures on design given at the University of Liverpool. The subject of the conversion of alternating to direct current is very much to the front at the present time. The crystal detector for converting the rapidly alternating currents employed in radio transmission work, into direct current, so that they make an audible signal in the telephone, is an example of their use. Liquid rectifiers can be used for converting an alternating current into a direct current, so that the A.C. supply can be used to charge an accumulator. This is a useful practical application. Copper oxide valves having unilateral conductivity are much employed for handling the large outputs used in industry. The introduction of the A.C. grid in Great Britain has also introduced an ever developing demand for these electrical valves.

Some engineers may think that too many mathematical theorems are given in this book, but examination shows that they are simple and easy to understand, and as a rule they explain very satisfactorily some of the physical phenomena.

Control of Electrical Motors

By Paisley B. Harwood. Pp. vii+390. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1936.) 22*s*. 6*d*. net.

EVERY year many thousands of motors are used for driving machinery. Each of these motors requires a controller of some kind. A simple manual starter might suffice for a small fan, but for the control required for a newspaper press something much more complicated has to be provided. Some engineer has to specify the duty of the controller required and someone else usually has to design and build it. In addition, it has to be installed and workmen trained to operate it. This book describes the working characteristics of the various kinds of motors and explains how these characteristics can be used for control purposes. Problems frequently encountered such as motor acceleration, dynamic braking and 'resistor' design are discussed fully. This book is easy to understand, and will be found useful by both engineers and operators.

Water-Power in the United States*

By Dr. Brysson Cunningham

III. Norris and Wheeler Dams

ON the side of the North American continent opposite to the valley of the Columbia River lies another notable valley—the Tennessee Valley, mainly in the State of that name, but also including parts of Mississippi, Alabama, Georgia, North Carolina, Kentucky and Virginia, with a basin comprising 40,600 square miles. For the co-ordinated development of this great territory, considerably larger than the whole of Ireland, a special body, the Tennessee Valley Authority, has been created by U.S. Congress with the duty of improving navigation, controlling floods, generating hydro-electric power and arresting soil erosion. For this purpose a series of ten dams of varying heights, and at irregular intervals, has been designed and in part executed along the course of the River Tennessee and its tributary the River Clinch. Perhaps the most notable of these, until recently, was the Wilson Dam, 137 ft. high and 4,860 ft. long, impounding a reservoir with a capacity of half a million acre-feet and generating power to the extent of 261,400 horse-power. The Wilson Dam was completed in 1925; but since that date, two other and even more striking dams have come into existence, the Norris Dam and the Wheeler Dam, completed as recently as last year.

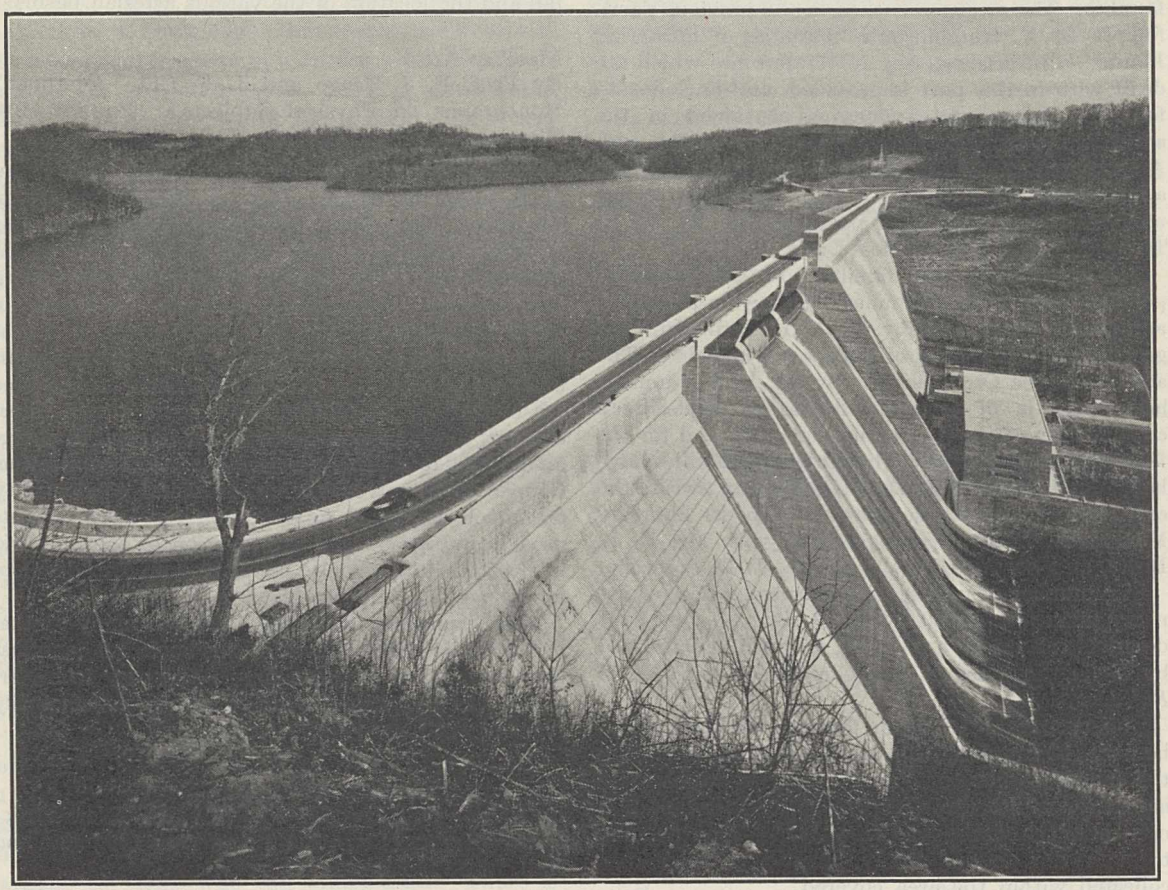

Fig. 1.

General view of the Norris Dam on the Clinch River, Tennessee, showing the power house and impounded lake.

Of the two, the Norris Dam (Fig. 1) located on the Clinch River in north-eastern Tennessee, about 25 miles north-west of Knoxville, is the loftier and impounds a reservoir of greater capacity. The impounded area is more than 50 square miles, with

* Previous articles in NATURE, May 1, p. 738, and May 22, p. 823.

a shore line of over 700 miles, winding in and out of many valleys formed by the hills above the dam. The reservoir capacity is 2,570,000 acre-feet at normal water-level, but may attain a maximum of 3,400,000 acre-feet. The dam itself is a straight mass-concrete structure, with a total length along its crest of 1,872 ft. It has a height of 265 ft above the lowest foundation level and is 204 ft. thick at the base. The spillway, confined within side walls of concrete, discharges into a concrete hydraulic 'jump pool', which extends 215 ft. downstream from the toe of the dam. Along the crest of the spillway, between two 12-feet concrete piers, are three hydraulically-operated steel drum gates, each 100 ft. long, designed to retain water at flood peak-level.

Though primarily a flood control and navigation work for the Tennessee and Mississippi Rivers, the Norris Dam is provided with power-generating plant consisting of two 66,000 horse-power generators. The cost of the entire installation was 35 million dollars (£7,000,000).

The Wheeler Dam (Fig. 2) is not remarkable for its height, which is only 72 ft., but it has a length of 6,335 ft.—1,475 ft. more than the Wilson Dam and 4,463 ft. more than the Norris Dam. It is located on the Tennessee River, 275 miles above Paducah, Kentucky, where the Tennessee joins the Ohio. Situated at the upper end of Wilson Lake, it is 15½ miles above the Wilson Dam at Muscle Shoals. It impounds a volume of water aggregating 1,260,000 acre-feet in a reservoir which has a shore line of 951 miles.

The dam is a gravity concrete structure, with the above mentioned length of 6,335 ft. composed, successively, of a non-overflow section 177 ft. long, 613 ft. power house, another non-overflow section 718 ft. long, 2,700 ft. of spillway, 1,756 ft. of non-overflow, 162 ft. of navigation lock structure and a final non-overflow section of 209 ft. to the north bank of the river. The spillway is divided into sixty bays, each bay being surmounted by a radial gate, 15 ft. high and 40 ft. long.

The power plant is of the outdoor type, with no housing over the crane or the generating units, the generators being protected from the weather by metal covers. The station is designed to accommodate eight main generating units of an

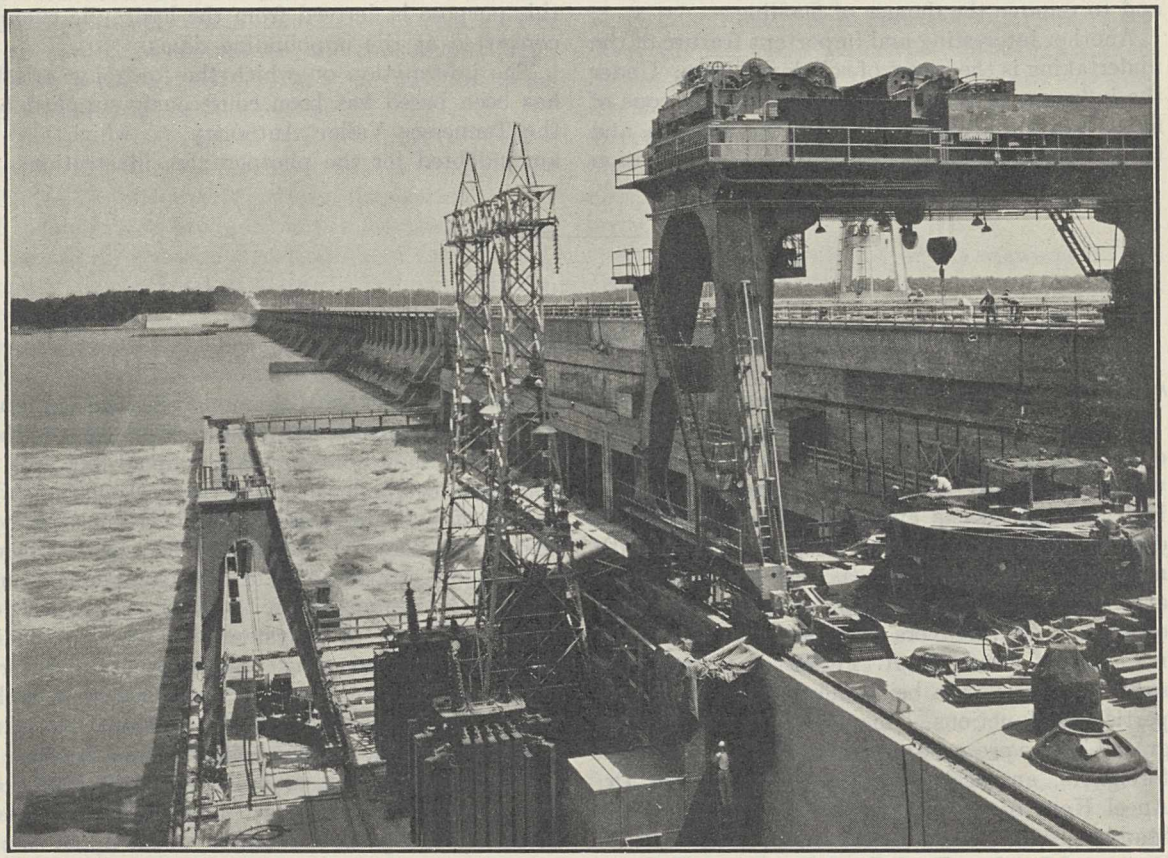

Fig. 2.

DOWNSTREAM VIEW OF THE WHEELER DAM ON THE TENNESSEE RIVER IN ALABAMA.

aggregate capacity of 360,000 horse-power. Only two units of 45,000 horse-power each, have as yet been installed. The system came into operation on November 9 last.

The individual projects, which have been described above, form part of an organized design for the utilization of the resources of the whole of the Tennessee watershed, by means of storage reservoirs in the headwaters, which will retain the run-off during the rainy season and release it during the dry season, so regulating the flow during the whole year to suit the requirements of navigation and to remove the danger of flooding.

Another interesting and important feature of the undertaking is the arrest of soil denudation. Under the influence of heavy rainfall, millions of tons of fertile top soil are removed from the hillsides, and further large quantities are lost by sheet erosion on the plains. This silt would normally be transported by the river currents over long distances, but the interposition of a series of dams causes it to settle and enables it to be reclaimed. But, going a step further back in preventive measures, the Authority is engaged in treating the eroded hillsides and encouraging the growth of vegetation with the aid of plant foods, and in particular, phosphates. At Muscle Shoals, a demonstration unit has been built to produce triple superphosphate from the raw phosphate rock found in central Tennessee. The electric power used for this purpose is derived from the hydraulic energy converted at the impounding dams.

The information on which the foregoing article has been based has been courteously supplied by the Tennessee Valley Authority, to which also I am indebted for the photographic illustrations.

Natural and Artificial Membranes

THE late Sir William Hardy was responsible for the foundation of the Colloid Committee of the Faraday Society, the duties of which were periodically to call meetings at which biologists, physicists and chemists could meet to discuss topics of mutual interest. The fifth meeting of this character was held on April 22–24 at University College, London, under the presidency of Prof. M. W. Travers of Bristol and the chairmanship of Prof. R. A. Peters of Oxford, the subject of discussion being membranes, their properties and functions. This particular meeting was graced by the presence of a number of distinguished foreigners, amongst whom we may include Dr. Ancel Keys of Rochester, Min., L. R. Blinks of Stanford, Mrs. Brooks of Berkeley, Cal., Prof. E. Gorter of Leyden, Prof. R. Collander of Helsingfors, Prof. E. Manegold of Dresden, Prof. Kurt H. Meyer of Genthod-Genève, Dr. W. Teorell of Uppsala and Dr. W. Wilbrandt of Bern, who all took part in the discussion. We were also pleased to note the presence of Profs. H. Mark and W. Ostwald, constant visitors to these meetings. It was to be regretted that several distinguished scientific workers were unable to present their papers ; these included Profs. A. Krogh, Newton Harvey, E. Ponder, W. J. V. Osterhout and Cole.

It was clear from the contributions and from the discussion that in spite of the importance, both structural and functional, of membranes in organized living systems, we are still a long way from obtaining a complete description of the microstructure of even one of these or a general interpretation of their behaviour. It would appear that all natural membranes are composite in the sense that they contain both a lipoid-like and a non-lipoid component. The structural relationship of the components to each other is still *sub judice*. Exponents were not lacking of the extreme view of complete stratification of each component, one possibly in a monolayer, as well as those who upheld the view of a systematic mosaic or even haphazard intermingling. Some species of interaction, at least between the components of each 'phase', appears necessary to account for the physical properties of the complete membranes.

Whilst cationic and anionic permeability of the protein component of the membrane and the 'solubility of organic substances' in the lipoid component, as well as the ionic partition as defined by the Gibbs – Donnan membrane equilibrium are readily understandable in general terms, it appeared to many that these now well-recognized and important factors do not suffice to account for all the observed phenomena. So many questions were asked by our biologist friends at the meeting for the consideration of the physicist and chemist that it seemed difficult at times to realize how much progress had been made since the first meeting summoned by the Colloid Committee. There were three questions raised of unusual interest to many non-specialists present, to which no generally satisfactory answers could be given. These were the apparently abnormal behaviour of the potassium ion, the origin of bio-electric phenomena and the mechanism of narcosis.

In a medium containing a variety of salts, no satisfactory mechanism has been proposed for the relative enrichment in potassium of a portion separated from the bulk by means of a membrane, for example, red cells as discussed by Dr. M. Maizels. It was doubtful whether membrane charge, ionic size, solubility or complex formation could be the operative factors. It may be significant that not only is the lipoid/water partition coefficient for many potassium salts or complexes favourable for such separation, a view supported by Prof. Osterhout, but also that the ionic mobility of the potassium ion is some fifty per cent greater than that of the sodium ion, and thus in a dynamic system with migrating water as medium some separation should be possible.

According to Profs. S. C. Brooks and W. Stiles, separations due to mobility difference might be effected by exchange with ions, for example, those of carbonic acid produced in the metabolism. But if the final dynamic distribution of ions be calculated on the basis of a steady Donnan diffusion equilibrium, Dr. Teorell showed that under biological conditions the accumulation due to interdiffusion could not amount to more than 2 per cent.

If separation be accomplished by continued ionic transport involving an electric potential or a counter current of flowing liquid, it serves to emphasize the fact frequently forgotten by the non-biologist that these interesting properties are exhibited by living systems, and as such their dynamic state, involving possibly a continuous and steady flux of ions or liquid, may prove of paramount importance.

Such a theory demands the existence and the maintenance of an osmotic or an electrical gradient present within the membranes. From the discussion supported by Dr. G. S. Hartley, it was considered that diffusion or non-polarizable potentials could perform work ; Prof. H. Freundlich suggested that work could also be obtained from temperature gradients (Soret effect). Drs. D. Gatty and R. B. Dean pointed out that redox potentials cannot be responsible for ionic accumulation. Dr. F. C. Steward and Dr. Ancel Keys inclined to the view that water and salt transport through plant cells and gills were due to metabolic processes oxidative in character.

Similar considerations may apply to the phenomena of bioelectric potentials. Some confusion exists owing to the misapprehension of the facts concerning the Donnan potential ; namely, that the membrane potential can only be measured either electrostatically by the use of non-reversible electrodes in the two solutions separated by the membrane, or by the difference in the potentials given by each solution separately to a reversible electrode against a standard electrode in a solution containing the common ion. In spite of the fact demonstrated beautifully by Prof. Kurt Meyer that both acidophil and basophil membranes can be detected by staining methods in systems exhibiting bioelectric phenomena, it is not only doubtful whether these methods are certain criteria for selective ionic permeability, but also the serious difficulty that currents can be drawn from these presumably readily polarized systems does not appear to be surmountable unless a dynamic mechanism is operative.

A number of papers, including those of Profs. K. C. Cole, L. R. Blinks and Drs. Dean and Gatty, involved the use of electrical measurements to obtain information as to the equivalent electrical circuit which a cell with various hypothetical structures would present ; *inter alia* it was brought out that Frickes' previous estimate for the thickness of the red cell wall must be considered in error.

An excellent summary of the views as to the origin of potential differences obtained in nerves was given by Dr. S. L. Cowan, who showed that diffusion potentials alone are not adequate. Dr. H. Rosenberg elaborated the core conductor system, whilst Mr. J. Z. Young discussed the chemical evidence as to the structure of the axon and the myelin sheaths of the nerve. He suggested in a general way that radially orientated lipoid layers may be responsible for nervous stimulation. It may be significant in this connexion that the phase boundary potentials exhibited by monolayers at interfaces are generally much larger than those potential differences obtainable by a Gibbs – Donnan distribution in biological systems containing a relatively high salt concentration ; such monolayers on polarization naturally accumulate ions, and provided there exists some mechanism for destroying or changing the monolayer on stimulation, the result would be an ionic flow. Papers were communicated on the properties of protein monolayers by Prof. E. Gorter, by Dr. J. S. Mitchell and Dr. J. H. Schulman. The Overton lipoid solubility hypothesis of narcotic action was revived in new form by Prof. Kurt Meyer, utilizing oleic acid as the model for the lipoid in evaluating the critical partition coefficients.

Prof. A. J. Clark, in a critical review, pointed out that the problem did not appear as simple as suggested by either Overton or by Traube, and that it appeared probable that selectively narcotizable and differentiated areas were involved. Several anæsthetics do not appear to conform to the oleic acid solubility rule, and the response of various types of nerve systems is not identical. The hypothesis advanced by Dr. Schulman and the present writer, in which interaction of the polar head or the non-polar portion

of a narcotic with a lipo protein complex, appears to overcome the disadvantage of the older theories, but in its present state of development is too general and vague to be of much practical utility.

Papers dealing with the more practical side of artificial membranes were presented by Prof. Manegold and Dr. W. J. Elford. Dr. G. S. Adair gave a paper dealing with refinements of the Donnan membrane equilibrium.

The meeting was one full of interesting topics, but somewhat discursive owing to the attempt to deal with too many diverse phenomena at one time. ERIC K. RIDEAL.

The Soviet Expedition to the North Pole

THE institution of a station for scientific research within a few miles of the North Pole marks a new phase in polar exploration, and is a development of the intensive exploration which the Soviet Union has pursued in Arctic regions during the last few years. The earliest attempts to penetrate high northern latitudes, away back in the sixteenth and early seventeenth centuries, were inspired by the hope of direct trade routes with the Far East and were alternatives to the still older attempts to find the North-West and North-East Passages. The early nineteenth century saw a revival of the northern ventures, and the Franklin disaster in the middle of that century led to a focusing of interest in polar regions, and stimulated journeys that were more adventurous than scientific in their outlook. But in an age of exploration that relied on man-hauled sledges and preserved food, that feared low temperatures and had to reckon with inevitable scurvy, the inner polar regions maintained their isolation.

Petermann's hypothesis of an open polar sea led to more than one failure and was utterly refuted by Nares, Greely and others. Then Nansen, imbued with scientific aims, brought a new technique to bear on the problem. The pack-ice that had thwarted his predecessors was to afford him the means of advance, and so in the drifting *Fram* he penetrated to lat. 86° N., and on foot some miles farther north. Nansen discovered a deep, landless polar ocean covered with drifting pack-ice. There the solution of the polar problem was to rest for some years. Even if Cagni beat Nansen's record by a few miles and Peary actually reached the Pole in 1909, these expeditions were mainly adventurous achievements and added little to scientific research. Amundsen's attempt to emulate Nansen had its value but failed to gain a high latitude.

Sledge and ship had both played their part in different ages : air-voyaging was next to be tried. Early attempts were naturally failures. In 1925 Amundsen reached 87° 43′ N. by air and returned with difficulty. In 1926, with Nobile, he crossed the Pole by airship, and in the same year Byrd also reached the Pole. These attempts showed not only the possibility of flying in arctic regions but also revealed large smooth floes with a relative lack of pressure ridges in the vicinity of the Pole. None of these expeditions, however, stayed more than a few hours, and others much less, at the Pole or anywhere in inner polar regions. Thus observations were casual and of little help, while little or no oceanographical work could be done.

Prof. O. J. Schmidt, who is in charge of the Soviet expedition to the Pole, aims at a more valuable type of exploration in placing a party in the vicinity of the North Pole for at least twelve months. The experience of arctic flying and arctic conditions generally that Soviet aviators and explorers have gained in recent years makes this possible. In connexion with the northern sea route between Europe and the Siberian rivers, of which much use is now being made, meteorological observations from the far north will be invaluable, especially in relation to the movements of the pack-ice. These observations are to be taken both at sea-level and at high altitudes. The direction of the ice drift will be studied and Prof. Schmidt points out that the station will be shifting continually in position, but it is not known if this shift will be steadily in one direction or rotatory around a certain point. In any event, these movements will facilitate soundings of the depths of the Arctic Ocean, of which little is known.

Chemical and physical analyses of the water are important in relation to the inflow of Atlantic water, and the bearing of that inflow on ice formation and ice-wastage. Good and bad ice years have long been recognized at Spitsbergen, Greenland and in the Kara Sea, but the factors involved are not understood and prediction is impossible. Magnetic and geophysical researches are also planned and biological examinations of the sea-water will be made. The station will be in radio communication with Moscow, and, it is hoped, in flying communication through many months of the year. The smooth floes afford a suitable

aerodrome, and shelters are being constructed. The Soviet Union Year Book Press Service, to which we are indebted for information, lays stress on the importance of the researches in establishing regular communication by air between Europe and America across the Arctic Ocean, a modern revival in new guise of the earliest dream of a transpolar route.

Prof. Schmidt has chosen as the resident leader of the station I. D. Papanin, who has had long experience in Franz Josef Land and at Cape Chelyuskin. With him are three other experienced men, E. T. Krenkel, P. P. Shirshov and E. K. Federov. The winter dwelling is to be a tent of duralumin and rubber lined with eiderdown, reindeer hide and canvas. It is 3·7 m. long, 2·7 m. wide and 2 m. high, and has windows of unbreakable glass. The whole tent weighs only 350 lb. This will replace the silk hut used on the expedition's arrival on May 21. Paraffin is to be used for cooking and for generating electric light. The original site of the station was 20 kilometres from the North Pole on the Franz Josef Land side. R. N. R. B.

Obituary Notices

Prof. Alfred Adler

ALFRED ADLER, who died suddenly on May 28, was the son of a Viennese merchant. He was born in 1870 and educated in Vienna. He decided to study medicine and qualified at the age of twenty-five years. Soon after qualification he fell under the influence of Freud, and formed one of that close little group which developed the new science of psychoanalysis. He stood some of the almost universal abuse which was flung against the Psychoanalytical Society when the new ideas became known.

In spite of this, Adler's ideas never accorded closely with those of Freud and almost from the beginning he showed that he intended to take a different road from his master. Whereas Freud developed elaborate psychic entities based upon the unconscious—the id, the superego and so on—Adler tended to simplify instead of elaborate.

So early as 1907 Adler described the influence of what he called 'organ inferiority'. He described cases which showed that the inferiority of an organ or group of organs might occur as a tendency in one individual or even in a family. He noticed that one person might have a series of diseases all attacking, for example, the urinary system. In the case of a family, one member after another would suffer from diseases all centred on one organ.

From this idea of the inferiority of an organ or group of organs, Adler developed the theory of 'overcompensation'. He felt that not only did the body try to compensate itself for the weakness but even succeeded in fortifying itself to an unnecessary degree, as, for example, when the scar following a wound is thicker and stronger than the original skin. He believed that this 'overcompensation' was not limited to the somatic or body-structure but also invaded the psychic superstructure. The result of this was that the organ which started as inferior was finally so overcompensated that it became superior to the other organs. He gave Beethoven as an example of this process, his early deafness only increasing the capacity to compose more and more magnificent music.

Adler found that he could explain every phenomena by his principles of inferiority and over-compensation. He put forward his views openly, and as they did not accord with those of infantile sexuality which Freud was developing at this time, there was a feeling of tension in the Vienna group of the newly formed Psychoanalytical Society. Adler was therefore asked to read a paper in which he could put his ideas before the Society. The result was a tremendous storm and, in spite of the fact that he was president of the Vienna Group, he was asked to withdraw. He did so and took nine other members with him. He then founded the Society for Individual Psychology (first called the Society for Free Analysis).

Away from the trammels of the Freudians, Adler developed better and applied his views to all types of neurosis. He thought that all neurotic illness was caused by a feeling of inferiority and a wish to dominate the environment through illness. This feeling of inferiority was engendered in childhood by the infant's weakness and its relation to its parents and brothers and sisters. This view is, of course, in direct contradiction to that of Freud, who believes that neurosis is the result of maldevelopment of the sexual instinct, which produces fixations at various psychic levels.

Adler felt that since man has the greatest strength, women must have a desire to be masculine and that neurotic illness in them is to be attributed to their wish to possess the strength of a man. He called this the 'masculine protest'. He believed that this was the cause of prostitution, homosexuality and even crime, as well as neurosis. Much of the neurotic illness in men, he thought, was due to the feeling of inadequate masculinity. All symptoms were explicable as symbolic attempts to express a purpose—in fact all neurosis was the expression of a purpose; fear of falling, for example, was explained as a fear of 'falling' sexually. Vomiting was a desire to reject some unpleasant thought or fact, and so on. He called this symbolization 'organ jargon'.

Adler's psychology is explanatory and easy to understand, but it lacks a coherent systematization

such as one finds in the psychoanalytical theories. This looseness allows anyone to interpret the theories as he fancies, and tends to reduce them to art rather than science. Adler himself felt this, and a year ago told the writer that he thought that many who preached his views did not properly understand them. He was reluctant to build a rigid system, however, since he felt that the human mind was not easily crushed into any inflexible mould.

Adler worked in Vienna until a few years ago, when he left and accepted a professorship in America. He has visited England nearly every year since in order to lecture. He was a lecturer of singular patience, and would answer questions long after his lectures were supposed to have finished. He was a man of great friendliness—those who knew him might differ from him in theory but they could not quarrel with him in person for he never showed the aggression of which he wrote so much.

Major J. H. Hardcastle

MAJOR JOHN HERSCHEL HARDCASTLE, who died at Sidcup, Kent, on April 21, was born on May 9, 1870, at Netherhall, Bury St. Edmunds, a son of the late Henry Hardcastle, barrister. His maternal grandfather was Sir John Herschel, the astronomer, and his great-grandfather was the famous discoverer of the planet Uranus.

Hardcastle was educated at Haileybury and the Royal Military Academy, Woolwich, passing out ninth from the latter; thence he received his commission in the Royal Regiment of Artillery in 1889. He was promoted to captain in 1899. In 1902 he joined the advanced class for artillery officers at the Ordnance College, Woolwich, and there came under the influence of Sir George Greenhill. After obtaining the 'p.a.c.' certificate he was employed by the Ordnance Board at Woolwich in 1904–6 on important research work on the resistance of the air to artillery projectiles. This work culminated in the production of the official Ballistic Tables, in the compilation of which he assisted. These tables are still used.

Retired from the Army in 1906, Hardcastle thereafter devoted himself to research work in internal ballistics and small arms, working successively with the firms of Kynoch and Nobel's Explosives. During this period he introduced the pointed bullet. At the beginning of the Great War he returned to Woolwich, and was given charge of the small arms range in the Arsenal. Here he continued his researches into the ballistics of small arms, and towards the end of the War, produced formulæ which facilitated the rapid calculation of small arms ballistics. He remained at Woolwich until 1924 and assisted in the compilation of the "Text Book of Small Arms". After his retirement from the Service he acted as ballistic consultant to Imperial Chemical Industries, Ltd., and B.S.A. Guns, Ltd.

Most of Hardcastle's publications are in the form of papers and articles in the *Royal Artillery Journal*. He put his scientific knowledge of small arms to practical use, as he was a very fine rifle shot and was in the English Elcho team. F. R. W. HUNT.

Prof. W. Natanson

POLISH science has sustained a heavy loss by the death, on February 26, of Prof. Władyslaw Natanson, who had occupied the chair of physics at the Jagellonian University of Cracow since 1904, after having been assistant professor there since 1894.

Natanson was born in Warsaw on June 18, 1864, his uncle, Jacob Natanson, being a Polish chemist of some distinction. He graduated as doctor of philosophy at Dorpat in 1888, having previously studied at St. Petersburg, Graz and Cambridge. He then went to Cracow, where his post-graduate researches in physics gained him the doctorate of that university too (1891). Three years later he became *Docent* and continued his work at Cracow until his death.

Practically all his life, Natanson was working in adverse circumstances. It was never easy for him to secure adequate equipment for his laboratory from the Austrian authorities in pre-War days, nor had the Polish Government funds to spare for several years after the country had regained its independence. In spite of this, Natanson succeeded in making valuable contributions to his science, particularly by his publications on the kinetic theory of gases, which he developed to explain phenomena connected with diffusion, expansion by heat, vortex rings, molecular collisions, etc.

Theoretical and mathematical papers from his pen and dealing with isothermal formulæ, Clerk Maxwell's law, the kinetic theory and dissociation phenomena have appeared from time to time in British periodicals, but most of his work was published in Continental journals. In 1903 he observed the accidental double refraction of liquids, which he endeavoured to explain by a molecular theory of refraction, reflection and extinction. He is perhaps best known in Great Britain for his investigations on the dissociation of nitrogen peroxide on heating, and its re-association on cooling.

For his own countrymen, Natanson contributed articles on scientific phenomena (several, for example, dealt with the blue colour of the sky) to the semi-popular reviews and he also wrote some standard text-books of physics. His "Elements of Physics", first published at Lwów in 1894, has gone through five editions. Prof. Natanson also had a wide knowledge of the history of science, and his biographical sketches of Bacon, Newton, Lord Kelvin and others did much to stimulate an interest in British culture and science among the Polish people. He was one of the original members of the Cracow Academy of Sciences, and was for seven years the editor of its publications. Later his work gained him recognition abroad, and among other distinctions he held the office of vice-president of the International Union of Physics. G. D.

WE regret to announce the following deaths:

M. Louis Ravaz, *correspondant* for the Section of Rural Economy of the Paris Academy of Sciences.

Prof. J. M. Reade, professor of botany in the University of Georgia, on May 8, aged sixty-one years.

News and Views

Science and Peace Problems

DURING the National Peace Congress held in London on May 28–31, under the presidency of Dr. E. W. Barnes, Bishop of Birmingham, one of its commissions considered the attitude which members of the scientific and medical professions should adopt towards peace questions. The chairman, Prof. S. Chapman, who also gave the opening address in place of Prof. P. M. S. Blackett, who was absent through illness, advocated the establishment under international auspices of a world service of information on current events, as objective as possible, without comment or propaganda: controlled by a body of men chosen internationally according to some carefully devised scheme (like the judges of the Hague Court), for their personal qualities of judgment and impartiality; the daily bulletin being broadcast in the principal national languages, to provide men and women everywhere with a basis on which to form their political judgments. Dr. H. Joules and Dr. Howard Hughes described the organization and work of the Medical Peace Campaign.

THE meeting devoted much time to the discussion of the present rearmament programme; criticism was made of its exclusive emphasis on imperial defence rather than on collective security, and its execution by private firms for profit, without the safeguards recommended by the Royal Commission on Armament Manufacture. The official plans for air raid precautions were also discussed, and the experiments in progress by peace groups of scientific workers at Cambridge and Oxford, which question the efficacy of those precautions (see NATURE of April 10, p. 606). Members of these groups outlined the means which had been found effective in arousing interest among laboratory workers and maintaining discussion of peace. While it was judged unlikely that scientific workers would be able to reach common ground on the question whether or no they should participate in military preparation, tolerance between men taking different attitudes on this question was urged, and co-operation in peace work to the utmost possible extent. It should be made clear that men of science, in common with most members of the public, regard war as degrading to human progress as well as futile, and are willing to help in national and international work aimed at discovering and removing its causes and preventing its recurrence.

Relations between Science and Philosophy

THE fourteenth 'Unity School' organized by Mr. F. S. Marvin was held in the last week-end in May at Farnham Castle, and was a great success. The subject for discussion was the "Relations between Science and Philosophy", and as both sides were well represented and a large proportion of the disputants were fairly young people, the debates were as lively and vigorous as they were invariably friendly. Though it was remarked afterwards that there seemed little 'unity' in the arguments, it may well be that a perfectly frank statement of difference in an atmosphere of friendly feeling is the best way to reach a useful working agreement. There was certainly no attempt to set up any water-tight compartment for philosophic thinking on one side, or any rejection of the useful function of philosophy by the men of science on the other. The only body of people who seemed to stand isolated and not acceptable to either side were the 'logical positivists'. But Wittgenstein was only a name—if so much—to most of the audience, and further investigation was clearly necessary.

ON the side of science, the most novel and striking contribution was made by Dr. Redcliffe Salaman, director of the Potato Virus Research Station at Cambridge. His demonstration of the infinitesimal size of these organisms introduced one into a region where vital and inanimate objects seem to exist indistinguishably side by side. He gave full details of the discoveries which have lately been made in this sphere in the United States, especially by Stanley. A full and useful discussion followed as to the nature and working of the protein crystalloid bodies now revealed in certain viruses. It turned largely on matter which has been communicated to NATURE, and left many of the audience wondering whether in these crystals something has not been found which is an actual link between living and non-living. It was decided at the close of the proceedings to continue the series of 'Unity Schools' on somewhat similar lines next year. Mrs. Innes, of Coneydale, Welwyn Garden City, will be glad to answer any inquiries. Contact between the various branches, especially of physical and social science, was felt to be the most useful line of approach to 'unity' at the present time.

Total Solar Eclipse of June 8

FROM Press reports, it appears that the total eclipse of the sun was observed under perfect conditions on June 8 at Canton Island in the South Pacific, where the American expedition organized by the U.S. Naval Observatory and the National Geographic Society of Washington had established its stations (see NATURE, April 24, p. 698). The *Daily Telegraph*, in co-operation with the National Broadcasting Company of America, arranged for Prof. A. Fowler to speak from a studio in Broadcasting House, London, to leading astronomers observing the eclipse on Canton Island, and the conversation is described in the issue of June 9. It appears that many photographs were taken, and that the corona, as was expected, was of the type of maximum solar activity.

The spectroscopic programme also seems to have been carried out successfully. The *Daily Telegraph* is to be congratulated upon its enterprise in obtaining direct information by radio of observations of an eclipse from a station more than 9,000 miles away. This very remarkable achievement merits a place in astronomical history.

Blood Groups in Africa

AT the beginning of April, Dr. Ronald Elsdon-Dew and Mr. J. de Bruijne, of the South African Institute of Medical Research, left Johannesburg on a prolonged tour of investigation of the blood groups of African peoples, and more especially of the Bantu. They propose to examine various tribes in Southern and Northern Rhodesia, Nyasaland, Tanganyika, Kenya, the Anglo-Egyptian Sudan, Uganda and the Belgian Congo. Dr. Elsdon-Dew, who holds the appointment of assistant pathologist in the Medico-legal Department of the South African Institute of Medical Research, has been engaged on an intensive study of the African blood-groups since 1932, and has examined the blood grouping of more than five thousand South African Bantu. In publishing his results, and the somewhat startling inferences as to the racial history of Africa to which in his opinion they appeared to point, he emphasized the fact that theorizing on the serology of African peoples must of necessity be tentative, for in this respect Africa is still the Dark Continent. The present expedition has been undertaken to remedy the lack of material for purposes of comparison which gave to his ethnological argument a highly speculative character, more especially as regards the existence of a hypothetical primitive black race, possessing neither the *A* nor the *B* groups, to which his results appeared to point, and with which, serologically, the Southern Bantu apparently showed a closer affinity than any other people of which the blood groups were known. The expedition, which is travelling by motor caravan, will be at work for about six months.

Local Archæological Studies in Nottingham

PUBLICATION of the first annual report of the Excavation Section of the Thoroton Society of Nottinghamshire falls opportunely at a moment when attention has been directed in Parliament to the opportunity open to local archæological and scientific societies to educate their neighbours, the local authority and the local public generally, as to the desirability of preserving for posterity buildings and erections of historical or æsthetic interest and places of natural beauty (see NATURE, Feb. 27, p. 345). The Excavation Section of the Thoroton Society has been formed as an outcome of the enthusiasm of a small number of students of the past history of the city of Nottingham, but more especially through the efforts of Mr. G. F. Campion, who was anxious that the work of local excavation carried out by himself and his father before him over a number of years should be placed on a permanent footing. Very wisely it was decided not to form a new society for this purpose, but to join forces with the Thoroton Society, the long-established centre of archæological and antiquarian interest in the county. Thus the activities of the newly established section will be directed not only to continuing the work of excavation, but also to the expert examination and record of the numerous evidences of the city's past cultural history which are being brought to light with increasing frequency in the building operations in progress in connexion with the improvement and enlargement of business and other premises in Nottingham. It should at least be able to ensure that nothing worthy of note is overlooked—a matter of much importance in an area like Nottingham which is honeycombed with underground caves. The record of the first year's operations covers not only the excavations in Nottingham's Norman and Plantagenet castle, and a Roman fortress, but also the supervision of excavations for building operations on eight sites, which produced some interesting samples of medieval pottery, including a fabric unknown from any other area and thought to be an exclusive product of Nottingham.

Miocene Implements

A NUMBER of years ago, the late Mr. Edward Westlake collected a very large series of specimens of flaked chert and flint from Upper Miocene deposits at Aurillac, and other sites, in the Cantal, France. These specimens, which number several thousands, are now housed at the Museum, Ipswich, where they are being studied by Mr. Reid Moir. When his examination of the collection is completed, Mr. Reid Moir proposes to describe it; but it may be said that a great many of the specimens exhibit, in a marked degree, certain characteristics which have been generally accepted as indicative of artificial shaping. Archæologists, and those interested in the antiquity of man, owe a debt of gratitude to Mr. Westlake's insight and care in getting together such a vast and important collection, which, from many aspects, is unique. There would seem to be no doubt as to the Upper Miocene age of the deposits from which the specimens were derived, and their adequate description may well prove to be a significant contribution to the solution of the problem of the antiquity of the human race.

Dry Crossings of the Nile

WITH reference to the two letters to the Editor in NATURE of June 5, p. 961, under the title "Dry Crossings of the Nile", from E. J. Wayland and Dr. H. E. Hurst, we have received the following from Dr. A. E. H. Tutton: "It may be of interest to add to the record of the observations by Dr. Wayland, from the air in 1930 and by an actual passage on foot in 1933, that a natural bridge does from time to time exist across the higher reaches of the Nile, strong enough to bear the elephant, a still more remarkable observation by my late brother-in-law, Mr. Leonard Loat, made and recorded by him in the year 1902, during his survey of the fishes of the Nile for Lord Cromer's Government and the British Museum (Natural History). In a letter to me

describing the difficulties then being met with, owing to the remarkable density and solidity of the 'sudd' between Fashoda and Gondokoro, he states that he had just observed a herd of over two hundred elephants walking on the sudd over the river. When he returned to England for a short leave, before undertaking a similar survey up the Blue Nile, I asked him whether he had recorded a solid fact or was indulging in a traveller's tale, as it seemed a very extraordinary circumstance, if true. He was most emphatic, however, as to its being literally true, and that he had ceased to count the elephants after the two hundredth; also that he had been accompanied by a big-game hunter friend at the time, as well, of course, as by the Egyptian skipper and Sudanese crew of his little survey vessel, a specially fitted dahabeyah, all of whom were greatly interested in the unusual spectacle. It is referred to in the obituary notice of Mr. Loat's work which appeared in *The Times* of April 30, 1932."

Memorial Tablet to Telford

DURING the conversazione of the Institution of Civil Engineers held on June 2, the president, Sir Alexander Gibb, unveiled a small tablet in the newly completed extension of the main library recalling Telford's gift of books to the Institution. When Telford accepted the presidency in 1820, he wrote: "As proof of my sincere desire to promote the prosperity of the Institution, I beg leave to present it a collection of Books connected with the profession of a Civil Engineer; they will form no inconsiderable portion of a suitable library, destined, I trust, at no distant period to become a valuable one. . . ." The library now contains 62,300 volumes, and Telford's books, with some others, have been placed in cases under the window in the west wall of the extension. Hollin's bust of Telford stands on the top of the cases, just above the tablet recalling his gift, while the oil paintings which the great engineer bequeathed to the Institution of the Pont Cysylltau Aqueduct and the Menai Bridge have been hung one on each side of the window.

Hooker's Aneroid Barometer

THE British Association has recently received from Miss Hooker, daughter of Sir Joseph Hooker, a gift of the aneroid barometer which used to hang by the fireplace in Charles Darwin's 'old study' at Down House, where it will now be replaced. A small repair has been kindly carried out by Messrs. Negretti and Zambra, and the aneroid is in working order. The face bears the words "Holosteric Barometer" and beneath them the figure 19, but no maker's name; the usual verbal indications of weather conditions are provided, and the dial is graduated continuously round the whole circle, figured from 25 in. up to 31 in., the remaining arc, between 31 and 25 in., being also graduated, as the whole scale is, in fiftieth parts of an inch. A small label, affixed perhaps when the instrument came into Hooker's possession, bears the name of "C. Darwin".

New Central Botanical Gardens for Moscow

BOTANICAL gardens, to be known as the Central Botanical Gardens of the U.S.S.R., will shortly be constructed on the southern and northern slopes of the Lenin Hills, on the outskirts of Moscow, according to the Soviet Union Year Book Press Service. The gardens will occupy a site of 825 acres, bounded by the projected Rublevskoye Chaussee, the Palace of Soviets, the Andreyev Canal and the Moscow River. A considerable part of the site will be used as a scientific research station. Special hothouses and structures will be put up in this section for the purpose. The middle of the Gardens will have a group of hothouses with varying artificial climates for the growing of plants from all over the world. A model orchard will be used for showing Michurin and other horticultural experiments. The Botanical Gardens will be attractively laid out with terraces, fountains and waterfalls. A funicular railway will connect the Moscow River Embankment with the top of the Lenin Hills. The Academy of Sciences of the U.S.S.R. has appointed a special commission to take charge of the designing and construction of the Gardens. The Commission includes V. L. Komarov, president of the Academy, Prof. N. I. Vavilov, Prof. B. A. Keller, A. M. Lezhav, director of the Central Department of Sub-Tropical Crops of the Commissariat of Agriculture of the U.S.S.R., and I. S. Gorshkov, director of the Michurin Central Laboratory of Genetics.

Future of the Coal Gas Industry

THE seventy-fourth annual meeting of the Institution of Gas Engineers was held on June 1–4, under the presidency of Mr. Stephen Lacey, of London. The papers contributed illustrated the problems and trends of the gas industry to-day—the tendency gathering in momentum on one hand to replace the consumption of raw coal by smokeless fuels and methods, and on the other to use fuels in a 'fluid' form amenable to ready and automatic control. The gas industry as a producer of smokeless fuels—both solid and fluid—is deeply involved in this trend, and the papers read retailed divergent opinions as to the future. A major problem of the industry is to maintain an economic balance between coke and gas. In Great Britain, gas engineers do not anticipate an early displacement of solid fuels; the industry must look for a combination of the two. Mr. C. F. Broadhead, of Melbourne, Australia, influenced by the acute instability of markets for by-products in that country, favoured concentration on complete gasification of coal. This idea, which is by no means new, is attracting renewed attention, and as Mr. Lacey said, is now the aim of an important branch of the Institution's research work. The uncertainty about the future of the supplies of suitable gas coal has stimulated interest in this problem. It has long been felt that the service of the gas industry would be more efficiently rendered if organized into larger units. Such an organization of the electrical industry has been imposed by law and a somewhat analogous process is now proceeding in the gas industry, but

voluntarily, and by the formation of corporations or holding companies. This process has evoked a mixed reception, and a paper by Mr. G. M. Gill records how the holding companies have already given technical and financial assistance to many small isolated companies, enabling them to pursue a more progressive policy.

A Meteorological Chronology

GEOPHYSICAL MEMOIR No. 70 of the Meteorological Office, by C. E. Britton, is entitled "A Meteorological Chronology to A.D. 1450". It is a symposium of various compilations of historical references to the weather of the British Isles, and a full bibliography of these is given at the end of the memoir. The main body of the work is in diary form, each year for which any notable meteorological event has been recorded appearing in turn, with particulars of such events. The first year is 2668 B.C., but it is pointed out that these very early dates, derived from such sources as the Irish Chronicles, are "largely legendary", being probably in the nature of oral traditions. The events chronicled in these years are chiefly the overflowing of various Irish lakes and rivers. The traditional period is followed by the Roman period, and the author points out that the very large number of meteorological events assigned to those times by various annalists and chroniclers do not appear to have been recorded by any contemporary writer; many indeed appear for the first time on record in the eighteenth century, with no information from the chronicler as to where the information was obtained. A typical specimen of this class of record is that of Dr. Thomas Short of Sheffield, who published a "General Chronological History of the Air, Weather, Seasons, Meteors, Etc." in two volumes in 1749—a work that occupied the worthy doctor for fifteen years. Without disputing the interest attaching to a work of the character of this symposium, one may doubt if its scientific value for meteorology is appreciable. It is amusing to notice the sobering of the record in its later years—the gradual dying out of such events as several days' downpour of blood, for example—with its suggestion that a very large part of the events before, say, A.D. 1000, are pure inventions or unrecognizable distortions of happenings not so very different from those of modern times.

A Theory of Atomic Structure

OUR attention has been directed to a theory of atomic structure which has been elaborated by Mr. C. F. Krafft, of Washington, D.C., and published by him in book form under the title "The Mechanistic Autonomy of Nature, with 1935 and 1936 Supplements". In this theory the atomic nucleus has been discarded in favour of a system of vortex rings arranged in the form of polygons, which may break up into chemically active groups and then fuse with other similar groups to form larger polygons. Thus the valency 'bonds' are made up of protons as well as electrons and the structural centre of the atom is apparently void. Whether this theory will survive the critical examination of the physicist is not certain, but the author claims that it enables him to interpret the chemical structures of compounds like diborane B_2H_6, etc., which are apparently anomalous. He asserts that "the very existence of diborane flatly disproves the entire nuclear theory of atomic structure, and not even its endorsement by high authorities can save it". In view of this bold statement, it is interesting to recall the fact that this very problem was brilliantly solved quite recently by Dr. E. Wiberg, an account of whose work on the subject was published in NATURE of February 27, p. 381.

Storage Battery Cars

RECENT progress in the design of storage batteries has turned the attention of traction engineers to developing storage battery cars for use on rails. These cars are very trustworthy in service and simple to operate. The maintenance costs of the equipment are much lower than when Diesel engines are used. Battery maintenance is usually carried out by the manufacturers on the basis of the number of miles run, and the cost depends on the current price of lead. The 'standstill' charges affect about ten per cent of the total stock in service, and due to battery overhauls they are usually out of commission for three or four days during the year. According to *Electrical Industries* of May 19, charging the batteries is very simple. It is carried out at night by connecting them to a power supply, and when fully charged the current is automatically switched off. At the terminal points of the system the whole battery can be replaced in a quarter of an hour. The distance that can be covered in a single battery charge is now 185 miles. Trials on the German State Railways show that some cars can travel for 250 miles non-stop at 25 m.p.h., and half that distance can be covered at 40 m.p.h. In the first case the power consumption is 18 watt hours per ton mile and in the second 26 watt hours per ton mile. At the present time, there are 170 cars on the German State Railways and their average speed is between 37 and 47 m.p.h. The average yearly car mileage during 1935 was 38,500. In Poland, France and the north of Italy, storage battery cars, mostly of the double bogie type, are run on tramways and suburban lines. Each car carries about a hundred passengers and has four motors giving a total of about 180 horse-power.

The National Physical Laboratory

THE annual report of the Laboratory for 1936 is presented in a new form. Hitherto it has been a quarto volume of about 250 pages, well illustrated and cost 12*s.*; now it is royal octavo of 144 pages without illustrations but costs only 2*s.* 6*d.* The saving of space has been secured by the omission of detail of limited interest. The death of Sir Joseph Petavel, the director, was referred to in the last report and until the appointment of a successor Sir Frank Smith was in charge. Several members of the staff have been transferred to other Government Departments or have been absorbed by industry. Many of the senior members have taken part in conferences at home and abroad and have lectured in provincial centres of industry on the work of the

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On the Origin of Laws of Nature

By Prof. E. A. Milne, M.B.E., F.R.S., University of Oxford

[In Nature of May 8, we published an article by Dr. Herbert Dingle entitled "Modern Aristotelianism". The article, as letters from a number of correspondents showed, created considerable interest, and it was decided to invite further contributions on the subject from a number of representative investigators. Prof. E. A. Milne, who was singled out by Dr. Dingle for particular criticism, has sent an article expounding the ideas behind his recent work and touching on many of the points under discussion. This serves as an opening article to a series of contributions, many of them sent in the form of "Letters to the Editor", which have been arranged roughly in the order of their content under the general title of "Physical Science and Philosophy". Finally, Dr. Dingle has summed up the discussion and dealt with some of the main points raised. It is hoped that a useful purpose will have been served by bringing together in this way a number of contributions bearing on the old question of the relation of physics to philosophy.—Editor, Nature.]

As the status of a recent series of writings by me has been called in question, it may be permissible to give some statement of their general tenour. The following account contains nothing that is not already explicit or implicit in the original investigations, and will be superfluous to those who have followed the details.

The general object of the investigations[1] in question was to determine the consequences of the assumption that the universe is, on the average, homogeneous. Had I used this as an ordinary physical hypothesis, that is, used it in conjunction with the existing body of physical doctrine and laws of Nature, the investigations would probably have passed without challenge. But I did not so use it. By an extreme application of the principle of the economy of thought, I investigated the consequences of this assumption without appealing to any empirical quantitative 'laws of Nature' whatever. I used it as a sole basis of argument. I did so because it early appeared that very much more could be deduced from it than was commonly recognized.* At first, indeed, I used it in conjunction with well-known kinematic laws developed in the so-called 'special' theory of relativity. Hence the provisional name 'kinematic relativity'. But it soon appeared that these kinematic laws were themselves the first deductions from the basic assumption, or from allied but simpler considerations in kinematics. From October 1933, when I first gave in seminars in Oxford the substance of Chap. ii of "World-Structure", I have endeavoured to develop the consequences without any empirical appeal save to the existence of a temporal experience, an awareness of a before-and-after relation, for each individual observer. Such an appeal, as has been emphasized throughout, is necessary before *motion* can be described at all. This is well understood from the works of A. A. Robb, and it has been evident from the time of Zeno. The exclusion of all empirical appeals was not complete in my own writings, but it has been further completed in papers by G. J. Whitrow[2], A. G. Walker[3], H. P. Robertson[4], and others. Some similar work has been developed by L. Page[5], and further work on the meaning of uniform time is in preparation by me in collaboration with Whitrow.

The hypothesis is that the universe is homogeneous on the average in both distribution and motion, both of these being quantitatively measurable, in principle, by any observer by means of measures of angle and

*It is fundamental because it enumerates a set of equivalent frames of reference.

temporal experience alone. The single measuring instruments allowed are the clock and theodolite, and it can be shown to be possible for clock graduations to be chosen so that the motion appears to be uniform to all of a class of observers, this mode of clock-graduation constituting the definition of the measure of 'uniform kinematic time t'. The indefinable concept of the rigid transportable measuring-rod is avoided, and the vague 'pointer-readings' of current relativity are made explicit. Throughout, full attention has been paid to the theory of knowledge—to Einstein's principle of introducing only elements which can in principle be observed.

It is an astonishing thing that the elimination of other empirical appeals, including all appeals to quantitative laws of physics, can be carried out as far as it can, however imperfect the present state of the theory. No one has been more astonished than the present writer. It is not an *a priori* belief to be scoffed at; it is a fact of experience to be reckoned with, that when we do thus eliminate such empirical appeals, regularities emerge (as logical consequences of the hypothesis) which play the part of the very laws of Nature which are *observed* to hold good. These regularities have the logical status of *theorems*, and the resulting logical structure has the status (or would have the status if it were perfect) of an abstract geometry based on axioms. It will be well therefore for a moment to consider what is meant by a geometry.

A geometry is a body of deductive inferences from axiomatic definitions which in effect afford the minimal descriptions of the subjects of discourse. The axioms of a geometry are essentially a complete set of definitions which delimit what we are talking about. The elements in the geometry—point, line, etc.—are never explicitly otherwise defined than as entities occurring in certain statements of relations. A geometer never says what a point *is*. Instead he makes a constructive statement about a point—he calls it an axiom—which affords a basis for inference. The logical consequences of the axioms constitute the theorems, and no 'fact' is appealed to in proving a theorem unless it has already occurred in an axiom or earlier theorem. This is what is meant by saying that geometry is an exact science—it makes no use of the principle of induction—it appeals to no empirical generalizations based on experience. The choice of axioms may be guided by suggestions derived from experience, but no recourse to experience is made in establishing the theorems. They depend for their validity on the self-consistency of the axioms and the freedom of the deductive steps from logical errors.

In this way systems are constructed which may or may not have their counterparts in the external world. We then appeal to observation to verify the existence, or establish the non-existence, of entities in Nature corresponding to the entities mentioned in the axioms—exactly or approximately as the case may be. For example, we can identify a given two-spread as a Euclidean plane by verifying (to a certain approximation) that the theorems of Euclidean geometry hold good in it; alternatively the two-spread may be such that the theorems of non-Euclidean geometry hold good in it. But in either case it is not that the theorems depend for their validity on recourse to observation or on any application of the principle of induction. It is their relevance to a particular state of affairs which can be so established. The role of observation is a double one: it is to discover theorems empirically, but it is also to verify the relevance of theorems discovered by inference from axioms, and hence to identify in Nature objects corresponding to, or approximately realizing, the entities mentioned in the axioms. The theorems exist in their own right. Thus when we say that geometry is an exact science, we are paying attention to one aspect—the deductive; we may equally say that it is an empirical science, namely, when we pay attention to another aspect—its relevance to Nature. In early Egyptian days, we understand, geometry was purely an empirical science; the Greeks carried it, though not quite perfectly, to its deductive phase.

Consider now physics. It has clearly an Egyptian aspect and a Greek aspect. The trend of physical investigation is at once to extend the number of known facts by empirical discovery, and to diminish the number of independent facts by establishing some as consequences of others. To the extent to which physics reposes on empirical laws of Nature not deduced from other laws—on 'brute facts'* capable of no further explanation—it is irrational and inexact; irrational because no reason can be assigned for the existence of these irreducible laws of Nature, and inexact because they can only be known to hold approximately, through the principle of induction. But we can contemplate the possibility of the completion of the second of the two trends mentioned, by reduction of the number of brute facts to the minimal description of what is the subject of discourse. Physics would then attain the status of a geometry. The minimal description, the relations satisfied by what *is*, would constitute the axioms. Theorems in the resulting logical structure would play the part of laws of Nature, and the validity of the theorems would depend, as in a geometry, on the self-consistency of the axioms and the correctness of the logical reasoning. The laws would exist not as regularities imposed from without but as a consequence of what *is*. The role of observation would again be to attempt to verify in Nature the existence of entities corresponding to those mentioned in the axioms or any afterwards constructed, by ascertaining whether the resulting theorems hold good in the external world. The relevance of the theorems to Nature would require to be established by observation, but these theorems or laws of Nature would hold good in their own right, as the inevitable consequences of our having said exactly what we were talking about.

Just as geometers have constructed systems and logical entities not realizable in Nature—like a four-dimensional cube—so here the resulting logical structure may not correspond to Nature. But just as results of great value have followed from the construction of non-realizable abstract geometries,

* It is not known whether this phrase is an insult to the facts or a compliment to the brutes.

so it is valuable to construct an abstract physics for its own sake. It is remarkable that those who are most prone to criticize my procedure on the ground that it is 'mere logic' immediately avail themselves of the results of 'mere logic' by adopting non-Euclidean geometries.

In the deductive structure raised, the substratum or smoothed-out homogeneous universe plays the same part as the plane in Euclidean geometry. It is the platform on which, or the background against which, the entities constructed have the properties enunciated in the theorems. Instead of properties of patterns of points, we have properties of sets of fundamental particles. The difference from geometry is that the platform is a moving one (in some of its modes of description). We have superposed *motion* on geometry. Like the plane or any surface of constant curvature in geometry, the substratum is of the same nature everywhere. It is a remarkable fact that the theorems to which one is led cover the successive provinces of kinematics, dynamics, gravitation and (in preparation) electrodynamics, and agree in general outline with the empirically discovered laws of Nature concerning these provinces. Whether the structure in all its details is 'realized' in Nature is an interesting and important question, but it is more interesting that the procedure is possible and at least partially successful. Nature *is* very like the abstract structure. An example of identification by appeal to observation is the identification of kinematic time in terms of dynamical time by comparison of the laws of dynamics obtained deductively with the empirical Newtonian dynamics.

This throws a flood of light on the problem of the 'origin' of laws of Nature. Empirical physics by itself cannot attack this problem. A residue of laws of Nature, not otherwise explained, would partake of the character of magic. To believe in this would be a form of superstition. It would mean that there were two creations, one of the matter of the world, another of a set of laws for the world to obey. The alternative belief is that the universe is rational. By this I mean that given the mere statement of *what is*, the laws obeyed can be deduced by a process of inference. There would then be not two creations but one, and we should be left only with the supreme irrationality of creation itself, in Whitehead's phrase. We can only test this belief by an act of renunciation, by exploring the possibility of deducing from some assumed description of just *what is* the laws which *what is* obeys, avoiding so far as possible all appeal to empirically ascertained laws. Laws of Nature would then be no more arbitrary than geometrical theorems. God's creation would be subject to laws not at God's further disposal. The laws would be consequences of the world-shape.

As for the basic assumption itself, that the world is homogeneous, it is perfectly open for anyone to make a different assumption (for example, that it is not homogeneous, but stratified round a particular centre) and repeat the procedure. Either the laws of Nature would come out different—in which case we should have to attribute new effects to distant unobservable portions of the universe ; or they would come out the same—in which case the original axioms would have been shown to be unduly restrictive, but the deductive character of the laws would be more secure still. Progress in this direction has been made by A. G. Walker, in repeating the procedure for a Riemannian space. But it was surely justifiable, in so unconventional a programme as the logical deduction of laws of Nature, first to explore the possibilities in the simple contexts of homogeneity and Euclidean private spaces. Through an unmapped region, one path must first be trodden.

The work was first criticized as being solely kinematical. Now that it has been developed so as to construct a dynamics and account of gravitation, critics appear to resent the fact that laws of dynamics come out as deductions. The individual investigator must be left in peace to state his own problems and solve them by his own methods, and criticism of these on the ground that they go beyond Renaissance science is merely a form of authoritarianism.

One further point may be mentioned. The procedure we have outlined is essentially different from that of general relativity. General relativity, on a basis of empirical experience, proposes certain laws of Nature, but is still, without further assumption, unable to say what are the contents of the world which is to obey them. In almost all its treatments of the cosmological problem, general relativity goes on to make the same assumption that I make, that the universe is on the average homogeneous. The assumption was first made by Einstein. But it now follows that general relativity makes redundant assumptions. For the assumption of homogeneity itself implies the laws that will be followed. It may be that the laws adopted in general relativity coincide with those that can be deduced from the assumption of homogeneity. If so, general relativity is fortunate. Actually investigations given elsewhere throw considerable doubt on the "field equations" of relativity, partly because the latter lead, if pushed to extremes, to inadmissible consequences. But the important point is that it is not legitimate *both* to assume a homogeneous universe *and* to assume laws for it to obey. Laws can be deduced from homogeneity, but the procedure is not reversible.

It may remove misapprehensions to remark that the actual discreteness or local inhomogeneity of the world in detail leads to no difficulties. Technical methods have now been developed for superposing on general homogeneity local variations, and for dealing with such systems as a spiral nebula.

[1] "Relativity, Gravitation and World Structure" (1935) ; "On the Foundations of Dynamics", *Proc. Roy. Soc.*, **154**A, 22 (1936) ; "The Inverse Square Law of Gravitation", *Proc. Roy. Soc.*, **156**A, 62 (1936) and **160**A, 1 (1937) ; " Kinematics, Dynamics and the Scale of Time", *Proc. Roy . Soc.*, **158**A, 324 (1937) ; **159**A, 171 and 528 (1937) ; "The Acceleration-formula for a Substratum and the Principle of Inertia", *Quart. J. Math. (Oxford)*, **8**, 22 (1937) ; See also my review of Bridgman's "Nature of Physical Theory", *Math. Gaz.*, p. 340 (Dec. 1936).

[2] Whitrow, G. J., "On Equivalent Observers", *Q.J.M.(Ox.)*, **6**, 249 (1935) ; "World Structure and the Sample Principle", *Z. Astrophys.*, **12**, 47 and **13**, 113 (1936–7) ; "Photons, Energy and Red-shifts on the Spectra of Nebulæ", *Q.J.M.(Ox.)*, **7**, 271 (1936–7).

[3] Walker, A. G., "On Milne's Theory of World-Structure", *Proc. Lond. Math. Soc.*, **42**, 90 (1937) ; Also *M.N.R.A.S.*, **95**, 263 (1935).

[4] Robertson, H. P., "Kinematics and World-Structure", *Astrophys. J.*, **82**, 284 (1935) ; **83**, 187 (1936) ; **83**, 257 (1936).

[5] Page, L., *Phys. Rev.*, **49**, 254 and 466 (1936).

Physical Science and Philosophy

Sir Arthur Eddington, F.R.S., Observatory, Cambridge

In his entertaining article in NATURE of May 8, Dr. Dingle inveighs against a new departure in scientific method, which has grown out of the revolution of thought provoked by relativity theory. For my own part, I will not quarrel with the label 'Aristotelian'. Whether or not my view has anything in common with that of Aristotle, it presents a definite contrast to the 'Galilean' view; and I feel great satisfaction at having shocked a die-hard of the latter school.

But I think Dingle is wrong in identifying the Aristotelians with those whose deductions rest on a supposed uniformity of the universe. (I have toned down his rhetoric a little.) A conspicuous example is Dr. Hubble's deduction that the red-shift in nebular spectra is due to some cause other than velocity. But I would not class Hubble as an Aristotelian; he is more usually held up as a model of orthodoxy to errant theoretical cosmogonists. For my part, I have made a point of exposing a number of fallacies, current at one time, which arose from applying the uniform model to actual conditions without taking account of the irregularity of the universe. The distribution of the galaxies, like the distribution of the planets, comes under the heading "particular events and objects of our experience" which, according to my statement partially quoted by Dingle[1], cannot be deduced from epistemological considerations.

A doctrine is not to be judged by the follies that have been committed in its name. If Kepler concluded by Aristotelian sophistry that there can be only five planets besides the earth, Bode obtained by Galilean induction his law of planetary distances. The planet Neptune contradicts them both impartially. I can only accept responsibility for the "new Aristotelianism" in the form in which it has been promulgated and applied in my own writings. I here offer a short explanation and defence of this form.

In physical science *a priori* conclusions have long been anathema. It has come to be accepted as a scientific principle that we can have no *a priori* knowledge of the universe. Agreed; provided that by 'universe' is here meant an *objective* universe, as was clearly intended when the principle was framed. But I have calculated, for example, the mass-ratio of the proton and electron by an *a priori* method which does not involve any observational measurements. For present purposes we may assume that the calculation is sound, since criticism has not been directed against any particular step in the argument. What follows? We are given—

(*a*) It is impossible to have *a priori* knowledge of an objective universe.

(*b*) The mass-ratio has been found by an *a priori* method.

Therefore: *Knowledge of the mass-ratio is not knowledge of an objective universe.*

After a rather extensive series of researches, I have found that a great part of the current scheme of physics is deducible by *a priori* argument and therefore does not constitute knowledge of an objective universe. When the constant, which I had regarded as the last stronghold of objectivity, namely "the number of particles in the universe", surrendered, I could no longer resist the conclusion: The general laws of Nature (embodied in the fundamental equations of mathematical physics), including the universal constants associated with them, do not express knowledge of an objective universe. If anyone points out that in that case they are not *really* laws of Nature or natural constants, I do not object; but I continue to use the designation applied to them in current physics. Observation, besides revealing laws of Nature, supplies details of particular systems and events. It is in these particulars that the objective element in our knowledge resides; there is no objective element in the general laws.

One consequence of the new outlook, which, I believe, rouses the passions of the Galileans, is that a law of Nature has a compulsory character, and is not merely an empirical regularity which we have hitherto found to be fulfilled in our limited experience. Conflict with the laws of Nature is *impossible*. "Impossible" you may say "is a word which no genuine scientist should use". Very well; let us substitute another—which makes the laws no less compulsory. Conflict with the laws of Nature is *unobservable*. Physical knowledge, as ordinarily expressed, is not stated in terms of observables, and requires translation in order to get at its observational content. Carrying the translation into the language of observables further than it is usually carried in relativity theory and quantum theory, it appears that the supposed deviation would have no observational equivalent. It is like trying to find the English equivalent of a misspelt German word.

In the theory of relativity it was realized for the first time that induction from observation should be based on what the observer observes, not on what he alleges he observes. This elimination of metaphysical speculation and return to the "hard facts of observation", besides purifying and extending Galilean induction, was also the origin of the Aristotelian divagation. Attention was directed, not only to the result of the measurement (*a posteriori* information), but also to the nature of the process of measurement (*a priori* information). We cannot, of course, predict the result of the measurement from our *a priori* knowledge of how the measurement is going to be made; but it does not seem unlikely that we should

be able to predict certain general properties that the resulting measurements will have. A valuer may arrive at the generalization *a posteriori* that no article in a certain house is worth more than sixpence ; the same conclusion might have been reached *a priori* by noticing that the owner furnished it from Woolworth's. The observer is called upon to supply the furniture of the mansion of physical knowledge. The Aristotelian by watching his method of obtaining the furniture may anticipate some of the conclusions which the Galilean reaches by inspecting the furniture.

Consider an example. A measurement consists in comparing a relation between two entities with a relation of the same kind between two other entities, the latter relation being treated as 'standard'. It therefore concerns four entities in all. Without more investigation we are led to expect that this number four will in some way make itself evident in the world-picture which embodies the results of observational measurement. This expectation in itself tells us practically nothing* ; many threads must be woven together before a pattern appears. But the example will serve to show where I get the straw for the bricks which I make—or drop—in my theory.

A more abundant source of *a priori* material is the fact that our inductions are based, not on observations generally, but on "good" observations, that is, observations made in (or reduced to) conventionally defined conditions. It must be emphasized that (according to Dr. Dingle) these conventions are entirely arbitrary. Faithful Galileans, who have no *a priori* theory of Nature, cannot speak of one convention being more 'natural' than another.

As a further example I may as well take what has probably been regarded as the most glaring application of Aristotelianism, namely, the result that the number of particles in the universe, N, is precisely $2.136.2^{256}$. I have not forgotten Kepler's fiasco with the number of the planets ; but in attempting to out-Kepler Kepler, we have the advantage of three hundred years' progress. The number N is one of the constants of physics, ascertainable approximately by Galilean methods. I describe it as it is described in current physics ; but, of course, if I show that it can also be calculated *a priori*, it cannot represent knowledge of an objective universe and therefore cannot be an arithmetical count of objective particles.

Quantum physicists tell us that a particle (say, an electron) is not normally in a definite place but is smeared over a probability distribution ; also that electrons are not distinguishable from one another. That does not seem very promising material for counting. (I would suggest to quantum physicists who suffer from insomnia that, instead of counting sheep in a green field, they should try counting electrons in a probability distribution.) Nevertheless, quantum physicists tell us confidently how many electrons there are in a gram of hydrogen. Ten years ago I wrote with enthusiasm about Dirac's delightful dodge, which turns arithmetic into wave mechanics by means of a wave function whose eigenvalues are the integers—at the same time emphasizing that it was a dodge[2]. By this method the wave function, which describes a system physically, is made to supply an arithmetic for reckoning the number of its particles. When the quantum physicist states the number of particles in a system, whether few or many, he gives the number reckoned by quantum arithmetic. The natural constant N is a number in quantum arithmetic ; it could have no other meaning, for Pythagorean arithmetic is a non-starter in the competition. When further we introduce the condition that the wave function of the universe is relativistic, that is, that it is the embodiment of information that has been obtained by observational measurements, we find that in the corresponding arithmetic the integers run only from 1 to $2.136.2^{256}$. This upper limit may be said to replace infinity in relativistic quantum arithmetic in the same sense in which the velocity of light replaces infinite velocity in relativistic kinematics. We can thus obtain the number of "all the particles that there are" from our *a priori* knowledge of the arithmetic that is used for enumerating them. From the philosophical point of view we have 'debunked' N ; but here we must stress rather the fact that the Aristotelian method anticipates information, which when obtained *a posteriori* by Galilean induction is counted as a valuable addition to knowledge.

Finally, since the statement of my philosophy[1] quoted without its safeguards by Dingle has also been quoted by almost all the reviewers of my book, I would like to make it clear that it is not the basis but the unexpected conclusion of the investigation.

* In particular, it does not warrant numerological speculations. I think the number four introduced in this way is responsible for the four dimensions of space-time, *but only indirectly*. In the actual calculation the number of dimensions of space-time is reached by the route $\frac{4.3}{1.2}-1-1=4$.

[1] "Relativity Theory of Protons and Electrons," p. 327.
[2] "The Nature of the Physical World", p. 219 ('Everyman' edition, p. 216).

Prof. P. A. M. Dirac, F.R.S., St. John's College, Cambridge

In his article in NATURE of May 8, Dr. H. Dingle criticizes my letter to NATURE of February 20, on the grounds that it departs from the Galilean scientific method of building up theory to fit observations. The successful development of science requires a proper balance to be maintained between the method of building up from observations and the method of deducing by pure reasoning from speculative assumptions, and I think my letter satisfies this condition; but as it was written rather concisely, I would like here to restate the main points of the argument in a form which brings its observational basis to the forefront, and is essentially independent of cosmological theory.

We begin by taking all the constants of Nature provided by observation, comprising the atomic constants and some astronomical ones, namely, the

gravitational constant, the red-shift of the spiral nebulæ per unit distance, and the average density of matter. (The last has a definite meaning on account of the fact that the spiral nebulæ are distributed with uniform density, as far as can be seen with the largest telescopes.) From these constants of Nature we construct all the simpler dimensionless numbers that we can, and make them all greater than unity by taking the reciprocals of those that are less. A remarkable result now comes to light. The numbers we obtain, instead of being scattered about in a random way, are distributed in clusters, there being one cluster of small numbers (the largest of these being the ratio of the mass of the proton to that of the electron), a second cluster of numbers in the neighbourhood of 10^{39}, and a third cluster of numbers in the neighbourhood of 10^{78}.

From kinematical considerations based on the assumption that the red-shift phenomenon appears the same when observed from different spiral nebulæ (a reasonable assumption in view of the uniformity in the distribution of the spiral nebulæ), one can deduce that the red-shift per unit distance is not constant in time, but is slowly decreasing, at the rate of about one part in 2×10^9 per annum. Milne's work provides a detailed treatment of this question. One may adopt a picture using the recession explanation of the red-shift, when the red-shift per unit distance will be slowly decreasing because of the increasing distance of the nebulæ. (But one is not obliged to adopt this picture, there being another mathematically equivalent picture without recession, based on Milne's time variable γ.)

The red-shift per unit distance has the dimensions of $\delta\nu/\nu l$, that is, $(\text{distance})^{-1}$. To get a dimensionless number from it, we must multiply it by a constant of Nature with the dimensions of distance, the simplest of these being h/mc, e^2/mc^2, and the corresponding expressions with the proton mass replacing the electron mass. The numbers so obtained are all less than unity and their reciprocals are in the 10^{39} cluster. These reciprocals must be slowly increasing in time, on account of the decrease in the red-shift per unit distance.

I now make the hypothesis that the clustering of the dimensionless numbers is a fundamental natural phenomenon, which will hold for all time. This requires that, since some of the numbers in the 10^{39} cluster are increasing, they must all be increasing in the same ratio, and the numbers in the 10^{78} cluster must be increasing according to the square of this ratio. This is a tentative hypothesis, which one may hope will prove susceptible to experimental verification in its developments, though any direct test would be very difficult on account of the enormous time-scale involved.

S. Chandrasekhar, in his letter to NATURE of May 1, makes an interesting contribution which, in the present terminology, would run as follows: if one brings the average mass of a star and of a spiral nebula into the scheme, one gets further clusters of numbers in the neighbourhood of the $1\frac{1}{2}$ and $1\frac{3}{4}$ powers of 10^{39}. From the foregoing ideas one would expect these to increase at the rate of the $1\frac{1}{2}$ and $1\frac{3}{4}$ powers respectively of the rate of increase of the 10^{39} cluster. This would enable one to calculate the rate of increase in the average mass of a star and of a spiral nebula, and hence also the rate of increase in the average number of stars in a spiral nebula.

Prof. W. H. McCrea, Queen's University, Belfast

DR. H. DINGLE'S objection to "modern Aristotelianism" seems to be itself what he would call Aristotelian rather than Galilean. For a truly Galilean point of view would be to observe whether Eddington, or Milne, or anyone else, can in fact deduce properties of the physical world from a knowledge of "the system of thought by which the human mind interprets to itself the content of its sensory experience". Should it turn out that such a deduction is possible, the fact would be a new and exceedingly important experimental result, throwing fresh light upon the relation of the "physical world" to the human mind, and worthy of the attention of all true Galileans. On the other hand, to say that, "Nature nothing careth whether her abstruse reasons and methods of operating be, or be not, exposed to the Capacity of Men," is to assert something about Nature which is meaningless in regard to the experimental observation of Nature; by definition, man can experiment or theorize only about that which *is* exposed to the capacity of men—perhaps a trivial remark.

What Dr. Dingle has done is to reopen the question of the relation of mathematical physics to experimental physics, since he claims to detect a new and perverted point of view in the former. Now a system of mathematical physics, apart from the alleged perversion, is the working out of the mathematical consequences of certain hypotheses. The worth of the theory is judged on one hand by the fewness and simplicity of its hypotheses, and on the other by the closeness of the agreement of its predictions with the results of observation, and also the number of phenomena which it can so predict from the one set of hypotheses. Incidentally, some misunderstanding seems to arise because, for mathematical convenience, the hypotheses are generally stated as axioms, postulates, or 'laws', since the purely mathematical part of the theory is not concerned with their truth, in any sense, but only with their mutual consistency. Newton's laws of motion, and the principle of relativity, are examples of the hypotheses of two such theories.

Mathematical physics advances then, in one direction, by reducing the number of its hypotheses. This is well shown by the examples quoted by Dingle. Newton's theory was based on the hypothesis of his laws of motion and *also* upon implicit hypotheses about space and time. General relativity replaced this dual set of hypotheses by a single set. There is now "no such thing as a body in space, but matter is an aspect of the space-time structure" (Veblen and

Whitehead). Also the number and accuracy of the physical predictions were thereby increased. Milne next gives us a theory with still simpler and more easily apprehended hypotheses framed in terms of the equivalence of observers. The emphasis is on this equivalence, rather than on the cosmological principle as a property of the universe, as suggested by Dingle. Also, once again the number of phenomena brought within the scope of the theory has been increased. I think it is fair, in the present state of the theory, to class Milne's equivalences of observers (there are two kinds of equivalence specified) with Newton's laws, or the principle of relativity, as basic hypotheses of a system of mathematical physics; then this system falls naturally into place in the historical development, without being guilty of the type of heresy suspected by Dingle.

However, there is one significant feature in Milne's theory which must not be overlooked. The constancy of the velocity of light was a hypothesis in some formulations of special relativity theory. In Milne's theory it becomes a *convention*. This possibility, that a hypothesis might be discovered to be a convention, or that the theory might be so readjusted that it becomes a convention, was clearly shown by Poincaré in "La Science et l'Hypothèse". There are actually other examples as well in Milne's work.

This leads us on to inquire how far back this process of reducing hypotheses may be pushed. Now there is one set of 'hypotheses' implicit in all these theories. They all inevitably postulate or spring from the processes of human thought. Is it then possible that we may dispense with all other hypotheses, that is, that all other hypotheses will be revealed as conventions of thought or the expression of thought? Eddington's theory, presented in the "Relativity Theory of Protons and Electrons", and referred to by Dingle, may be regarded as, in fact, an effort to do this. The scientific attitude is, not to cavil at the attempt, but to see if it is successful. The theory is still too new for a consensus of scientific opinion to have been expressed; it is the purpose of this note merely to suggest that it is in harmony with the historic evolution of mathematical physics*. I fear, however, that I may have intruded into a region where angels might well fear to tread.

* Some of these ideas were developed in an inaugural lecture on "Mathematics and Cosmology", before the Queen's University on October 25, 1936.

Prof J. B. S. Haldane, F.R.S., University College, London

Dr. Dingle's spirited attack on Prof. Milne and Sir Arthur Eddington, in Nature of May 8, will inevitably excite some sympathy among workers in sciences such as biology, where chains of reasoning are shorter than in cosmology. If Milne really attempted "to derive the laws of dynamics rationally —without recourse to experience", he would perhaps merit the title of traitor which Dingle awards him. But in actual fact his cosmological principle is based on the hypothesis that observers on the earth are not unique, and that observers on other stars would form a world-picture similar to our own. Now this hypothesis would have appeared fantastic to Aristotle, Ptolemy or St. Thomas. It is based on the deduction from astronomical observations that our own sun is one of a very large number of stars in one of a very large number of galaxies. The fact that it can be used as a basis for dynamics is remarkable, but Milne certainly did not frame it "without recourse to experience".

Similarly Eddington states that "An intelligence, unacquainted with our universe, but acquainted with the system of thought by which the human mind interprets to itself the content of sensory experience, should be able to attain all the knowledge of physics that we have attained by experiment". May be, but the system of thought in question happens to be determined, at least to a considerable extent, by the sensory experience. If Sir Arthur Eddington's entire sensory experience had been confined, say, to listening to the music of Bach, I venture to suggest that his system of thought would have been different. Actually this system is a highly specialized social product. I doubt whether the system of thought employed by the Australian aborigines to interpret their sensory experience would be so sound a basis for theoretical physics as that current in Cambridge.

However this may be, I trust that Dr. Dingle's article will not prevent biologists and geologists from studying Milne's cosmology, which, if accepted as a working hypothesis, will profoundly affect their sciences. For example, kinematical time, which is finite in the past, appears to be appropriate for describing chemical events, whilst many physical phenomena, such as the earth's rotation, proceed uniformly on a dynamical time scale which is infinite in the past, and corresponds to the logarithm of the kinematical scale.

Now many of the properties of matter are relations between chemical and physical events. For example, a given mass of a particular mixture at a certain temperature and pressure generates so many ergs per second. Clearly such properties of matter must change with time if Milne is correct; and $1{\cdot}5 \times 10^9$ years ago on the kinematic scale they may well have been sufficiently different to make life as we know it impossible. The conversion of chemical into kinetic energy is an essential feature of animal and even plant life; and in terms of our ordinary dynamical units this was, according to Milne, less rapid in the remote past.

There was, in fact, a moment when life of any sort first became possible, and the higher forms of life may only have become possible at a later date. If this is accepted, it cannot but alter our conceptions of evolution. Similarly a change in the properties of matter may account for some of the peculiarities of pre-Cambrian geology.

It is to be hoped that Prof. Milne and his colleagues will work out the secular changes in properties of matter such as density, rigidity, viscosity, surface

tension, conductivity and the like, which may be expected to follow from his cosmological principles. If the results serve to illuminate the history of geological and organic evolution, we shall be kept too busy to find much time to blame him for a perhaps unduly idealistic account of their origin.

Dr. Harold Jeffreys, F.R.S., St. John's College, Cambridge

Dr. Dingle's article in Nature of May 8 seems to me an excellent statement of the position, and I am in full agreement with it. It is curious that advocates of the views criticized in the article usually refer to Mach, a large part of whose work was devoted to attacking the 'inherent necessity' view of causality and to showing how the laws of mechanics, then and usually still taught as a deductive science, were derived from experimental considerations. The principle of the rejection of unobservables, which is a practical working rule of induction, has now become misapplied, so that anything that an author does not wish to talk about can be disposed of by being called 'unobservable', however clearly observable it may be; and 'unobserved so far' and 'unobservable' are taken to mean the same thing, thus denying that there can be any knowledge that we have not already.

Milne[1] remarks that "the laws of dynamics are exact and simple, and hold good, because they are logical inferences from axiomatic definitions, obtained by a fairly short deductive train of thought. It is thus no longer necessary to appeal, as a warrant for a belief in the 'truth' of these laws of nature, to the so-called 'principle of induction', which has never been satisfactorily stated, much less demonstrated . . . observation vindicates, not the truth of the theorems . . . but the applicability of the definitions to the things occurring in nature".

We may admit the legitimacy of giving definitions and axioms as intuitive suggestions, working out the consequences deductively, and then testing the results by comparison with observation. But, as Whitehead and Russell point out, a definition depends for its validity on the existence of the thing defined, and therefore requires either a postulate or an existence theorem. It is perfectly possible to write down a definition of a plane equilateral right-angled triangle, but that is not the end of the matter. Where the existence has to be tested by experience the postulate depends on experience for its verification; and without a principle of induction there is no reason to suppose that it is true in any instances beyond those yet discovered. The fact that the principle of induction cannot be demonstrated from the axioms of logic has been known for centuries; but that merely shows that additional axioms are necessary. The axioms of logic also cannot be demonstrated; the great achievement of Whitehead and Russell was to reduce their number. Without using induction, Milne and Eddington could not order their lives for a day, and what they are really asserting is that they are entitled to use special axioms in physics, for which no need has been shown.

As for the statement of the extra postulates needed for a theory of induction, the essentials are better stated in Karl Pearson's "Grammar of Science" than in any other work known to me. Following on Pearson's lines, I think that I have given a set of simple postulates that will enable any type of observational data to be assimilated into the theory, without any need to assume that different fundamental principles of learning are needed in different subjects[2]. Other systems have been given by Prof. R. A. Fisher and other statisticians. Enough has been said already about the differences between these. The important point at the moment is that they are all satisfactory in the senses that (1) they do not treat any hypothesis as *a priori* certain; (2) they provide methods of choosing between hypotheses by means of observations; (3) they give estimates of the parameters involved in any hypothesis consistent with the observations; (4) they recognize that decisions made by them will sometimes be wrong, and give estimates of, alternatively, what degree of confidence we can reasonably attach to a decision, or how often, supposing that we use the rules regularly, we may expect the decisions to be right; (5) they all contain provision for correcting wrong decisions when observations become capable of giving further tests, thus recognizing that scientific progress is by successive approximation.

I should expect the decisions by my methods to lead to the correct decisions most rapidly, because the method contains more explicit provision for allowing for the whole of the data; but many rules given by Fisher, and others accepted by him, are of exactly the same form as mine and would in practice be used in the same way, while in other cases where there are differences the actual limits recommended are such that it would be extremely rarely that the decisions would differ in any specific application, and then we should both be doubtful. I should much prefer Fisher's system to any that asserts that only one hypothesis is to be considered at all, and, apparently, that a subject is meaningless until its laws have been predicted in terms of this hypothesis.

In one respect the position is even worse than is stated in Dr. Dingle's article. An author's preference for what he believes to be a deductive system may lead him to omit arguments that tend in favour of his main thesis. Einstein's theory was not the first explanation of the excess motion of the perihelion of Mercury. A completely satisfactory theory, as it then appeared, of the motion had already been given by Seeliger, attributing it to the attraction of the zodiacal matter. This accounted also for the excess motion of the node of Venus, which is 3·5 times its standard error, and was not explained by Einstein's theory. A larger random error would be expected to occur about once in 2,500 times; even allowing for the fact that the residual is selected as the extreme one out of 15 we should still not expect it more than

once in 170 times. Newcomb had such confidence in this determination that he proposed to use it to determine the solar parallax dynamically. Yet writers on relativity all say that the residuals are perfectly satisfactory. De Sitter's last reference to the subject[3] gives a recalculation, which increases the residual, but he calls Newcomb's standard errors probable errors. Enough extra matter to account for the node of Venus would put the perihelion of Mercury hopelessly wrong. It might just possibly be a random error, but it was not legitimate to assert this until the alternative of additional matter near the sun was disposed of, particularly as such matter was seen. The question was whether there was enough of it. Similarly the refraction of a gas near the sun could account for the eclipse displacement of star images. Being prepared to take Einstein's theory seriously, but not being satisfied with the purely *a priori* arguments given for it, I obtained estimates of the quantity of matter present from its observed luminosity, and found that it would not account for 1/100 of the observed effects[4]. Consequently Einstein's theory was the only one suggested that was in accordance with the facts.

Now the remarkable thing is that, so far as I am aware, no exponent of relativity has ever mentioned this work or any improvement on it, the possibility of which I fully admit. It is apparently considered satisfactory to give a purely deductive theory from premises that are capable of being questioned, leaving with only a passing mention a residual with a natural alternative explanation that would not ordinarily be exceeded more than once in 170 times; while the fact that the theory is the *only* suggested one that is observationally tenable is not thought worth mentioning. Or is it not mentioned *because* mention of it would admit that the postulates of the theory are not *a priori* certain? In any case mention of the matter would make the theory more convincing to those who are not dazzled by the mathematics.

Milne similarly is trying to combine the phenomena of light and gravitation into one system, but the only observed facts that appear to involve the velocity of light and the constant of gravitation together, the three Einstein crucial tests, are postponed for treatment to an indefinite date as minor effects. As most of his work rests on the hypothesis of the universal applicability of the Lorentz transformation, I should expect it to lead to the same predictions as Silberstein's, which *a priori* seemed as reasonable as Einstein's, but made the perihelion of Mercury go round the wrong way and had to be dropped when there appeared to be no reasonable possibility of enough extra matter to put it right[5]. At all events, there is still no reason to believe that Milne's theory will lead to the correct predictions.

I think the source of the trouble is the belief that there is some special virtue in mathematics. Instead of being regarded as what it is, a tool for dealing with arguments too complicated to be presented without it, it has become emotionalized to such an extent that many people think that nothing but mathematics has any meaning; whereas the opinion of some of the best pure mathematicians is that the characteristic feature of mathematics itself is that it has no meaning. Russell has described mathematics as "a subject where we do not know what we are talking about, or whether what we are saying is true". Its function in science is to connect postulates with observations. The utility of deduction is that in investigating the consequences of a well-supported law it is a convenient approximation to induction.

[1] *Proc. Roy. Soc.*, A, **158**, 326 (1937).
[2] "Scientific Inference", 1937, and references there given.
[3] *Univ. of Calif. Math. Publs. in Math.*, **2**, 152 (1933).
[4] *M.N.R.A.S.*, **77**, 112–118 (1916); **80**, 138–154 (1919).
[5] *Phil. Mag.*, **36**, 94–128, 203–205 (1918).

Dr. Norman R. Campbell

SCIENCE, as Dr. W. H. George has lately told us, is the activity of scientists; it has no heresy but orthodoxy. Accordingly when we find fellows of the Royal Society indulging in speculations that would have been dismissed as fantastic (or, even worse, metaphysical) a few years ago, the proper course is not to accuse that body of disloyalty to its seventeenth-century traditions, but to inquire why new interests have appeared so suddenly. The answer to that question is very simple: the new developments could have been (I think they were) predicted before they occurred.

Science (or at least physics) has long consisted of two distinct but complementary activities. The distinction between them has been obscured, because they were practised by the same individuals; but now that physicists are rapidly segregating into experimenters who evolve no theories and theorists who make no experiments, there is no excuse for ignoring it.

One activity consists in the discovering of demonstrable experiments and their description (so far as that is possible) by laws. Its procedure is induction, which is the building up of experiments of higher rank out of elements which are themselves experiments of lower rank. It is peculiar to science; its products grow by accretion with little secular modification; it is the source of the practical utility of science.

The other activity consists in devising theories to explain the laws. Explanation is not peculiar to science, and is subject to fashion. But it always consists essentially in some substitution of ideas more intrinsically acceptable for ideas less acceptable. The process can be analysed into three steps: the formulation of hypotheses involving the acceptable ideas alone, the deduction of consequences from these hypotheses, and the translation of these conclusions into propositions concerning the less acceptable ideas by means of a 'dictionary'. The peculiarity of scientific explanations is that they often (not always) predict new laws in addition to explaining old ones.

Until the end of the nineteenth century, the hypothetical ideas introduced by scientific explanations owed their acceptability to an analogy with terms used in stating laws, an analogy so close that the same word was usually used for both, and the distinction between them was overlooked by the careless. But the more thoughtful always realized that (for example) a statement about the density of water was nothing but an imperfect way of describing a demonstrable experiment, while a statement about the density of the æther meant something that could not be demonstrated by experiment at all.

At the beginning of the twentieth century, laws were discovered for the explanation of which no 'mechanical' theory of this type could be found. At first it seemed as if the second activity of science might have to be abandoned. But propositions were found from which, by the aid of dictionary, laws could be deduced and actually predicted. They resembled the conclusions of a mechanical theory in this, and also in involving ideas (such as a wave-function or an imaginary co-ordinate of space) that had no relation, apart from the dictionary, to any demonstrable experiment; they differed in not being deducible from any intrinsically acceptable hypothesis.

If the older structure of science was to be restored, hypotheses had to be found from which these propositions could be deduced. Since the propositions were mathematical and invented by mathematicians, it was inevitable that the hypothetical ideas should be highly abstract and that the acceptability of the resultant theory should lie in a conformity to mathematical ideals, that is to say, in the rigid deduction of elegant conclusions from the minimum of self-evident axioms. The work that makes Dr. H. Dingle so indignant appears to me a laudable attempt to solve this problem, and, if I read his "Principles" aright, so it does to Prof. Dirac. If the attempt succeeds, experimenters will not, of course, gain the satisfaction that they derived from mechanical theories—that appears a vain hope; but even the horror of logical rigidity that inspires the average experimenter may permit him to prefer a logical theory to none at all. Moreover, it is not necessary that all traces of the old analogies should be lost. It is noteworthy that both Dirac and Milne introduce as fundamental an idea closely analogous to the agreement between observers, which is implied in the conception of a demonstrable experiment. (Here, of course, Dr. Dingle is definitely wrong. If only a single observer existed, neither the Doppler nor any other experimental effect could be detected; he would have no means of distinguishing fact from his individual illusions.)

The rest of Dr. Dingle's objections appear to arise partly from a trivial dislike of the term universe —which I share, partly from a flat denial of the duality of physics and a persistent confusion of theories and laws. If he does not deem it important to observe the distinction between what is and what is not demonstrable experiment, surely he should welcome a movement to amalgamate the Royal with the Aristotelian Society.

By Prof. L. N. G. Filon, C.B.E., F.R.S., University College, London

WITHOUT necessarily endorsing all that Dr. Dingle has written about "the New Aristotelianism", one may be permitted to welcome a protest against the modern tendency in physics to proceed from abstract and universal *a priori* mathematical theories, which seem to spring up nowadays in disconcerting numbers.

The real trouble seems to be that, instead of starting from the facts of observation and gradually building up by induction particular laws, which may or may not eventually be linked up, some men of science appear to think that they can solve the whole problem of Nature by some all-inclusive mathematical intuition. What they are really doing is not to explain Nature, but to explore the possibilities of the human mind. If Prof. E. A. Milne's statement, which Dr. Dingle quotes, is really to be taken literally, it would probably lead to the conclusion that all science reduces to pure nominalism, a view to which few would care to subscribe.

Surely it is time that we got back to the more secure, if more pedestrian, methods of the nineteenth century. I seem to remember a phrase, to which at any rate a good deal of lip-service used to be paid, about hypotheses "which were crushed in the solitude of the study". Judging by much of the scientific literature which gets published nowadays, something seems to have gone wrong with the crushing-machines.

Prof. W. Peddie, University College, Dundee

THE recent timely strictures (NATURE, May 8, p. 784) made by Dr. Dingle on modern Aristotelian tendencies in some theoretical investigations in physics suggest that a change in the line of (metaphysical) attack on physical problems might be of value.

For example, if we *postulate* (1) an at least four-dimensional plane universe, (2) motion in our three dimensional part of which is responsible for our idea of space, while, (3) constancy of speed of an observer in the four dimensions originates our idea of absolute time, (4) local time is measured out by the fourth dimensional component of speed, we can deduce the necessity for special relativity.

By modification of some of these postulates we can revel in general relativity.

The difference in effect is that we can blame the objective physical universe for the formal laws of thought, instead of the laws of thought for the structure of the universe. We can escape from Aristotle and get back to Galileo and Newton.

Prof. R. A. Sampson, F.R.S., Royal Observatory, Edinburgh

I HOLD that we are limited beings, limited as regards our senses, which are our only means of understanding the world, and limited also in time. We cannot enlarge our reliance upon our senses. The microscope, for example, is taken as a means for

extending our observations. But any observations made must be afterwards interpreted by the senses, so that we can infer what they represent. We are limited, too, in regard to understanding any communicator. Anything we hear must pass through two barriers: first, whether it expresses correctly what the communicator feels—many professional writers have complained of the difficulties of expression—and second, whether we can understand it. Many cases will occur to anybody when these limitations prove a total blank. I hold all this so strongly that I am not even interested to discuss the question with anyone who holds the opposite view. That means that I am *not* a "modern Aristotelian". But I doubt whether anyone is. Even in the case of those whose words may bear the fair conclusion that they *are* modern Aristotelians, and suppose that they *are* independent of experience, I think that their practice belies their words. It is a very difficult thing even to express oneself so as always to be consistent. Even with the advantage of consisting of the same skin and bone, one often says contradictory things. Then there is the certainty that the statements are not absolute, but depend upon the refinement of our means of expression. The same problem of understanding the world must present itself to primitive peoples, who have no adequate means of expressing their meaning.

The means that are supplied to us provide a distinctly partial means of understanding the world. The senses are very crude, and can only be extended in the way shown above. We cannot tell what time is. Probably it is no more than a physical displacement, like other physical displacements. If this is so, Nature, which depends upon the isolation of events, disappears as an entity, for it depends upon the separation of the time co-ordinate from the others. As I have remarked in another place, it would be a very convenient thing to foretell the future—even to foretell it roughly. We have memory, but not foresight. Descartes' dictum "*je pense, donc je suis*" ought to be supplemented with "*I thought and so I was*", for this is constantly used for prediction, in fact it supplies the second proposition upon which we can base a third. Indeed most of our devices seek to extend the intellect into the future. Ignoring the primitive claims to do so—like 'second sight'—the rules of logic are just such another device, a more subtle device but of just the same sort. For where logic works, it offers to tell us what will happen in another place and time, of which, *ex hypothesi*, we have no experience. Of course a large part of logic is merely explanatory, a mere unfolding of what is implied in the statements. Take, for example, mathematics. The statements found in Euclid are contained in the definitions, postulates and axioms. They are mere statements. None of them can be proved or disproved, and their interest is derived from the way in which they accord with the external world. All the rest is a process of unfolding; and so for other cases—all follow the same model.

To resume, however, the question of predicting the future. It is the greatest error that mathematics makes, that it slurs the distinction between the past and the future—the quantities that it handles being timeless. Of course, in a minor way, the distinction *is* recognized in thermodynamics, in the proposition that entropy must always increase in the future. But this is the sole case, and it might even be traced to a formal expression of the proposition that there is a notorious difference between the past and the future.

The fact is that Nature is becoming enormously complicated—almost too complicated to use. In the old days the same man might be a doctor of medicine and an astronomer. The same word was used to describe an astronomer as an astrologer. Those days are past. We must now be content with theories which express the *essentials* of what we wish to express; to take again an instance from mathematics, the ordinary theory of rotating liquids, which alone is workable, is indefensible hydrodynamically; yet it expresses what is essential of 'centrifugal force', and would collapse without it. So it contains what is requisite, and we extrapolate from it confidently. All statements that we reach are extrapolations from experience, and, as such, their interest depends upon their being approximately true. With our imperfect means of recognizing the truth, we cannot expect any statement to be more than approximately true.

What then is 'observable'? Ought we to limit theorems strictly to what is observable? To confine oneself to what is observable, seems to mean confining oneself to mere verification of experience. For before it is experienced, a prediction is of no value, and after it is experienced, the theory that has led us to look in the right place, must be replaced by the experiment made. Expectation, in fine, depends upon theory. The Brownian movement, for example, must be shown to accord with expectations. A theory will be more or less extensive according to the man that frames it. Some will be complicated by things which must for ever remain unobservable. Some will be directed to a single point. Some will be very extensive, like the law of gravitation or the theories of general dynamics. All are mere guesses at truth. This seems to supply the point of reconciliation for framing a theory independent of experience, such as is denounced in Dr. Dingle's article, and also with the work of a poet or other humanist, who gives us at most a number of illustrative cases.

Prof. C. G. Darwin, F.R.S., Christ's College, Cambridge.

THE most interesting moral to be drawn from Dr. Dingle's article has nothing to do with his detailed criticisms of the theories of the "Aristotelians"; but raises the matter of the curious relationship which at present subsists between metaphysics and science. This has somehow gone wrong. On one hand, we have the philosophers claiming that their metaphysics underlies all possible knowledge, so that they may tell us what we are allowed to think; surely at first sight one would have expected that they

could settle the present controversy with authority. On the other hand, we have the men of science, each with his own philosophy, most usually formed without any professional study of the subject, often held with a most polemic enthusiasm, and showing the wildest divergencies.

The noticeable point is that these contradictious views do not seem to matter in the very least; we none of us doubt that both Dingle and Milne will help on the progress of science, and that the advances they make will have scarcely anything to do with their opposite metaphysical ideas. Each of us must have a philosophy of things in general, and this will include our philosophy of physics; it may be extracted from us with the methods of Socratic dialogue by some more expert philosopher, and the inquiry will usually reveal inconsistencies and impossibilities in our system, so that we shall end by wanting to do to our inquirer what the Athenians did to Socrates. That is not the point; for the fact remains that it is the science and not the philosophy that matters, and that most men of science do not find it worth their while to read the works of metaphysicians. Is not the salient fact about the philosophy of science that no professional philosopher can write a book that a man of science wants to read? Ought there not to be a meta-metaphysics which would bring a message of comfort telling us not to bother about our philosophy—a command most of us already obey—because we can get on with our business without it? A book on this subject would show what a lot of things there are that do not matter much, and it would have the advantage that *a fortiori* no one at all need read it.

Coming down to the details of the controversy, Dr. Dingle seems to want to hamper speculation by forbidding all but the most pedestrian forms of inductive reasoning. It is surely hard enough to make discoveries in science without having to obey arbitrary rules in doing so; in discovering the laws of Nature, foul means are perfectly fair. If Dirac is not to be allowed to conjecture the age of the earth from certain curious numerical coincidences, then Maxwell committed quite as great a crime in conjecturing that the velocity of light was the same thing as the ratio of the electric and magnetic units. It is absurd to maintain that such guesses are illegitimate. On the other hand, some of Dingle's antagonists have held the view that all Nature is contained in our own minds. Surely even if this is true it is a very unimportant statement. Is it not like asserting that anyone who knows the rules of chess has the whole game in his mind, and therefore will always win?

G. J. Whitrow, Christ Church, Oxford

Dr. H. Dingle, in his recent article "Modern Aristotelianism", not only attacks the particular methods adopted by contemporary mathematical investigators in relativistic cosmology, but even refuses to admit that this subject is worthy of scientific investigation as it is based not only on experience but also on reason. This point of view, like most *anti-intellectual* philosophies, is based on a pure myth—in this case a mythical interpretation of the history of scientific method.

Since modern scientific method originated with the Copernican theory of the universe, let us consider the attitude which Dingle, as an empiricist, would have adopted towards the then new cosmology. With the instruments of the day, there were no known celestial phenomena which were not accounted for by the Ptolemaic system. Consequently, he would have regarded Copernicus's system as a "pseudo-science of invertebrate cosmythology", and would have preferred the laborious Ptolemaic inductions built up through centuries of patient observation, since to the many serious objections which were advanced against his theory Copernicus could only plead mathematical simplicity. It follows that Dingle himself would have been in the 'Aristotelian' camp.

It is well known that the driving force of Kepler's work was his belief in the existence of mathematical harmonies in Nature. Similarly, Galileo regarded mathematics as the appropriate instrument for scientific investigation. "We do not learn to demonstrate from the manuals of logic but from the books of demonstration which are the mathematical"[1]. It is of course true, as Dingle points out, that he said "Nature nothing cares whether her reasons and methods of operating be or be not understandable by man". This, however, as Dingle does *not* point out, was merely an expression of his rejection of Scholastic *teleology*. In point of fact, Galileo's whole life's work was based on the firm belief that Nature *is* understandable by man. "Philosophy is written in that great book which ever lies before our eyes—I mean the universe. . . . This book is written in the mathematical language"[2]. Indeed there is little doubt that Galileo regarded mathematical demonstration as an *a priori* method of reaching truth. As Mr. J. J. Fahie has shown, "in order to be able to demonstrate to his opponents the truth of his conclusions he had been forced to prove them by a variety of experiments, though to satisfy his own mind alone he had never felt it necessary to make any"[3].

We thus find, *pace* Dingle, that the methods of modern cosmologists are not so opposed to those of Galileo as he would have us believe. The three fundamental rules on which scientific investigation has proceeded since the days of Copernicus are: (1) belief in the simplicity and uniformity of Nature; (2) belief in the possibility of a mathematical description of Nature; (3) gradual discarding of all anthropocentric concepts. In particular, the last rule has recently found expression in the principle of relativity which, in general terms, can be said to signify the autonomy of individual points of view in the description of Nature. Prof. Milne's concept of equivalent observers is one way of making this idea precise. Relativistic cosmology, as based on these three rules, is therefore in the true spirit of all natural philosophy since the Renaissance.

In conclusion, I should like to direct attention to the extreme outburst of irrationalism which leads

Dingle to declare that "Even mere verbal heresies such as the statement that science is based on faith in an 'Order of Nature' have been few and harmless; we may have said such things, but we have always acted as though Nature might nothing care for what our minds call order". Actually, the exact opposite has been the case, for, as Prof. Whitehead points out, "there can be no living science unless there is a widespread conviction in the existence of an Order of Things and in particular of an Order of Nature"[4].

[1] "Opere", 1, 42 (1842).
[2] "Opere", 4, 171 (1842).
[3] Singer, "Studies in the History and Method of Science", 2, 251 (1921).
[4] "Science and the Modern World", 5 (1926).

Fr. W. McEntegart, S.J., Heythrop College, Chipping Norton

Dr. Dingle's contribution to Nature of May 8 is a notable portent, and his challenge is happily juxtaposed with Lord Rayleigh's article on "Optical Contact". The inarticulate many who are pining for a return to reality in physics must be heart-stirred to find themselves possessed of such a champion. But one small protest must be entered. Why does Dr. Dingle dub his unrealist targets Aristotelians? Let him, if he so wishes, name them Kantians, Hegelians, or even Fichteans. But Aristotle and every loyal Aristotelian are on his own side. No experimenter, aiming, in Dr. Dingle's own words, "at rationalizing observations", ever did, or ever can, dispense with philosophy. Sound metaphysics will not compensate for an insufficiency of observation, culpable or otherwise; but the absence of a solid metaphysical foundation will vitiate all attempts to rationalize observations, howsoever abundant they be.

The fiction that the peripatetic philosophy is an airy tissue of aprioristic deduction should by now be outworn. It is, itself, a rationalization of experience, firmly keyed to reality. It is the mould the impress of which is still unmistakable in the language and thought of the civilized world, and some serious acquaintance with it would have a salutary and sobering effect on the 'universe' fabricators of whom Dr. Dingle complains. Sir Arthur Eddington, for example, would find it a philosophy ample enough to accommodate all the intuitions he can command, and might cease to claim for epistemology a super-comprehension of the existing physical world.

Dr. Dingle bewails the new 'cosmolatry' engendered "by metaphysics out of mathematics". If, in the spirit of true science, he would but disregard the traditional taboo, and discover for himself what metaphysics is, he could not impute such disreputable offspring to so noble a sire: and is his confidence that "the theory of relativity appears to be the *innocent* cause" (italics mine) securely founded as touching the innocence? The 'hall-marks' alleged recall a wise saying of Whitehead's, "The history of science is strewn with the happy applications of discarded theories."[1]

[1] "The Principle of Relativity", p. 3.

Dr. H. Stafford Hatfield

Dr. H. Dingle's rather drastic remarks concerning present-day physical speculation seem to overlook one main factor in the progress of science—the capture of young minds of the highest order of genius. The genius of every age turns mainly in that direction which offers the greatest scope for the creative imagination. Since the time of Galileo, physics has seemed (however wrongly, according to some philosophers) to promise an eventual view of all time and all space, just as biology seems to offer an approach to the secret of life and mind. Most of their greatest exponents have combined a vigorous care for fact and sound reasoning with a delight in imaginative speculation. The great army of lesser workers, experimental hobbyists, mathematical cross-worders, even competitive careerists, find their world brighter for the speculative flights of men who, furthermore, often beat each at his own game.

A science kept strictly to facts and formulæ by 'pale Galileans' would recruit very few geniuses, fewer rank-and-file workers, and—fewer endowments!

Prof. G. Dawes Hicks, emeritus professor of philosophy, University College, London.*

I find it difficult to discuss profitably Dr. Herbert Dingle's article, because, although I think there is a certain amount of justification for the protest he raises against some current ways of presenting theories of the physical world, yet it seems to me he lays the blame upon the wrong things, and draws a contrast for which there is really no warrant.

Let me dwell, first, on the latter point. There were doubtless bastard "Aristotelians" at the time of the Renaissance, as there have been bastard "Galileans" since. But no genuine follower of Aristotle would have admitted for a moment that "Nature is the visible working-out of general principles known to the human mind apart from sense-perception", or that "reason could, apart from sense, dictate the course of experience". As a matter of fact, in handling the question how we come to reflect on general principles, Aristotle consistently maintained that it was by induction from experience. The human intellect was not supposed by him to operate *in abstracto*. On the contrary, general principles came to be apprehended in and through the matter furnished in sense-perception. Aristotle never questioned the validity of the data of sense. Indeed, the human mind or consciousness, as he conceived it, was just a capacity of becoming aware of the facts presented in sense-perception, and of dealing with them by certain processes which constitute the mental life itself. At the basis of all these was the process of sense-perception (αἴσθησις), and sense-perception had for its objects individual things (τὰ ὄντα). Not only so, sense-perception was not viewed as merely passive receptivity. It was a natural discriminative faculty

* This contribution, and another by Prof. H. Levy, were not received in time to be submitted to Dr. Dingle for possible comment. Prof. H. Levy's article arrived after this Supplement had been made up, and is printed on p. 1025 of this issue.

(a δύναμις κριτικὴ). Out of it, in its cruder forms, memory comes to be, and out of frequently repeated memories of the same thing experience (ἐμπειρία) is developed, from which again "originate the skill of the craftsman and the knowledge of the man of science". "These states of knowledge are neither innate in a determinate form, nor developed from other higher states of knowledge, but from sense-perception" (*Post. An.*, ii, 19, 100^a). The human subject is capable, that is to say, of comparing experiences of various kinds with each other, and of concentrating attention upon one characteristic of a concrete thing to the exclusion of other characteristics. Thus, on the basis of familiarity with concrete things, the human subject can build up an abstract science.

Aristotle certainly distinguished between the way in which we come to be aware of general principles and the evidence there is for their truth. He did not fall into J. S. Mill's error of supposing that the instances used in an induction serve not only to lead us to recognize the general principle they exemplify but also to guarantee likewise its validity. Aristotle held, indeed, that the ultimate principles of science neither permit of nor require proof. When through induction the scientist has attained to such a principle, reason enables him to see by a kind of intellectual intuition that it is true. We may, or may not, acquiesce in this view; but, at any rate, it does not involve the inconsistency of first pronouncing induction by imperfect enumeration to be (as it obviously is) fallacious, and then assuming that the ultimate premiss of natural science is established by this fallacious method.

That Aristotle failed to recognize the importance of observation in scientific investigation is simply contrary to all the information that has come down to us. In biology particularly, whether we have regard to his powers of observation, his collation of what others had observed, or his theoretical deductions therefrom, we cannot but be struck by his remarkable achievements. "All the general propositions which he expresses are", said Cuvier, "inductions, resulting from observation and the comparison of particular facts; never does he impose a rule *a priori*". "Linnaeus and Cuvier have been my two gods", wrote Darwin, "though in very different ways, but they were mere schoolboys to old Aristotle". It is worth while mentioning here a significant example of the trait we are referring to. After having discussed in detail the manner of the generation of bees, Aristotle adds, "the facts, however, have not yet been sufficiently grasped; if ever they are, then credit must be given rather to observation than to theories, and to theories only if what they affirm agrees with the observed facts" (*D. G.*, iii, 10, 760^b).

There was, indeed, among the ancients generally no lack of appreciation of the need of careful and accurate observation; and they probably made more use of experiment than appears at first sight from the accounts we have of their work. Not then in this respect did their outlook differ from ours. The acceptance of the Copernican system may, from some points of view, be looked upon as the beginning of the modern scientific era. Yet clearly it was not merely by patient observation of the facts that this revolution came about, for such observation by itself could have led no one to question what seems, on the face of it, to be an obvious fact, that the sun moves round the earth. Rather was the Copernican hypothesis adopted because it enabled the movements of the heavenly bodies to be expressed in simple mathematical formulæ. But, so far as mere observation of the facts was concerned, there was nothing to suggest that the simplest explanation could be accepted as the true one. It was because it had come to be seen that science requires, in order to get under way, the recognition of some theory of what explanation means, that the Copernican hypothesis in the long run prevailed. It is noteworthy that Galileo himself expressed his admiration for that trust in reason which induced Copernicus to adhere to his view, in spite of its apparent discrepancies with the facts, until more delicate observations in the end confirmed it. The great claim of Copernicus to fame rests, he said, upon this, "that he did constantly continue to affirm (being persuaded thereto by reason) that which sensible experiments seemed to contradict".

Now, this "rape of reason upon the senses", as Galileo styled it, really means that in order to make headway the scientific investigator is constrained to adopt, either explicitly or implicitly, as a necessary preliminary, some general philosophic view, good or bad, of the nature of things. "If science is not to degenerate into a medley of *ad hoc* hypotheses, it must", writes Whitehead, "become philosophical and must enter upon a thorough criticism of its own foundations." It is not because Sir Arthur Eddington and other writers on relativity approach their theme from a philosophic point of view, but because the particular philosophic point of view which they adopt so often evinces itself as thoroughly unworkable, that one finds their treatment unsatisfactory. For example, when it is laid down, at the start, that "the task of physical science is to infer knowledge of external objects from a set of signals passing along our nerves", or rather that "the material from which we have to make our inferences is not the signals themselves, but a fanciful story which has been in some way based on them", that "it is as though we were asked to decode a cipher message and were given, not the cipher itself, but a mistranslation of it made by a clumsy amateur", we are confronted not with a "new Aristotelianism" but with a theory of knowledge which leaves us gasping when we try to put its parts together. It is not a question, as Sir Arthur Eddington appears to think, between idealism and realism; neither on idealistic nor on realistic grounds could a notion of this sort be sustained. There are certain well-established truths concerning the nature of knowledge which simply put a view of the kind indicated altogether out of court. The unfortunate thing is that the excessive specialization of the present day renders it well-nigh impossible for the scientific investigator to keep himself abreast of what is being done in the fields of epistemology and the logical scrutiny of scientific method.

Deductive and Inductive Methods in Science: A Reply

By Dr. Herbert Dingle

LET me begin by removing misapprehensions. I did not, as Prof. J. B. S. Haldane suggests, attack Prof. E. A. Milne and Sir Arthur Eddington: "Milne", "Eddington" and "Dirac" were merely symbols to facilitate reference to certain passages of literature with which alone I was concerned. Nor did I award Prof. Milne the title of traitor. My point was that the present menace was *not* one of the ordinary cases of treachery to a cause; it was the creation, through the aberration of some of our most gifted minds, of an atmosphere in which treachery can flourish. I have the greatest respect for Prof. Milne, but I cannot speak too disrespectfully of the cosmological principle.

To Aristotle also let justice be done. The answer to Father McEntegart's question is that I called my "targets" Aristotelians because they have revived the fallacy maintained by those whom Galileo and Newton called Aristotelians and who never, to my knowledge, disowned the name. To avoid libelling Aristotle, I put the word in quotation marks at its first appearance.

Except in one instance, I will not comment on matters of history: the facts are open to anyone who is able and willing to read them without prejudice. But I feel that I must put in a mild protest on behalf of Newton. Referring to modern mathematical physics, Prof. W. H. McCrea tells us that "for mathematical convenience, the hypotheses are generally stated as axioms". I had noticed that, and I can see the convenience when one comes to interpret the results, though its advantage at other times escapes me. But it must be remembered that Newton's mathematics was much cruder than ours, and since he repeatedly said that he did not make, and strongly deprecated, hypotheses, the probability would seem to be that he meant what he said: and when you try to discover what he did mean by hypothesis, you find that it was something so like Prof. Milne's cosmological principle that, if you relied only on the effete process of inspection for comparing them, you would scarcely know the difference. It appears, therefore, that Prof. McCrea, in classifying Newton's laws of mechanics with Milne's principle, has credited Newton with a mathematical facility not available in his day.

Several questions have been raised relating to the theory of knowledge and such subjects, which interest me very much, but I must keep to the main point. If anyone is interested in my attitude to these matters, he will find it expressed in a book now passing through the press; having tried to be explicit in 100,000 words I do not wish to court misunderstanding in fifty. For the purpose of this discussion, therefore, I adopt the view that the aim of science is the discovery of the truth about Nature. I do not think those are the best terms of expression, but they are simple and familiar and are perfectly applicable here. The question is then whether we can discover the truth about Nature rationally without recourse to experience.

I am delighted at Sir Arthur Eddington's admission that "we can have no *a priori* knowledge of the universe". His further statement that "*Knowledge of the mass-ratio* (of proton to electron) *is not knowledge of an objective universe*", for all its italics, comes a little late to be impressive, for it is what a die-hard of the Galilean school has been vainly trying to tell him for years. Even so lately as in his "New Pathways in Science" he wrote (pp. 20–21) that protons and electrons have "the same kind of connection with human experience as . . . sticks and stones and stars".

Sir Arthur's new discovery confirms my statement that his philosophy lags far behind his instinctive ideas. Without any of his genius, I saw clearly nearly a decade ago what has been revealed to him only through the agony of giving birth to an intricate theory; my 'hunch' that there is a lot in the theory is accordingly strengthened. But since for one person who tries to understand it there are a thousand who are guided, or misguided, by his philosophy, I venture to assure him that his new vision does not include the requirement that conflict with the laws of Nature is impossible or unobservable. That is even farther from the truth than his earlier belief in universal indeterminacy, which presumably cannot remain undisturbed in the face of inviolable laws. However, all in good time: meanwhile, let me also have my italics—"*We can have no* a priori *knowledge of the universe*". *Vicisti, Galilei*!

Prof. Milne simply makes it more clear than before, if possible, that he is not concerned with science at all. He does not want to find out the truth about Nature; he wants to make a theory. "Systems are constructed which may or may not have their counterparts in the external world". We then appeal to observation "to discover theorems empirically" and "to verify the relevance of theorems discovered by inference from axioms". (I thought some observers tried to discover facts, but apparently I was mistaken; physics based on facts is "irrational and inexact".) But the appeal is really idle, for "theorems exist in their own right", which is by no means affected if observation should prove them irrelevant. We go on looking, presumably, through a kind of race-memory generated in the old days of superstition.

I have no objection to theorems existing in their own right, any more than to the Choral Symphony existing in its own right, but I should very much like

to know by what right Prof. Milne steals a march on Beethoven by publishing his creations in scientific journals. He is not interested in the standard metre because it is "undefinable". It is a part of the external world, of course, but he does not want to know about the external world; definitions are all that matter. He writes about hypotheses because they can be defined as axioms, but he will not write about Nature. Since when has the Royal Society been dedicated to the study of definitions?

I am not, I repeat, trying to stifle newly-born methods of research; the situation is quite other. Let us see by an actual example what this theory is. It made its debut early in 1933 in a 95-page article[1] arising out of the problem of the apparent recession of the nebulæ. After four years, crammed with developments and amendments of the fundamental idea, the solution of the problem is reached; here it is: "Imagine a nebular nucleus joined to ourselves by a material bar, supposed rigid; would the end of the bar and the nebular nucleus separate? The answer is that they would if the bar is deemed rigid Deductive and Industive Methods of Science, etc. 2 on the kinematic scale but not if the bar is deemed rigid on the dynamic scale."[2] Everything depends, it seems, on what we "deem"; what, then, shall we deem the standard metre? Ah, we cannot say; that is undefinable. Well, perhaps not enough has been published yet; suppose we wait a few years longer? No, that will not do, for the question is settled already; this is the "solution of the much-disputed problem" (p. 345)—"we resolve the controversy" (pp. 328–9). And, indeed, Prof. Milne's problem *is* solved, because it is not a problem about Nature but about "axiomatic definitions" existing in their own right. On the *scientific* problem, which is concerned with the actual world, we get no help. I hope Prof. Haldane is not building too much on the possible contribution of the theory to the problem of organic evolution. When an observer in a spiral nebula can define an organism in terms of clock-readings, some light *may* be thrown on the clock. Since a non-observer on the earth can never define the standard metre in any terms, the prospect can scarcely be called bright.

I did not overlook Dr. H. Stafford Hatfield's point concerning the "delights of imaginative speculation" or "the capture of young minds". These practices, like armament racing, add to the joy of life, and the capture of young minds can be illustrated well enough (though more serious and equally relevant examples exist) by the last paragraph of Mr. G. J. Whitrow's contribution (p. 1008), in which he calls a statement of mine an "extreme outburst of irrationalism" because he has found a remark by Prof. Whitehead which exemplifies it. That before criticizing it all at he should have found a contribution to science by Prof. Whitehead or another that would collapse if one removed the presupposition of an "Order of Nature"; and that, if he had found it, he would have shown my statement to be inaccurate (contrary to fact) and not irrational (contrary to reason)—did not occur to him. Having been well captured, he has lost the faculty for distinguishing fact from reason, and, using Milne's device of "an extreme application of the principle of the economy of thought", he rushes into print in my support. Mr. Whitrow might have made brilliant and valuable contributions to science—and this is what kinematical relativity has made of him. Yes, it is fine sport, and may even bring endowments. If I were a rich would-be dictator, I would endow it myself.

Once more, I do not want to hamper speculation in the way Prof. C. G. Darwin suggests. I cited Prof. Dirac's letter not as a source of infection but as an example of the bacteria that can flourish in the poisoned atmosphere; in a pure environment it would not have come to birth, and we should still have had the old, incomparable Dirac. One may fly as well in pure as in foul air, and with better prospects. But my concern is with the general intellectual miasma that threatens to envelop the world of science, and I emphatically disagree with Prof. Darwin's opinion that it does not matter what you think about science so long as you advance it: as well say that it does not matter what you burn so long as you make a bonfire. If I never added a jot to science, but succeeded in discovering and making clear what science is doing, I should consider that I had served my generation at least as well as if I had discovered a new universe. In the last resort, this matters almost more than anything else. How many physicists to-day feel confident that they can read a statement concerning our ordinary everyday consciousness of time, for example, and say, not whether it is true or false, but even whether it is sensible or nonsensical, a serious idea or a clever hoax? The criterion for distinguishing sense from nonsense has to a large extent been lost: our minds are ready to tolerate any statement, no matter how ridiculous it obviously is, if only it comes from a man of repute and is accompanied by an array of symbols in Clarendon type.

If this state of mind exists among the *élite* of science, what will be the state of mind of a public taught to measure the value of an idea in terms of its incomprehensibility and to scorn the old science because it could be understood? The times are not so auspicious that we can rest comfortably in a mental atmosphere in which the ideas fittest to survive are not those which stand in the most rational relation to experience, but those which can don the most impressive garb of pseudo-profundity. There is evidence enough on the Continent of the effects of doctrines derived "rationally without recourse to experience". To purify the air seems to me an urgent necessity: I wish it were in other and better hands.

[1] *Z. Astrophys.*, **6**, 1 (1933).
[2] *Proc. Roy. Soc.*, A, **158**, 345 (1937).

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Laboratory. In addition to the maintenance of standards of measurement and testing of instruments and materials, much research work has been done on these subjects and on others submitted by industry. A noise meter and an instrument for measuring the penetration of daylight into rooms have been devised. Light magnesium alloys of greatly increased strength have been produced, and the eight wind tunnels have been in full use. The use of protective coatings on steels liable to corrosion and the reasons for the superiority of certain oils as lubricants are being investigated, while the ship testing tank has been requisitioned for 78 per cent of the merchant shipping constructed in Great Britain.

Motion Pictures of Solar Prominences

VOL. 3, No. 1 of *Publications of the Observatory of the University of Michigan* contains a detailed description by R. McMath of the tower telescope of the McMath-Hulbert Observatory which has recently been built near Lake Angelus, Michigan. The telescope, which comprises a modified form of spectroheliograph ("the spectroheliokinematograph"), has been used to take successful motion pictures of solar prominences. A specimen film, taken in K_2 light and showing various types of prominences, was recently exhibited at one of the monthly meetings of the Royal Astronomical Society. The gradual movements of comparatively stable prominences, the eruptive motions of others, and the frequent occurrence of descending streams of prominence matter from points of origin above the chromosphere were beautifully shown (*The Observatory*, 85, April 1937). It may be noted that the founders and designers of the Observatory and its equipment, R. R. McMath, H. S. Hulbert and F. C. McMath, are non-professional astronomers. Acknowledgment is made by them in the present memoir for assistance received in advice, expert supervision and financial assistance during the building of their tower telescope.

The Zoological Society of London

"THE ZOO" progresses from strength to strength. A hundred years ago the then Earl of Derby announced that its income had mounted to £19,123; now it is £154,172; then the visitors numbered 363,392, now they are 1,962,122 plus another 484,453 at Whipsnade. The members who now number 8,307 were then 3,050. The stock of animals has increased slightly more rapidly than the members, 3,867 as against 1,025 in 1837. One of the outstanding exhibits of the year (according to the report submitted to the annual general meeting on April 29) was a group of young Hainan gibbons, but the first giant forest hogs to reach any zoological garden and several white-tailed gnus, a species now extinct in a wild state, will arouse equal interest when they are shown to the public. The only important building erected during the year was the Studio of Animal Art, opened on April 21 by Lord Snell; but an innovation which obviously met a popular need was the launching of the *Zoo* magazine, the circulation of which has been maintained at close upon 100,000 copies.

Dr. Marie Stopes's Coronation Ode

IN his broadcast speech on Coronation Day, May 12, H.M. King George referred to the Royal office to which he has succeeded as the symbol of the constitutional link of Empire and to the service which he desired to render while occupying it. Dr. Marie Stopes has been inspired by the occasion to compose a lyrical poem in which she happily expresses these and other attributes of kingship as represented by his present Majesty. Four lines may be appropriately quoted here: they are:

Thou art the centre of a world-wide plan
To make new triumph out of world-worn things.
Thou art the symbol of a nation's pride,
The dazzling spear-point of an Empire's van.

The ode has been attractively printed upon a card, and copies may be obtained from The Eclipse Press, 108 Whitfield Street, London, W.1, at the price of 1*d*. each or twenty for 1*s*.

Jubilee Number of *The Observatory*

THE inception sixty years ago of a monthly magazine, *The Observatory*, familiar in its blue covers to astronomers the world over, is commemorated in a special number, No. 755, of April 1937. The magazine was founded in 1875 by Sir William Christie, who was then Astronomer Royal, and it has been carried on voluntarily ever since, without any monetary grants whatsoever towards its publication, by successive editors, mainly from the observatory staffs at Greenwich and at Cambridge. The present number includes three special articles. In the first, Sir Frank Dyson reviews "Sixty Years of Meridian Astronomy"; the second article is by Sir Arthur Eddington (himself one of the joint editors in 1913–19) who writes on "Interstellar Matter"; while the third is from a veteran contributor (editor, 1893–1912), Mr. H. P. Hollis, whose article deals with Airy's water telescope. A unique feature of the past which found an appreciative circle of readers was the monthly anonymous contributions "From An Oxford Notebook" by the late Prof. H. H. Turner; these notes ran without a break from 1894 until Turner's death in 1930. The current numbers of *The Observatory* carry on the useful work of providing a report of the discussions at the meetings of the Royal Astronomical Society (including Geophysical Discussions), in publishing articles by astronomers of international repute, and in airing views in the pages devoted to correspondence.

Inland Water Survey

CAPT. W. N. MCCLEAN is publishing, in graphical form on standard sheets, the continuous record of water-levels, flows and storages, and of rainfall and temperature, for the Rivers Garry, Moriston and Ness, the three principal rivers of the Ness Basin of 700 square miles area. The records cover the period September 1929 to the end of the year 1935, and are obtainable in separate sheets or as a complete series for each river in a portfolio. The river survey work has been organized and carried out by the staff of River Flow Records, under the direction of

Capt. W. N. McClean. Capt. McClean's activity in the development of inland water survey in Great Britain is well known, and, in addition to the practical survey and record work, he initiated and organized the discussion on the need for this survey which the British Association staged at the York meeting in 1932. That discussion gave birth to the committees of the British Association and Institution of Civil Engineers, and these two bodies induced the Government to appoint the present Inland Water Survey Committee of the Ministry of Health. The graphical records of the Ness Basin rivers, which are similar to those published for the Aberdeenshire Dee, are the first of their type in Great Britain, and bring together the hydrological and meteorological records in a form from which statistical data may be derived or any desired analysis of river flow and storage may be made. A list of the published records can be obtained from "River Flow Records", Parliament Mansions, Victoria Street, S.W.1.

American Society of Ichthyologists and Herpetologists

AT the twentieth annual meeting of the American Society of Ichthyologists and Herpetologists, held in Washington, D.C., on May 4–8, the following officers were elected : *Honorary Presidents*, John T. Nichols and Dr. Leonhard Stejneger ; *President*, Prof. William K. Gregory ; *Vice-Presidents*, L. M. Klauber, Leonard P. Schultz, and Dr. Hobart M. Smith ; *Secretary*, M. Graham Netting ; *Treasurer*, Dr. Arthur W. Henn ; *Editor-in-Chief*, Mrs. Helen T. Gaige, of *Copeia* ; *Ichthyological Editor*, Lionel A. Walford ; *Herpetological Editor*, Karl P. Schmidt. The election of four honorary foreign members was announced at the meeting ; namely, for 1936, Prof. David M. S. Watson, University College, London, and Prof. Franz Werner, University of Vienna ; for 1937, Dr. Leo S. Berg, Academy of Sciences, Leningrad and Dr. W. Wolterstorff, Museum für Natur- und Heimatkunde, Magdeburg. Dr. G. A. Boulenger is the only previously elected honorary foreign member.

Systematics in Relation to General Biology

WHILE taxonomy has, in the past, been based mainly on morphology, the advances of the past two decades in cytology, ecology, genetics and other branches of biology have made it necessary to consider their contribution to the subject. As a result of meetings of members of the staffs of the John Innes Horticultural Institution and the Royal Botanic Gardens, Kew, and of zoologists interested in taxonomy, a "Committee on Systematics in Relation to General Biology" has been formed, the first open meeting of which will be held in the rooms of the Linnean Society, Burlington House, London, W.1, on June 25 at 3 p.m. All biologists interested in the general aims of the Committee are invited to attend. The chairman of the Committee is Dr. Julian S. Huxley, and the two secretaries are Mr. J. S. L. Gilmour, Royal Botanic Gardens, Kew (botany), and Mr. H. W. Parker, British Museum (Natural History), South Kensington, S.W.7 (zoology).

U.S. National Academy : New Members

THE following have recently been elected members of the U.S. National Academy of Sciences : Dr. C. B. Bridges, geneticist at the California Institute of Technology ; Dr. O. E. Buckley, assistant director of research in the Bell Telephone Laboratories ; Prof. A. J. Dempster, professor of physics in the University of Chicago ; Prof. E. W. Goodpasture, professor of pathology in Vanderbilt University ; Dr. C. G. Hartman, research associate in the Department of Embryology, Carnegie Institution of Washington : Dr. D. F. Hewett, U.S. Geological Survey ; Prof. Leo Loeb, professor of comparative pathology in Washington University (St. Louis) ; Dr. D. A. MacInnes, associate member (physical chemistry) in the Rockefeller Institute for Medical Research ; Dr. G. R. Minot, Boston City Hospital ; Prof. J. von Neumann, professor of mathematical physics in the Institute for Advanced Study, Princeton ; Dr. S. B. Nicholson, astronomer at Mt. Wilson Observatory ; Prof. Otto Struve, director of Yerkes Observatory ; Dr. F. B. Sumner, professor of biology in the Scripps Institution of Oceanography ; Dr. Charles Thom, soil microbiologist in the U.S. Department of Agriculture, and Prof. E. C. Tolman, professor of psychology in the University of California. Prof. August Krogh, of the University of Copenhagen, known for his work in animal physiology, has been elected as a foreign associate of the Academy.

Announcements

DR. W. L. BURGESS, Dr. W. R. Wooldridge, Sir Louis J. Kershaw and Sir Joseph Barcroft have been appointed members of the committee set up in November 1936 to review the facilities available for veterinary education in Great Britain (see NATURE, 138, 837 ; 1936). Dr. Thomas Loveday, vice-chancellor of the University of Bristol, has been appointed chairman.

PROF. JAMES YOUNG, director of the Department of Obstetrics and Gynæcology at the British Post-Graduate School, has been elected chairman of the Medical Advisory Board of the British Social Hygiene Council in succession to Sir Farquhar Buzzard.

PROF. WLADIMIR KÖPPEN, the eminent Graz meteorologist, has been awarded the shield of nobility by the German Chancellor.

DR. RICHARD HESS, professor of zoology at the University of Berlin, has been made an honorary member of the Bavarian Academy of Sciences.

THE British Homœopathic Congress will be held at the Langham Hotel, London, W.1. on June 17–18. Further information can be obtained from the Secretary, 69 Elizabeth Street, S.W.1.

THE Fourth General Assembly of the International Union for the Scientific Investigation of Population Problems will meet in Paris on July 28 in connexion with the International Population Congress, organized by the French National Committee, which takes place on July 29–August 1.

Letters to the Editor

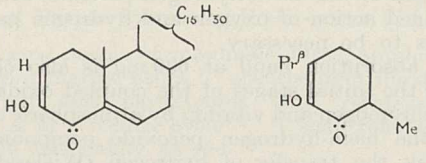
The Editor does not hold himself responsible for opinions expressed by his correspondents. He cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*

NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 1022.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Cholesterol and the Adrenal Cortical Hormone

THE discovery that carbon atom C_4 of the cholesterol molecule is the first point of attack by oxygen, leading to the formation of *cis* $\Delta^{5:6}$-cholestene-3 : 4-diol, suggested that this reactive primary oxidation product may play an important role in the metabolism of cholesterol and the sexual hormones. Experimental confirmation of this view was obtained by feeding experiments, which showed that the diol, as well as its intermediate dehydration product cholestenone, were converted into coprosterol by the animal organism[1]. We have now obtained by mild oxidation of the 3-monoacetate (or 3-benzoate) of the *cis*-diol what is probably the oxide of 4-ketocholestenol 3-acetate (or 3-benzoate). These compounds yield on hydrolysis a highly reactive substance $C_{27}H_{42}O_2$ (I) which possesses the typical grouping —C=COH—CO— of diosphenol (buchu-camphor) (II).



I II

As the substance, like diosphenol, functions in several tautomeric forms, it would be arbitrary to assign a definite structural designation to it, and we propose to call it *diosterol* (abbreviated from diossterol). Diosterol reduces ammoniacal silver solution, reacts with Schiff's fuchsin reagent and, as a phenol, is extracted by strong alkali from its ether- or benzene solution, forming yellow insoluble sodium or potassium salts (see forthcoming publication in conjunction with W. W. Starling and V. A. Petrow). The intensity of the ultra-violet absorption spectrum of diosterol is $\varepsilon_M = 22{,}000$ at 320 mμ, that is, twice that of diosphenol at 270 mμ [2]. Diosterol was found to be identical with the "substance $C_{27}H_{42}O_2$" of Inhoffen[3] and Butenandt and Schramm[4], which is obtainable by a complex debromination process from certain bromination products of cholestenone.

Since it has now been shown that the same substance arises from cholesterol in definite stages by a process which may easily be visualized as a biological oxidation occurring in the animal organism, a hint is afforded in explanation of the unknown position and function of the fourth oxygen atom in the adrenal cortical hormone. This labile hormone, essential for life, possesses the remarkable property for a steroid of reducing ammoniacal silver solution, usually attributed to the terminal —$CO.CH_2OH$ group in ring IV.

It is tempting to suggest that the diol and its immediate oxidation products may be steps in the biological formation from cholesterol of the labile cortical hormone of the adrenals. A new experimental approach to the system from the sexual hormones and other steroids has become available by the further discovery that the conversion of cholesterol into *cis*-cholestene-3 : 4-diol can be effected by simple treatment of cholesterol dibromide with silver acetate in pyridine at room temperature. This reaction, which is probably a general one, is at present being applied to steroids with a 5 : 6 ethylenic linkage.

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Inhibition of the Gonadotropic Activity of Pregnancy Urine Extract by the Serum of Rabbits injected with an Extract of Male Urine

THE fact, first noted by Bachmann, Collip and Selye[1], that the serum of an animal which has received prolonged treatment with gonadotropic substances may acquire antigonadotropic properties, has been confirmed by many other workers. Twombly[2] was able to produce active antisera by the use of solutions of pregnancy urine extract, most of the initial gonadotropic activity of which had been lost during storage, and his results suggested that the presence of active gonadotropic substance, in the material used for repeated injection, might not be essential. We were able to confirm this supposition.

A precipitate was prepared from the urine of normal male factory workers, by a method known to be suitable for concentration of the gonadotropic substances in pregnancy urine. 3·9 mgm. of this preparation (representing 50 c.c. original urine) yielded no trace of a gonadotropic reaction in immature female rats ; 22·5 mgm. (representing 300 c.c. original urine) produced œstrus in six treated rats, with luteinization in one of them.

An adult female rabbit received 10·0 mgm. daily for 30 days (representing 130 c.c. original urine daily). The ovaries were apparently not influenced by this treatment, for autopsy showed ripe follicles only. Each of six immature female rats then received a total dose of 1·2 c.c. of the blood serum of this rabbit at the same time as 5 units of gonadotropic hormone

from pregnancy urine (Pregnyl Organon). The serum was injected on the left, the Pregnyl on the right side of the rat, the number and interval of the injections being the same as in pregnancy tests according to Aschheim and Zondek. Œstrus, enlargement of the uterus and luteinization were completely inhibited by the addition of the rabbit serum, occurring only in the controls. Each of six immature male rats then received 12 R.U. of Pregnyl and 3 c.c. serum during six days. Addition of the serum to the gonadotropic preparations here inhibited the enlargement of seminal vesicles, preputial glands and testes produced in controls which had the same dose of hormone without serum.

Our experiments prove that it is possible to obtain rabbit serum with antigonadotropic properties by repeated injection of a negligibly gonadotropic extract of male urine. It is just possible that the effect is due to the traces of the gonadotropic hormone; more likely, in view of the experience of Twombly, it is due to the presence of a different substance with similar antigenic properties.

Rabbits treated with serum from a non-pregnant mare failed to produce a serum that would inhibit the gonadotropic activity of pregnant mare serum in immature female rats.

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Coupled Oxidation of Ascorbic Acid and Hæmochromogens

HÆMOCHROMOGENS catalyse the oxidation of ascorbic acid at a *p*H below 7 at which there is no autoxidation of ascorbic acid. We can confirm this result of Barron and co-workers[1], but the authors have overlooked that the hæmochromogens also undergo oxidation under these conditions, as first observed by Karrer and co-workers[2]. The product of this oxidation is verdohæmochromogen, a bile pigment iron compound[3].

We have studied in detail the coupled oxidation of ascorbic acid and pyridine hæmochromogen, with the double purpose of investigating whether or not the physiological hæmoglobin breakdown and the bile pigment formation can be assumed to proceed in this way and of getting information on the mechanism of hæmin catalysis.

Hæmin was measured spectro-colorimetrically as hæmochromogen after reduction with hyposulphite, ascorbic acid iodometrically or with the indicator method in the hydrogen cyanide trichloroacetic acid filtrate. In air, with a 2 mM. vitamin and a 0·2 mM. hæmin concentration, half the hæmochromogen is transformed into verdohæmochromogen during 1 minute at 37°, and after 10 minutes this oxidation is complete. The catalytic effect of pyridine-hæmochromogen, studied by Barron and co-workers over periods of hours, was thus largely due to verdohæmochromogen. The oxidation of 1 mgm. of vitamin is accompanied by that of 0·2-0·3 mgm. hæmin, rather independently of the hæmin and vitamin concentrations; at lower oxygen pressure relatively more hæmin is oxidized. The oxidation of vitamin leads to dehydroascorbic acid. Glutathione does not inhibit verdohæmochromogen formation in presence of ascorbic acid, but protects ascorbic acid by back reduction of dehydroascorbic acid; with glutathione alone, not verdohæmochromogen, but a hæmatin compound of hæmatin *a*-type is obtained. In the system oxygen—hæmochromogen—ascorbic acid—glutathione—glucose dehydrogenase—glucose, the oxidation of a hæmatin to a bile pigment is coupled with the final oxidation of glucose alone.

The mechanism of the coupled oxidation is interesting. Peroxides do not produce a verdohæm compound with hæmatin in pyridine, or if acting on pyridine hæmochromogen in the absence of atmospheric oxygen. Catalase, however, inhibits the oxidation of hæmin, though not of vitamin. Incubating hæmin in dilute pyridine with ascorbic acid and a small amount of hydrogen peroxide in nitrogen, the formation of a brown-green compound with an absorption band at 639 mμ is observed; by its properties it can be clearly distinguished from acid hæmatin and from hæmochromogens possessing a similar absorption band. It is retransformed to hæmochromogen by hyposulphite, by catalase and more slowly by ascorbic acid alone, and is oxidized to ferric hæmochromogen (parahæmatin) by ferricyanide or by an excess of hydrogen peroxide. We conclude from these observations that the substance is a ferrous hæm-hydrogen peroxide compound containing pyridine and perhaps also ascorbic acid. In nitrogen it is relatively stable in presence of an excess of ascorbic acid, but on admittance of air it is rapidly oxidized to verdohæmochromogen. For the oxidative splitting of the porphyrin ring to the bile pigment chain the action of oxygen on the hæm-hydrogen peroxide compound (combined action of oxygen and hydrogen peroxide) appears to be necessary.

The absorption band at 639 mμ is also observed during the initial stages of the coupled oxidation of hæmochromogen and vitamin by atmospheric oxygen. Here the hæm-hydrogen peroxide compound may arise by the transfer of hydrogen (Wieland) from ascorbic acid to a hæmochromogen peroxide (Warburg) formed initially by the action of oxygen on hæmochromogen; or hydrogen peroxide may be formed first in a radical chain mechanism[4] and then combine with hæmochromogen. The formation of the hæm-hydrogen peroxide compound from hæmochromogen, oxygen and ascorbic acid, and its reaction with hydrogen peroxide and ascorbic acid (peroxidase action of the hæmochromogen) constitute a catalytic cycle in which hæmochromogen acts as oxidase on ascorbic acid.

At the moment we are not yet certain how great a part of the vitamin oxidation proceeds in this way. Ferric hæmochromogen oxidizes ascorbic acid and ferrous hæmochromogen is re-oxidized by molecular oxygen. 10^{-3} mol cyanide inhibits very little, but ascorbic acid can reduce cyanhæmatin in pyridine. We have, however, additional evidence that the vitamin oxidation cannot be explained on this basis alone. If vitamin were to act in the reduction process only, we should find a greater percentage of the hæmatin in the form of hæmochromogen, when a higher vitamin concentration is applied; the contrary is true. For this reason, it must be assumed that ascorbic acid catalyses the oxidation of ferrous hæm, which is to be expected from the reaction mechanism described above. Again, vitamin may be oxidized and ferric hæmochromogen (in addition to verdohæmochromogen) arise in the reaction of the hæm-hydrogen peroxide compound with oxygen.

The absorption spectrum of the new hæm-hydrogen peroxide compound differs totally from that of the methæmoglobin-hydrogen peroxide compound[5] and from the catalase-hydrogen peroxide compounds[6] of Keilin and Hartree. In its properties it seems more closely related to the former.

An absorption band at 639 mμ has been observed by Warburg and co-workers[7] when *B. pasteurianum* was exposed to air in presence of cyanide. There is reason to assume that it is due to the same substance. Cyanide inhibits catalase and causes the accumulation of hydrogen peroxide in the cell, thus producing conditions under which the substance is formed from protohæmochromogen. Quite different are the substances with a band at 628 mμ, occurring as cytochrome a_2 in several bacteria; they are probably biliviolin-hæmochromogens[8].

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Visual Purple System in Fresh-water Fishes

THE visual purple of mammals, birds, amphibians, and certain marine fishes [1,2] absorbs light maximally at 500 mμ and participates with retinene and vitamin A in a retinal cycle[3,4]. I shall refer to this substance specifically as *rhodopsin*[5]. The visual purple of freshwater fishes possesses different spectral properties[1]; I shall refer to it as *porphyropsin*.

I have examined the porphyropsin system in white perch (*Morone americana*), yellow perch (*Perca flavescens*) and pickerel (*Esox reticulatus*).

Isolated dark-adapted retinas from these fishes are purple in colour. Exposed to bright light they turn russet, then fade slowly to very pale yellow. The fading from russet to yellow occupies about ¾ hour at 25° C. and possesses a high temperature coefficient. Russet retinas, replaced in darkness, regenerate porphyropsin, though always markedly less than is present in the dark-adapted retina. The light process is therefore reversible. Completely faded retinas do not re-form porphyropsin; isolation of the retina from the eye abolishes this reaction.

The nature of these changes is revealed further in the following experiment. Right and left retinas of six dark-adapted white perch were prepared separately. One set was extracted by shaking with benzine in the dark, then was bleached in bright light and immediately re-extracted. The second set was bleached and allowed to fade completely before extraction. The three benzine extracts were brought into carbon disulphide and their spectra measured. They were concentrated in chloroform, and spectra of the blue products obtained on mixing with antimony trichloride were examined. The results are summarized in the following table:

Retinal condition	Colour	Benzine extract in CS_2	Antimony chloride reaction
(*A*) Dark-adapted	Purple	Faintest yellow: simple end-absorption in violet	Very small band at 696 mμ
(*B*) Bleached	Russet	Deep orange-yellow: end-absorption with inflection (band ?) at about 400 mμ	High band at 703 mμ
(*C*) Bleached and faded	Faint yellow	Light yellow: simple end-absorption	High band at 696 mμ; broad flat hump at 640 mμ

No trace of either vitamin A or retinene was found in these tissues (compare Wald[3]). The orange-coloured 703 mμ chromogen, liberated from porphyropsin by light (*B*), is transformed by the facing process into the yellow 696 mμ chromogen (*C*), a small quantity of which (about 1/5) is present also in the dark-adapted retina (*A*).

Solutions of porphyropsin from the three species examined possess identical properties. Spectra of a neutral preparation from white perch in 2 per cent aqueous digitonin, measured against an equal depth of distilled water, are shown in Fig. 1. Curve *g* is the spectrum of the solvent alone.

Porphyropsin in solution possesses a broad absorption band maximal at 522–525 mμ (curve *a*). The secondary rise in absorption following a minimum at 430–440 mμ has appeared consistently, but may be due to impurities. The preparation was exposed to bright light (about 700 ft. candles) for ½ minute.

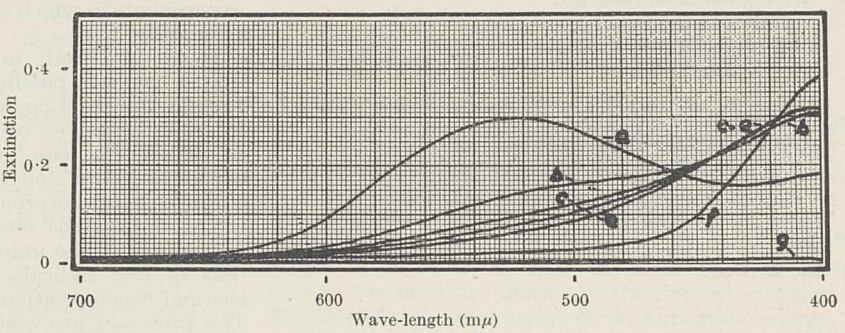

Fig. 1.

It was then left in complete darkness and its spectrum measured periodically, curve *b* at 1·2 min., *c* at 4·6 min., *d* at 12·5 min., and *e* at 64·5 min. from the onset of the irradiation. The initial orange-coloured photo-product (curve *b*) slowly turned to yellow, the extinction falling in the region of 520 mμ and simultaneously rising below 444 mμ. Dark processes in this instance account for 34 per cent of the total fall in extinction at 520 mμ.

The final yellow product (curve *e*) changes colour reversibly with *p*H, tending toward orange in acid and pale yellow in alkaline solution. The present residue, brought to *p*H 11, yielded curve *f*.

This final bleached product yields all its colour on shaking with benzine containing 1 per cent ethanol. The benzine extract, brought back into neutral aqueous digitonin solution, possesses a spectrum-like curve *e*. It changes in colour with *p*H, as does the whole bleached residue. It appears to be the same pigment as is extracted from bleached (russet) retinas (extract *B* in the table above).

If curve *e* is subtracted from *a*, a function roughly symmetrical about a maximum at 540 mμ is obtained, similar to that observed by Köttgen and Abelsdorff[1]. Below 455 mμ this function assumes negative values. Obviously it does not, as commonly supposed, represent the spectrum of porphyropsin itself.

In every detail so far examined, the porphyropsin system faithfully reproduces the behaviour of the rhodopsin system, but with quite different components. It appears to consist in a retinal cycle of the form: porphyropsin $\underset{(1)}{\overset{\text{light}}{\rightleftarrows}}$ russet pigment (703 mμ chromogen + . . .) $\underset{(2)}{\rightarrow}$ yellow pigment (696 mμ chromogen + ...) $\underset{(3)}{\rightarrow}$ porphyropsin. Isolation of the retina from the eye cuts the cycle at (3); extraction of porphyropsin from the retina eliminates in addition reactions (1) and (2).

It is curious that the possession of porphyropsin divides all freshwater fishes so far examined—eleven species in all—even from certain very closely related marine forms[2,3]. The division may not be absolute, for recently a number of marine fish visual purples have been reported to possess intermediate properties[6]. These are possibly mixtures of rhodopsin and porphyropsin. The coincidence in a single retina of a number of visual systems accurately parallel in behaviour but differing in spectral properties should serve as an appropriate basis for colour vision.

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March 31.

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Thermal Decomposition of Ethylene Oxide

IN a series of papers communicated to the Faraday Society, an attempt has been made to prove that the thermal decomposition of a number of simple organic compounds may be accounted for on the assumption of the following mechanism.

The compound *A* first undergoes change in accordance with an equation,

$$A = B\ (+ C). \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This process, which is called the *background reaction*, is readily reversible when *A* and *B* represent ethane and ethylene, propane and propylene, and probably acetaldehyde and vinyl alcohol, but in the case which we are considering it is sensibly irreversible.

The main reaction, following on the background reaction, is represented by the equation,

$$A + B = AB = (AB) = C + D, \text{ etc.} \quad . \quad (2)$$

where *AB* and (*AB*) represent the activated complex, and what has been termed an *unstable intermediate*. The latter may decompose, or even react with some other substance such as hydrogen, to form one or more products, but it is the rate of formation of the unstable intermediate which we measure.

Further study of the thermal decomposition of ethylene oxide furnishes confirmation of this theory. This compound decomposes in the neighbourhood of 400° according to the equation,

$$(CH_2)_2O = CH_4 + CO. \quad . \quad . \quad . \quad . \quad (3)$$

A little acetaldehyde is formed at the same time, and it has been suggested that this compound is an intermediate between ethylene oxide and the final products. However, it seemed likely, according to the theory referred to above, that the reaction,

$$(CH_2)_2O = CH_3CHO, \quad . \quad . \quad . \quad . \quad (4)$$

might really be the background reaction for the decomposition of ethylene oxide, in which case the main reaction would be initiated by the collision of molecules of ethylene oxide and acetaldehyde, represented by,

$$(CH_2)_2O + CH_3CHO = [(CH_2)_2O, CH_3CHO] = 2CH_4 + 2CO. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

To test this theory, quantities of ethylene oxide and acetaldehyde were heated separately in a silica tube for 30 minutes at 400°. In a third experiment the same quantities of ethylene oxide and acetaldehyde was heated similarly, mixed together. As was anticipated, the quantity of carbon monoxide and methane obtained from the mixture was about ten times the sum of the quantities obtained in the experiments in which the two compounds were heated separately.

So far, it is only in the case of the hydrocarbons that it has been possible to eliminate the effect of the background reaction, and to study the main reaction by itself. However, by experimenting at temperatures at which ethylene oxide and acetaldehyde alone decompose very slowly, it is found to be possible to study in detail the process represented by equation (5).

This hypothesis enables us to dispense with the conception of the unimolecular reaction, and also of that of free radicals, in order to account for the thermal decomposition of simple organic compounds. The processes are accounted for on the supposition that the reactions involve simple bimolecular processes.

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Inhibitions of Organic Decompositions by Nitric Oxide

STAVELEY and Hinshelwood have recently shown that the thermal homogeneous decompositions of certain substances such as ethers and aldehydes are to a greater or less extent inhibited by small amounts of nitric oxide, and they have used this as a means of establishing the extent to which these reactions

proceed via a mechanism of reaction chains[1]. With dimethyl ether the effect is most marked, and in this case, when the decomposition is about half completed, the reaction velocity rises fairly abruptly to its 'normal' value.

We have attempted to discover the fate of the nitric oxide by photographing the absorption spectrum of the gaseous reaction mixture at intervals during the course of a run. About ten photographs were usually taken, with exposures of some seconds each, in a run of about thirty minutes in all. The spectrograph was a small quartz - fluorite instrument with dispersion 2000–7000 A. and the source was a hydrogen discharge.

With dimethyl ether at the beginning of the run, only the doublet nitric oxide bands at 2265 and 2151 A. were observed. As the reaction proceeds, these bands gradually weaken, and are replaced with increasing intensity by the strongest bands of ammonia at 2210, 2166, 2127, etc. . . . The stage of the reaction at which the nitric oxide disappears agrees entirely with that expected from the kinetic measurements. This seems to indicate that whatever the primary stage in the interaction of radicals with the nitric oxide may be, a final product, which does not affect the rate of the reaction, is ammonia. The process

$$NO + CH_3 \rightarrow CO + NH_3$$

would be expected to be exothermic, but we regard this process (occurring in one stage) as unlikely. Other experiments shortly to be described suggest that formaldoxime may be an intermediate product, while the condensation from the reaction mixture by liquid air of a blue product may indicate an unstable nitroso compound.

When the above experiment is repeated using diethyl ether, the nitric oxide bands are gradually replaced as the run proceeds by a continuous absorption which extends towards 2300 A. from shorter wave-lengths, and there are indications of a few feeble diffuse bands in the region of 2350 A. This result might well arise from the presence of methylamine, formed by the process :

$$C_2H_5 + NO \rightarrow (X) \rightarrow CH_3NH_2 + CO$$

The experiments are being extended to other cases such as the decomposition of alkylene oxides.

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[1] NATURE, **137**, 29 (1936); *Proc. Roy. Soc.*, A, **154**, 335 (1936); A, **159**, 192 (1937); *J. Chem. Soc.*, 812, 818 (1936).

Photochemical Reduction of Ceric Ions by Water

IT has been shown previously that in the absorption of ultra-violet light by several ions in aqueous solution, the photochemical primary process consists in a transfer of an electron from the ion to a water molecule, with a consequent evolution of gaseous hydrogen. This was shown to occur both with negative ions[1] and positive ions[2].

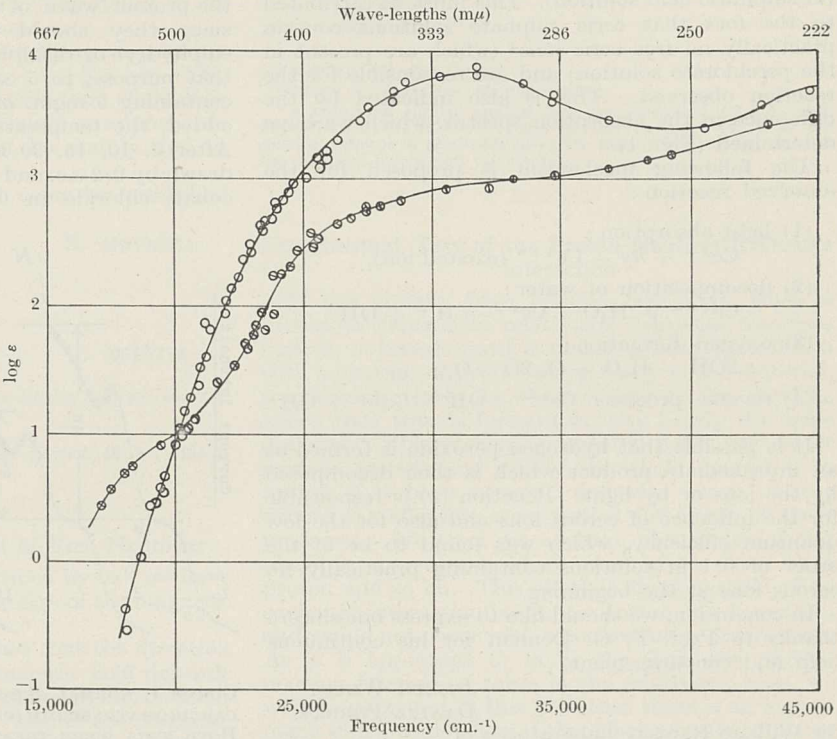

Fig. 1.

MOLECULAR EXTINCTION COEFFICIENTS OF CERIC PERCHLORATE (Ø) AND CERIC SULPHATE (O).

Some form of inverse process has been shown to occur in the elementary process of the quenching of fluorescence of dyes and in the photo-reduction of dyes in solution[3]. By exciting the dye molecule an electron goes into a higher level, leaving an empty electronic level behind into which an electron from a neighbouring molecule may be taken up. In this process the electron affinity of the excited molecule is increased by the amount of its excitation energy.

Our experiments have shown that a similar photochemical process can take place between ceric ions and water. By irradiating an aqueous solution of ceric perchlorate with the ultra-violet light of a strong mercury arc (described previously[2]) we found a considerable evolution of a gas which was shown to be pure oxygen. For example, in one of our experiments, which was carried out with a solution containing 0·1 mol ceric perchlorate (complete absorption) and about 1 mol perchloric acid per litre, 2 c.c. of oxygen (N.T.P.) were evolved per litre per minute at the beginning. The speed of the reaction decreases while the reaction is going on. This was found to be due to the formation of cerous ions, this view being confirmed by experiments where cerous salt was added directly. Cerous ions also appear in the presence of

traces of organic substances in the solution which are easily oxidized by ceric ions.

The rate of the reaction remains unchanged by filtering the light through 20 per cent perchloric acid solution. The perchlorate ion, in fact, is practically non-absorbing in the near ultra-violet and is also not decomposed to any appreciable extent under the conditions in question. We did not observe the reaction on irradiating a solution of ceric sulphate (in sulphuric acid solution). This must be attributed to the fact that ceric sulphate solutions contain practically no free ceric ions[4] (which are present in the perchlorate solution) and are responsible for the reaction observed. This is also indicated by the difference in the absorption spectra, which we have determined (Fig. 1).

The following mechanism is proposed for the observed reaction :

(1) light absorption :
$Ce^{4+} + h\nu = Ce^{4+*}$ (excited ion).

(2) decomposition of water :
$Ce^{4+*} + H_2O = Ce^{3+} + H^+ + OH.$

(3) oxygen formation :
$2OH = H_2O + O$, $2O = O_2$; or

(4) reverse process : $Ce^{3+} + OH = Ce^{4+} + OH^-$.

It is possible that hydrogen peroxide is formed as an intermediate product which is then decomposed by the ions or by light. Reaction (4) is responsible for the influence of cerous ions and also for the low quantum efficiency, which was found to be of the order of 0·1 in solutions containing practically no cerous ions at the beginning.

In conclusion, we should like to express our sincere thanks to Prof. F. G. Donnan for his continuous help and encouragement.

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[1] Franck, J., and Haber, F., *Sitz. Preuss. Akad. Wiss.*, 250 (1931).
[2] Weiss, J., NATURE, **136**, 794 (1935). See also Poterill, Walker and Weiss, *Proc. Roy. Soc.*, A, **156**, 561 (1936).
[3] Weiss, J., *Trans. Faraday Soc.*, **32**, 1331 (1936). Weiss, J., and Fishgold, H., *Z. phys. Chem.*, B, **32**, 135 (1936).
[4] cf. Williams, E., Ph.D. thesis, University of Wales (1931).

Polarographic Investigations in Serological Cancer Diagnosis

IT has been mentioned in these columns[1] that the polarographic 'wave' produced on the current-voltage curve by sulphydryl or disulphidic groups of serum proteins can be used to distinguish between normal and carcinomatic serum. In the case of cancer the height of this characteristic protein 'wave' is always found a little lower than that in the normal serum. However, when the sera are treated with alkali hydroxide, the height of the protein 'wave' increases in the carcinomatic serum less than in the normal serum. Thus the differences between them are considerably increased. This circumstance has to be ascribed to the liberation of sulphydryl or disulphidic groups of serum proteins as the result of their alkaline denaturation. (Cf. Mirsky and Anson[2].)

The same increase of the height of the protein 'wave', leading to a similar difference between carcinomatic and normal serum, can be obtained when the proteins are denatured with diluted hydrochloric acid (*c.* 0·05 *N*). The denaturation under these conditions proceeds at room temperature very slowly, but can be hastened at a higher temperature (*c.* 40° C.).

A polarographic study of the kinetics of the peptic cleavage of native and coagulated proteins has revealed an increase of the protein 'wave'. This effect has now been applied to show the differences between the protein 'wave' of carcinomatic and normal serum, since they should be largest when the active sulphydryl or disulphidic groups are liberated. For that purpose, to 5 c.c. of 0·05 *N* hydrochloric acid containing 5 mgm. of pepsin, 0·2 c.c. of serum was added, the temperature being maintained at 40° C. After 5, 10, 15, 30 minutes the solution was withdrawn by 0·2 c.c. and added to 10 c.c. of $1·6 \times 10^{-3}$ *N* cobalt chloride in 0·1 *N* ammonium chloride in

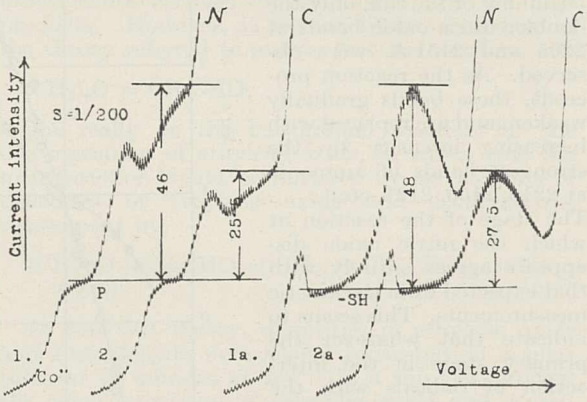

Fig. 1.

CURVE 1, NORMAL SERUM, 'WAVE' $P = 46$ MM. CURVE 2, CARCINOMATIC SERUM (CANCER UTERI), 'WAVE' $P = 25·5$ MM. BOTH SERA WERE TREATED WITH PEPSIN AS INDICATED ; DILUTION OF SERUM = 1 : 1,250.

CURVE 1*a*, HYDROLYSATE OF NORMAL SERUM, CYSTINE 'WAVE' = 48 MM. CURVE 2*a*, HYDROLYSATE OF CARCINOMATIC SERUM, CYSTINE 'WAVE' = 27·5 MM. ; DILUTION OF SERUM = 1 : 2,000.

0·1 *N* ammonia. The polarographic curve due to this solution showed the expected increase of the protein 'wave'. Thus this method of procedure leads to a quick differentiation—in about 15 min.—between carcinomatic and normal serum. From the material examined polarographically up until now (more than 250 different samples), the height of denatured protein 'wave' in carcinomatic cases is smaller by 3–50 per cent than the height of that from normal serum. The decrease depends on the stage of development of cancer. It must be remembered, however, that some acute cases of inflammation and fever yield a serum reaction similar to that of cancer.

From these experimental facts two questions arise : namely, what is the origin of the difference between carcinomatic and normal serum, and whether disulphidic or sulphydryl or both groups are responsible for the polarographic effect of proteins ?

To solve these problems the sera were completely hydrolysed with 5 *N* hydrochloric acid (1 c.c. of serum boiled with 20 c.c. of 5 *N* hydrochloric acid for 8 hours and made up to 100 c.c. with water) and the hydrolysates were submitted to polarographic analysis for cystine by my method[3]. In the hydrolysate

of the carcinomatic serum there was always found less cystine than in the normal one. The ratio of the cystine concentration agrees with the ratio of the polarographic 'waves' of the denatured or proteolysed serum proteins (see Fig. 1).

These results were controlled by a colorimetric estimation of cystine in the hydrolysates using the Folin-Looney uric acid reagent as modified by Mirsky and Anson[4]. Thus in carcinomatic serum the content of the protein cystine was found to be smaller than in the normal serum.

As to the question whether the group responsible for the polarographic reaction is —S—S— or —SH, polarographic as well as colorimetric tests show that the sulphydryl groups are not present in an appreciable amount, so that the effect must be mainly due to the disulphidic groups.

Details of this research will soon be published elsewhere.

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[1] NATURE 139, 330 (1937).
[2] Mirsky, A. E., and Anson, M. L., *J. Gen. Physiol.*, **19**, 427 and 439 (1936).
[3] Brdička, R., *Collection*, **5**, 238 (1933).
[4] Mirsky, A. E., and Anson, M. L., *J. Gen. Physiol.*, **18**, 307 (1935).

Sign of the Magnetic Moment of Free Neutrons

USING a method recently described by us[1], we have now succeeded in determining the sign of the magnetic moment of free neutrons.

The method is based on the fact that the direction of precession of particles in a magnetic field depends on the relative direction of spin and magnetic moment (this is the meaning of the term "sign of the magnetic moment"). The direction of precession can be observed by using bars of magnetized iron as polarizer and analyser[2], which are placed at an angle relative to each other[1, 3].

The experimental arrangement was essentially the same as before[1] except that one of the iron rings was rotated through an angle of 90° with respect to the other, with the neutron beam as axis, making the polarizing and analysing fields perpendicular to each other and to the neutron beam.

A short solenoid was placed between polarizer and analyser, coaxial with the neutron beam. The field in the solenoid (which was found to be fairly homogeneous, on account of the iron below and above) was adjusted so that the neutrons should rotate by 90°, on the average, on passing through the coil. (For calculating this field the magnetic moment of the neutrons was assumed to be 2 N.M.). The neutrons leaving the polarizer in the 'parallel' position arrived at the analyser in the 'parallel' or 'antiparallel' position, depending on the direction of precession, with correspondingly different probabilities of getting through.

The current in the solenoid was reversed at short intervals, and 480,000 counts in all were taken. An unambiguous difference of $(1{\cdot}10 \pm 0{\cdot}29)$ per cent of the total intensity (of which about 50 per cent was due to thermal neutrons) was found on reversing the current. The magnitude of the difference was of the order expected, which shows that the field in the solenoid was correctly chosen.

From the sign of the difference we conclude that the magnetic moment of the free neutron is negative that is, the relative direction of spin and magnetic moment is the same as in the electron. This agrees with expectations based on the magnetic moments of proton and deuteron.

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[1] NATURE, **139**, 756 (1937).
[2] Bloch, F., *Phys. Rev.*, **50**, 259 (1936).
[3] A somewhat similar method has been proposed by I. I. Rabi, *Phys. Rev.*, **51**, 652 (1937). It may be well to point out that the classical concept of precession gives the same result, in our case, as the quantum mechanical calculation.

Experimental Test of the Proton-Neutron Exchange Interaction

IT has already been pointed out[1] that, when a proton or neutron of relativistic velocities traverses matter, it travels until it makes a head-on collision with a neutron or proton respectively in the material, transferring nearly its whole energy to this particle, which then travels forward in very nearly the same direction. This process is repeated again, the latter particle transferring nearly its whole energy to a proton if it be a neutron, and vice versa. Let q denote the effective cross-section for either of these processes. We thus get a chain, the proton giving place to a neutron, which then gives place to a proton and so on. This effect is characteristic of an exchange interaction between a proton and a neutron, and would not occur with a non-exchange interaction. As it is important to know the form the proton-neutron interaction takes in the relativistic case, we wish to point out in this note that there is an experiment which could show if the effect mentioned above does or does not exist.

Suppose we have three counters in a vertical plane, d_1 and d_2 being the distances between 1 and 2 and 2 and 3 respectively. Let us suppose that a proton passes through the topmost counter 1 in the right direction to pass through the other counters. It can then be shown easily that the chance of a proton passing through the counter 2 is

$$(\sigma_p + \sigma_n e^{-\sigma q d_1})/\sigma,$$

where σ_p and σ_n are the number of protons and neutrons per c.c. of the substance respectively, and $\sigma \equiv \sigma_p + \sigma_n$. The chance of the counters 1, 2 and 3 being traversed simultaneously by protons is then

$$(\sigma_p + \sigma_n e^{-\sigma q d_1})\,(\sigma_p + \sigma^n e^{-\sigma q d_2})/\sigma^2,$$

and the chance of protons going through 1 and 3 irrespective of whether a proton or neutron traverses 2 is

$$\{\sigma_p + \sigma_n e^{-\sigma q (d_1 + d_2)}\}/\sigma.$$

The ratio of triple coincidences in 1, 2, 3 to double coincidence between 1 and 3 is therefore

$$R(d_1,d_2) \equiv \frac{(\sigma_p + \sigma_n e^{-\sigma q d_1})\,(\sigma_p + \sigma_n e^{-\sigma q d_2})}{\sigma\{\sigma_p + \sigma_n e^{-\sigma q (d_1 + d_2)}\}} \qquad (1)$$

If

$$\sigma q d_1 \gg 1, \quad \sigma q d_2 \gg 1, \qquad (2)$$

then the above ratio is $\sigma_p/(\sigma_p + \sigma_n)$, and if either d_1 or d_2 is zero it is unity, as is otherwise obvious. Thus

by measuring the ratio of the number of triple to double coincidences between three counters with suitable thicknesses of some heavy material between them, it should be possible to see if the proton-neutron chain mentioned above exists or not.

Dr. Jacobsen and Prof. Clay have independently measured this ratio with d_1 and d_2 corresponding to 10 cm. of lead, and have found that it is unity to within the accuracies of their experiments. If q have the value calculated using the proton-neutron interaction[1] found from considerations of nuclear stability[2], namely, $0{\cdot}16 \times 10^{-24}$ cm.^{2}, then the condition (2) is satisfied for 10 cm. of lead, and the ratio R should be 0·4 instead of 1. But the above result is not conclusive, since, as we have already pointed out[1], this value of q may easily be in error by a factor of four, as the interaction we have used is not relativistically invariant. We have, however, carried out the above calculation for the scattering both in the system where one of the particles is initially at rest, and in the centre of gravity system, and shown that even in the extreme relativistic case the two cross-sections only differ by a factor four, so that superficially the lack of relativistic invariance of the interaction does not seem to be so serious. In order to get a more definite result, the above experiment should therefore be repeated with at least 50 cm. of lead between each pair of counters.

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[1] Bhabha, H. J., NATURE, **134**, 934 (1934).
[2] Wick, G. C., *Z. Phys.*, **84**, 799 (1933).

Points from Foregoing Letters

DR. O. ROSENHEIM and Dr. H. King state that they have converted the primary oxidation product of cholesterol, an unsaturated α-glycol, by further mild oxidation into a most reactive substance of the diosphenol type, called diosterol. In its unusual reducing capacity and other properties, diosterol resembles the cortical adrenal hormone. It is suggested that the latter may arise from cholesterol by stepwise oxidation through the α-glycol, thus explaining the position and function of the hitherto unplaced fourth oxygen atom.

Experiments by P. de Fremery and B. Scheygrond show that the enlargement of the uterus, œstrus and luteinization, brought about by injection of gonadotropic substances in immature female rats, can be completely inhibited by simultaneous injection of antigonadotropic rabbit serum. This latter is obtained by repeated injection of a negligibly gonadotropic extract of male urine in a rabbit.

The oxidation of hæmochromogen (a constituent of the blood), accompanying the oxidation of ascorbic acid when the two are brought together, has been investigated by Dr. R. Lemberg, B. Cortis-Jones and Norrie. The authors discuss the mechanism of this reaction, which leads to the formation of verdohæmochromogen (a bile pigment iron compound) and may throw light upon the physiological hæmoglobin breakdown and the formation of bile pigment.

The behaviour of porphyropsin (a dark purple pigment extracted from the retina of freshwater fishes) on exposure to light has been investigated by Dr. G. Wald, by following the light absorption of porphyropsin solutions under various conditions. The author concludes that, under the influence of light, porphyropsin in the retina of the fish goes through a cycle of changes similar to that previously shown to take place in the case of rhodopsin, the corresponding visual purple pigment in mammals, birds and certain marine fishes.

If ethylene oxide and acetaldehyde are heated together at 400° C., they yield ten times more carbon monoxide than when heated separately. This fact, according to Prof. M. W. Travers and C. G. Silcocks, indicates that in the thermal decomposition of ethylene oxide, although only small amounts of acetaldehyde are detected, nevertheless the intramolecular transformation of ethylene oxide into acetaldehyde actually forms the background reaction which precedes the final decomposition.

Recent work has shown that nitric oxide can inhibit the homogeneous decomposition of some organic molecules, presumably by breaking reaction chains. This may be due to the interaction of the nitric oxide with free alkyl radicals. By following the change in the absorption spectrum of the reaction mixture, Dr. H. W. Thompson and M. Meissner find that in the inhibition of the decomposition of dimethyl ether by nitric oxide, ammonia is formed as the nitric oxide disappears. In the decomposition of diethyl ether, the nitric oxide bands are gradually replaced by a continuum and feeble bands indicative of the presence of methylamine. Both these results are understandable in terms of the reaction of nitric oxide with the methyl and ethyl radicals.

Dr. J. Weiss and Dr. D. Porret report the liberation of oxygen by ultra-violet light in solutions of cerium perchlorate without appreciable decomposition of the perchlorate ion. The authors ascribe the reaction to a photochemical process during which a ceric ion is first excited and then gives up an electron to the water molecule, becoming itself reduced to the cerous state. Hydrogen peroxide may be formed as an intermediate product.

Further experiments on his 'polarographic' test for proteins are reported by Dr. R. Brdička. This test, the author claims, is of value in cancer diagnosis, though some acute cases of inflammation and fever give a similar reaction. He ascribes the effect upon the polarographic curve to the disulphidic groups.

By studying the direction of precession of 'polarized' neutrons in a magnetic field, Dr. O. R. Frisch, Dr. H. von Halban and Dr. Jørgen Koch have found that the magnetic moment of free neutrons is negative, that is, the relative direction of spin and magnetic moment is the same as in the electron. This agrees with expectations based on the magnetic moments of proton and deuteron.

H. J. Bhabha points out that the existence of the exchange interaction between protons and neutrons of high velocity (comparable with that of light), predicted on theoretical grounds, could be checked by measuring the ratio of the number of triple to double coincidences between three 'counters' with at least 50 cm. of lead between each pair of 'counters'.

Research Items

Rise and Decline of Populations

THE Galton Lecture of Mr. J. M. Keynes at the annual dinner of the Eugenics Society, on some economic consequences of a declining population, is printed in the *Eugenics Review*, **29**, No. 1. The enormous increase of capital during the nineteenth century was partly a result of increasing population, although standards of life and the average period of production were also involved. This period was not lengthened by much more than ten per cent during the fifty years before the Great War, while the annual increment in the standard of life has seldom been more than one per cent. Therefore in a stable population, equilibrium conditions of prosperity can only be maintained by causing a smaller proportion of income to be saved, or by so reducing the rate of interest as to make profitable very large changes in technique or in consumption which involve a much larger use of capital in proportion to output. For the maintenance of prosperity and internal peace we must depend on policies of increasing consumption by more equal distribution of incomes and forcing down rates of interest. A rapidly declining population would involve many severe problems, but even in a stationary population we should be more exposed to unemployment of resources. Sir Walter Layton, in his remarks, agreed with Mr. Keynes's general conclusions, but pointed out that a declining population furnished the best conditions for improving their eugenic quality. Mr. H. D. Henderson stated that the chance of increasing the standard of life in a stationary population is likely to be missed through increasing expenditure on armaments, and concluded that the fall of the birth-rate in Western countries had already gone dangerously far.

Fossil Antelopes and Oxen of the Siwalik Hills

FEW areas have shed so much light upon the development of Tertiary ungulates as the Siwalik Hills of India, and Guy E. Pilgrim's account of the antelope and oxen remains in the American Museum of Natural History adds much to our already extensive knowledge of that fauna (*Bull. Amer. Mus. Nat. Hist.*, **71**, 729–874; 1937). The results, drawn mainly from the collections made by Dr. Barnum Brown, throw light on many hitherto obscure problems of identity, affinity and taxonomy. This is particularly so in relation to the Boselaphine group, in which six new genera have been described, and the additional material has enabled the author to redefine the family as characterized by horn-core having a sub-triangular cross-section, produced by a strong anterior and weaker posterior keel united by a flat or slightly convex surface, and a third less definite angle. It has also added to our knowledge of the relationships of the living nylghai, and suggests that the developmental centre of the Tragelaphinae was not in Europe, where the fossil forms show a more advanced stage of evolution than the Siwalik fossils, but that they probably migrated thence from a region still undiscovered, probably in or near northern Africa.

A Puzzling Fossil

THE fossil remains of a very puzzling organism are described by D. J. Scourfield (*Proc. Roy. Soc.*, B, Feb. 1937) and it is aptly named *Aniktozoon loganense* (ἀνικτός). Twenty-nine specimens, some of which are in an excellent state of preservation, are used as the basis for the description. They come from the Ludlow Beds of Logan Water, Lanarkshire, almost certainly from the same horizon as *Ceratiocaris* and *Pterygotus* and were collected in and around 1904. At first sight, the clear photographs accompanying the paper suggest a crustacean, but, as the author demonstrates, their reference to this group or indeed to any other group of the Arthropoda cannot be supported by detailed examination. Since they were obviously segmented, the only other known possibilities are the Annelida and the Chordata. They do not agree with the former. The suggestion is put forward that they are to be regarded as representing a lowly chordate stock allied to the Tunicata, but unlike any of the existing forms.

Growth-Rate of *Trochus niloticus*

A DETAILED investigation of the rate of growth of this species in the Andaman Islands has been made by Dr. H. S. Rao (*Rec. Indian Museum*, **38**, 473; 1936). It was found from a statistical analysis of many measurements that the rate of growth in the shell bears an inverse relationship to age as determined by the maximum diameter of the shell between certain stages. The monthly rates of growth at each stage, taken with other facts in the life-history, suggest that in the Andaman Islands the full age may exceed ten years, the diameter from the eighth to the eleventh years ranging from 12 to 14 cm. This is a marked difference from the age estimates of *T. niloticus* which have been made on the Queensland coast of Australia, but Dr. Rao, accepting the differences, attributes them to differences in the conditions of growth in the two areas. In the Andamans, the rate of the growth of female individuals exceeds that of males, but here also, as Dr. A. P. Orr found in the waters of the Great Barrier Reef, the seasons in tropical seas do not appear to cause any differential growth.

Polychætous Annelids of Japan

THE examination of an extensive collection of polychætous annelids made by Prof. Okada in the neighbourhood of Seto, Japan, has enabled P. Fauvel (*Mem. Coll. Sci. Kyoto*, Sept. 1936) to add a new species, *Eunice ovalifera*, and to review the Japanese representatives of the group. In all, eighty-two species have been recorded by various workers; the present collection includes seventy of them. As might be expected from the geographical position of the country, its polychæte fauna indicates a mixture of arctic, temperate and tropical forms. In addition to cosmopolitan species, nineteen are peculiar to the Japanese and neighbouring seas, fifteen are boreal or arctic and twenty-six are tropical. It is

suggested that the pelagic larval stages of many of them may have been introduced by the great ocean currents reaching the coast. The Kuro-Siwo brings up the warm water from the tropical Pacific to the west coast, and the Oja Siwo comes down from the Arctic Ocean to the north coast.

Cytology of Brassica Hybrids

CROSSES between radishes and cabbages have been made previously in Russia and Japan, and no chromosome pairing was found in the F_1 hybrid meiosis. Dr. R. H. Richharia (*J. Gen.*, **34**, No. 1), in a study of the F_1 hybrid between radishes and Brussels sprout, found a maximum of six bivalents, both parents having $2n=18$ chromosomes. This difference in chromosome pairing may be ascribed to the different varieties used. Brussels sprout, curly kale and radish all showed 9 bivalents regularly in diakinesis, while the cabbage had a ring or chain of 4. The maximum number of secondary associations was five in curly kale and four in the radish. Twenty-one F_1 hybrids were examined at meiosis, and about half were hypotetraploids with frequent trivalent and tetravalent chromosomes, the hybrids showing great variation. Six principal types of chromosomes can be distinguished in *Raphanus sativus* and *Brassica oleracea*, as well as in *B. chinensis*, *B. Rapa* and *B. pekinensis* which have $2n=10$ chromosomes, and their F_1 hybrids. The last three species show five large and five small bivalents at meiosis, except that in *B. Rapa* the distinction is less marked. In *B. pekinensis* × *B. Rapa* there are occasionally two free univalents, and 'non-disjunction' sometimes takes place. Studies of secondary pairing in these species did not lead definitely to a primary basic number.

Subsidence of East Tokyo

IN some parts of Tokyo, and especially in the districts of Hukagawa and Honzyo on the east side of the River Sumida, recent re-levellings show that there has been a notable sinking of the surface (NATURE, **137**, 281; 1936). In the centre of the city, in the Marunouti district, as Prof. N. Miyabe points out (*Bull. Earthq. Res. Inst.*, **15**, 102; 1937), many buildings have apparently risen above the ground, one at the rate of nearly 3 in. a year. In both regions, a layer of alluvial soil, 108 ft. thick in the former and 65 ft. in the latter, rests on a hard bed of tertiary rock. A little more than a year ago, two special bench-marks were installed in Hukagawa and Honzyo. They were fixed into the tops of tubes driven down into the tertiary bed. Precise levellings carried out in April and October 1936 showed that the ground-level was lowered 1–2½ in. with reference to the surface of the tertiary layer. In the Marunouti district, the buildings rest on piles also penetrating the tertiary rocks. Levellings carried out in August 1929 and October 1936 reveal a depression of very nearly the same amount as the apparent uplift of the buildings. Thus, Prof. Miyabe concludes that the subsidence is due, not to any downward warping of the crust, but to the contraction of the surface soil.

Magnetic Properties of Silver Difluoride

THE magnetic properties of silver difluoride, AgF_2, are of interest in connexion with the question of the arrangement of the atomic linkings in this compound. The corresponding oxide, AgO, is practically non-magnetic, and previous determinations have shown that the difluoride has only a very small susceptibility. This result has now been confirmed (E. Gruner and W. Klemm, *Naturwiss.*, 25, 59; 1937); but it has also been found that at low temperatures (− 183°) much higher values of the susceptibility are obtained, the substance becoming weakly ferromagnetic, with a Curie temperature of about − 110°. It is considered that the compound contains Ag.Ag linkings, the spin moments being anti-parallel at ordinary temperatures, becoming, in part, parallel at lower temperatures.

Steam Tables

THE issue of the *Journal of Research of the Bureau of Standards* of April contains a paper of sixty pages by N. S. Osborne, H. F. Stimson and D. C. Ginnings in which their determinations of the properties of water and steam between 100° and 374° C. are described. The work was rendered necessary by the extended use of higher pressures and temperatures in steam engineering, and the results obtained have been co-ordinated by international conferences in order to secure closer agreement in the steam tables embodying recent international research. The method used is to heat electrically a known mass of water and steam in an insulated calorimeter, observing temperature and pressure of the contents and the energy communicated. This is done either with the contents fixed or with withdrawal of liquid or vapour at will. Tables are given of the principal data from each type of experiment, and the final results are embodied in tables of the specific volumes of liquid and vapour in c.c. per gram, the saturation vapour pressure in kilos per sq. cm., the latent heat, the enthalpy (total heat), and the entropy of liquid and of vapour in international steam table calories (4·18605 international joules) per gram and the Clapeyron factor $T\frac{dp}{dT}$ at 5° C. intervals from 100° C. to 365° C. and at smaller intervals to 374·15° C., the critical temperature.

Characteristic Curves for Meteors

THIS is the title of a very interesting paper by Mr. J. G. Porter (*J. Brit. Astro. Ass.*, 47, 6, April 1937), in which he investigates various problems connected with the size, velocity and composition of meteors, and especially the relationship between these and the luminosity. He carries out a very exhaustive analysis, basing his formula on the atmospheric resistance on Davidson's formula (*J. Brit. Astro. Ass.*, November 1913). Mr. Porter finds that there should be a difference of about ten miles in height in the appearance of the fast and slow meteors, that is, those moving with velocities 40 and 10 miles a second, respectively, and the results of the Arizona Expedition largely corroborate this theoretical deduction. Meteors seen at low heights are shown to have lost a considerable fraction of their velocity, and must also be fairly large, generally speaking. It does not follow that they are very bright, because some of them may be of the type of the pyroxene and olivine minerals, possessing high volatility and low luminosity when heated. Mr. Porter hopes to continue his investigation, especially on the question of velocities, from which he may be able to form an opinion on the relative frequency of hyperbolic orbits.

Modern Aristotelianism

By Prof. H. Levy, Imperial College of Science, London

DR. H. DINGLE's plea in NATURE of May 8 for a return to experimental criteria in science, is timely. One may differ from him over the grounds upon which it rests, or over the stress he places on certain points : it is difficult to imagine any genuine disagreement with his main contention.

It must be realized, however, that since the days of Galileo, much has happened in science, not only in the extent of the fields that have been explored, in the nature of the instruments that have been designed to facilitate investigations, but also in our understanding of the relation between the subject matter of science, and those who pursue its study. It is never easy for those who are absorbed in the detailed technique to appreciate the philosophy and the methodology underlying their practice, or even to recognize when and why they are establishing new traditions. To-day we are witnessing such changes, and Dr. Dingle's protest will have been effective if it has the effect of forcing us to examine, in an objective way, the path that is being trodden.

Mathematics is one of the instruments the power of which since the days of Galileo has reached a completely new level. Its success in analysing the relations between empirical data, and in exploring the implications of speculative theories, has tended so to impress the mind, not of the mathematician alone, as to confer on it almost the status of an independent and self-constituted science. It is in directing attention to this dangerous intoxication with symbols that Dr. Dingle's protest is well-founded.

This situation is, however, not to be met merely by a reiteration of the Galilean basis of science, as Dr. Dingle does, or by falling into the error of identifying physics with the whole of science, as Dr. Dingle, following the lead of the mathematician, also does, but demands a positive statement of the place of mathematics and its attendant speculations in the process we call science.

For mathematics is essentially an *instrument* of discovery, and no instrument that has to tell us about the behaviour of the world can function in a vacuum. It works on something, and that something must be extracted from the experience of men in the objective world. Now the trouble begins to show itself in the first instance in the fact that the mathematician can handle ideas and speculations apparently divorced from any empirical background. He starts with *given* assumptions convenient to himself and his mathematical equipment. I say *apparently* with deliberation, because since the mathematical craftsman is primarily interested in the logical coherence of his analysis, he and those others who cannot follow his intricate symbolism, find themselves less and less disposed to examine the origin of the ideas on which he is experimenting with his mathematics. "Mathematics," he would say, "is the formulation of rational relations between any given set of ideas." He does not inquire whence the ideas come, what material circumstances in the world about him have stimulated them, how far they are a direct reflection of the changing situation in the objective world, or how far they are simply a novel combination of such stimulated ideas. As soon as this issue is raised, we can see how futile it is to tilt at the *a priori* principles of the Aristotelians ; there are none. What has to be exposed to view is the human experiential basis on which the principles unconsciously rest.

If it is true that the *given* ideas of the mathematician do not come as a miracle out of the void, that they are in some sense 'transformed' observations, then whatever assertions he may make that he can dispense with observations, he is nevertheless pursuing an aspect of inquiry subsumed under the phase—"The formulation of rational relations between 'transformed' sense observations". How close this seems to be to Dr. Dingle's position, judging from his statements, as he judges from those of Prof. Milne, may be seen by comparing this phrase with his own definition of science :

"Science is the formulation of rational relations between sense observations."

Like Sir Arthur Eddington, however, Dr. Dingle is better than his definition, for what he is really pleading for is something slurred over in his definition, the conscious interplay and mutual directiveness between theory and experiment. Every theory and every experiment rests on an interwoven history of both. "Science," one might have expected him to assert, "is the empirical-cum-rational analysis and directing of natural process." Of course, even the speculative cerebrations of mathematicians are natural processes, and in that sense the subject of scientific interest, not to physicists, but to psychologists ; or, when a mathematician asserts that by thought alone (whatever that is) the whole of physics can be deduced, of interest to medical psychologists. What we are here concerned with is the results of that cerebration, because they purport to state something about physics.

Two criteria are required in their examination. One will enable us to judge when results are such as to justify incorporation in the expanding body of scientific fact and generalization. That test is a practical one, and in the final analysis must be applied by the experimenter. Even a theory based on so-called *a priori* principles, where it purports to express itself in terms of observables, can have this test applied to it. Where it cannot be so expressed, it does not fall within the ambit of science : it is mathematically experimental speculation. Provided we recognize its nature and do not attempt to dress it up in the outward guise of science, who can assert that the experience so gained is of no value in the pursuit of legitimate science ? It is the emergence of this class of inquiry that distinguishes the present epoch in mathematical physics from that of Galileo.

To assert that such studies *may* have a secondary place in suggesting other more directly scientific inquiries does not provide the justification for their indiscriminate pursuit. Some other criterion must be found. ". . . hundreds of pages, and thousands of pounds are being spent . . . by a growing army of 'research' workers . . ." writes Dr. Dingle as

evidence of the folly of the new vogue in universes. Here the appeal is certainly not to any criterion directly or indirectly implied in his definition of science, but to a social judgment. He points the finger at something which he conceives, rightly or wrongly, to be anti-social, because it involves wastage on studies not directly scientific. This is a very significant matter. It is importing a consideration that has become more and more prominent in the columns of scientific journals during the past few years. It asserts that the justification for a scientific inquiry is not entirely a narrow scientific one but rests also on its social value. It is an admission of the interconnexion of science with society, that the direction of scientific development, and therefore that the body of theory and practice embraced within science at any given time, is dependent on fundamental social, besides scientific, considerations. That he makes this judgment is itself a fact: hence on his own definition of science he must find a rational place for it within that definition. As it stands there is none. If he objects to the work of the mathematical speculators, he can do so at present only with respect to these phrases that purport to deny the necessity for observation. He cannot object to the expenditure of funds on such investigations, or to publications that are simply mathematical exercises in fictitious model universes.

Just as soon as we face up to this, these various problems begin to fall into a more integrated scheme. We state the position thus:

(1) Science is a social effort to understand, control and fashion Nature in accordance with developing human needs and desires.

(2) The method of science is an ever-growing and ever-deepening integration between experiment and inference, emerging in successive generalizations and successive applications to practice.

(3) Mathematics is an instrument designed to assist in the drawing of inferences from sets of data or of ideas, or what amounts to the same thing, to test such sets for their logical consistencies and inconsistencies. It is in the exposure of the latter that the deficiencies in the original data are made plain. Out of these deficiencies emerges a drive towards further experimentation.

Now when the mathematics under (3) can be regarded as accessory to the inferential processes of (2), and so can be integrated with experimental development, it can be said to have a place in science. This would include that part of pure mathematics that in the widest possible sense can be looked upon directly or indirectly as 'instrument-making'. In no circumstances can (3) ever replace (2).

To decide that a particular series of investigations ought to be encouraged, and, in particular, that it ought to receive the blessing of dominant scientific groups—and this is ultimately the question raised by Dr. Dingle—equally implies deciding to discourage other investigations, since the income to science is limited. Such a decision can emerge only from a thoroughly clear conception of what is implied in (1). It involves planning science pure and applied in its social context, and not following it indiscriminately for intellectual, financial or æsthetic interest; we are very far from that at the present moment, as the leader columns of NATURE amply testify. Dr. Dingle is therefore tilting at one of the irrational features of social and scientific life. The fact that such irrationalism manifests itself in bold assertions of the type Dr. Dingle rightly deplores, that (3) can replace (2), is in such circumstances a natural consequence of the persistent refusal of scientific men to devote as much careful thought to science as a movement as they are prepared to give to their specialized fields.

Royal Observatory, Greenwich Annual Visitation

THE annual visitation of the Royal Observatory, Greenwich, by the Board of Visitors took place on June 5.

The report of the Astronomer Royal includes for the first time an account of the work of the Nautical Almanac Office, which has been made a branch of the Observatory, with Mr. D. H. Sadler as a chief assistant and superintendent of the Office. It is of interest to recall that the *Nautical Almanac* originated with Maskelyne, the first volume appearing in 1765, the year following his appointment as Astronomer Royal, and that the almanacs for fifty years were produced under his direction.

The Nautical Almanac Office has recently undertaken the compilation of an *Air Almanac*, specially adapted to the requirements of aerial navigation. The first issue will cover the last three months of the present year. The *Air Almanac* will give the air navigator the astronomical data in a convenient form; but the present methods for the subsequent computation are not suitable for conditions in the air. Extensive altitude and azimuth tables will be computed in the Office, which will give the solution of the spherical triangle directly.

The astronomical observations at Greenwich during the past year suffered inevitably from the poor observing conditions, the year having been one of the most cloudy on record. 6,573 transit observations were obtained, including 107 observations of the sun and 86 observations of the moon. The observations of the moon continue to show a decrease in the correction required by the longitude according to Brown's tables. These tables, however, include Newcomb's great empirical term and, when comparison is made with pure gravitational theory, the moon shows an increasing departure from its computed place. This departure is produced by variations in the rate of rotation of the earth. The accumulated effect of such variations in a year may at times amount to one second. The best astronomical clocks are scarcely sufficiently uniform in rate to detect such an error. It is possible that a clock of the vibrating quartz crystal type would have the desired accuracy. Such a clock is under construction for the Observatory at the National Physical Laboratory and should be of material help in improving still further the accuracy of the time signals sent out from the Observatory, which are necessarily based on predicted clock errors.

The Observatory controls, by means of hourly signals, the Post Office Talking Clock, a service that was inaugurated by the Astronomer Royal on July 24. The calls on this clock are at the rate of ten million a year. It is intended to install similar clocks in other large centres.

The observations made with the Cookson floating telescope on a uniform programme for the past twenty-five years are under discussion. These observations have shown that the mean position of the Pole has not varied, during this period, by more than two or three feet.

The 28-inch refractor is being used for the general re-measurement of all double stars in the northern hemisphere, within the range of the telescope, that show appreciable motion in a decade. 692 observations on 238 double stars were secured. A comparison image micrometer has been constructed to facilitate and improve the accuracy of the measurement of close pairs. The filar micrometer will continue to be used for the wider pairs.

The projection method of measuring photographs has been applied to the micrometer used for the measurement of the stellar parallax plates. The star image and the micrometer wire system are projected on a screen, which is adjusted to the measurer's most comfortable reading distance, and both eyes are used, so avoiding eye fatigue.

Sunspot activity has increased greatly during the year. There were about twenty-five groups the maximum area of which exceeded 500 millionths of the sun's hemisphere. An intense magnetic disturbance, the largest for ten years, was associated with a large spot, the meridian passage of which occurred on April 24. The study of bright eruptions on the sun has been continued. These are becoming much more frequent with increasing solar activity; 72 were observed during the year. A close relationship between these eruptions and sudden fadings of short-wave radio transmissions has been established. The spectrohelioscope is being used also for measures of radial velocities, mostly in connexion with dark markings associated with sunspots, and of the maximum intensities of prominences and of bright and dark flocculi.

Magnetic observations were made at Abinger throughout the year. The mean values of the magnetic elements for the year 1936 were as follows :

Declination . . .	W.	11° 20·0′
Inclination . . .		66° 41·8′
Horizontal intensity .		0·18524 gauss
Vertical ,, .		0·43007 ,,

Preliminary investigations in connexion with the new reversible transit circle have occupied much time. The figuring of the pivots was completed, the maximum departure of each pivot from the circular form being about one-twelfth of a micron. The errors of the one-degree divisions on the 24-inch fixed glass circle have been determined ; these show a range of only 0·4″. Observations with the transit micrometer revealed some anomalies that required extensive investigation, and that necessitated the return of the micrometer to the makers for correction.

The slit spectrograph for use with the 36-inch reflector has been completed. It has optical parts made of ultra-violet crown and flint glass, combined with quartz, being intended specially for work in the ultra-violet region. It can be used with a dispersion of either one or three prisms.

During the year, Mr. W. Bowyer, who had been in charge of the Time Department, retired after forty-four years' service and Mr. D. J. R. Edney, secretary, retired after nearly forty-five years' service.

The Astronomer Royal concludes his report with a reference to the difficulties connected with the installation of mercury vapour discharge lamps for lighting certain streets in the vicinity of the Observatory. Representations have been made to the Greenwich and Lewisham Borough Councils of the probable effects of the extension of such lighting along the roads across Blackheath. The two Borough Councils have expressed their desire to avoid interference with the work of the Observatory, and extensions of this system of lighting have been held in abeyance, so that possible alternatives could be thoroughly investigated.

Rothamsted and Agriculture

Annual Inspection of the Rothamsted Plots

A LARGE and representative gathering met at Rothamsted on June 4 for the annual inspection of the field plots and the laboratories. The Right Hon. W. S. Morrison, Minister of Agriculture, was present. Prof. H. E. Armstrong, recently elected chairman of the Lawes Agricultural Trust Committee, was unfortunately unable to attend owing to ill-health, but sent the following message :

"My connexion with Rothamsted goes back to 1870, when I first learnt to know Sir John Lawes and Dr. Gilbert. I am the only member of the original Trust Committee, established in 1898. I claim to have been instrumental in bringing both Sir Daniel Hall and Sir John Russell away from Wye here. I have therefore accepted the Chairmanship of the Trust Committee with singular satisfaction, though I know full well that it is now beyond me to exercise much constructive control.

"We are at the close of our first critical period. We have acquired the Trust and purchased our release from Sir John Lawes' conditions. We owe this advance to our present Director : he has carried out a task of greatest difficulty, with conspicuous success, for which I, as Chairman, would thank him on behalf of my colleagues and the friends of agriculture in general.

"At first the work of the Trust was confined to the experimental fields, laid down by Sir John Lawes and Dr. Gilbert—no funds were available for other

work. Sir John Russell, with extraordinary acumen, by his pertinaceous appeals, has secured funds enabling us to extend the ambit of our inquiries : by his genius we have acquired the estate and so extinguished our obligation. According to the Trust Deed, the Committee has the sole management, superintendence and direction of the Trust and of the investigations and experiments to be carried on. Authority rests with the Committee. This is not sufficiently recognized.

"We shall need now to frame a constitution to govern the future, by no means an easy task. Studying the work done during all but seventy years, I have long formed the very definite opinion that the 'living soul' does not enter sufficiently into our activities. We need to force our workers more out into the field.

"Sir John Lawes began his experiments in prehistoric times, in a controversy with Liebig over the need of wheat for nitrogen beyond that derived from natural sources. He was thereby led to initiate the systematic trials embodied in the plots, now known to all the world, using various mineral salts in conjunction with either ammonia or nitrates. Lawes did not consider that much more would be learnt from the crops, apart from the periodic effect of climatic variations. He thought, however, that in course of time the soil might be affected. It is to be feared that he inaugurated a policy which, if persevered in, may be ruinous to the soil.

"Lawes provided only for the continuation of the plot experiments. When the Development Commission divided agricultural inquiry into sections, it assigned *Soil* to Rothamsted. There were no funds for the work but the difficulty was overcome by the munificence of the Goldsmiths' Company. It is most unfortunate that our wings were thus clipped to the soil. The agricultural artist cannot work in a single medium—the policy of the Commissioners was a wrong one, wherever applied—equally at Aberdeen and Cambridge. As a consequence we have been far too much confined to academic laboratory work—animal and plant have scarcely been in the picture.

"Rothamsted policy in future must be to make food production and nutritional values—A.P.H.'s benenutrition—the main study. No higher work can be undertaken. A great and hopeful future is before farming, if only it be properly led. Special care must be given to training men for its service. To this end, the teaching of chemistry and physiology in general must be made effective in school and university. Training must be given in the use of eyes and especially in the exercise of scientific thought—of thought based upon knowledge.

"Agriculture has a sufficient task before it. The work done by Sir John Lawes is a great one—who shall say what his influence has been in introducing systematic inquiry into farming ? Thus far, however, he has taught us mainly what not to do—the course usually followed in the most fruitful inquiries but not to be long maintained."

In the absence of Prof. Armstrong, Sir Bernard Greenwell presided. Mr. Morrison, speaking of Rothamsted Experimental Station, said that it is a remarkable example of State aid and private enterprise, and that the suspicion with which research used to be regarded by the practical man has largely passed away. Mr. Morrison then reviewed some of the broad considerations which have guided him in developing his recent agricultural policy. Soil fertility is the most fundamental aspect of the agricultural problem. With a fertile soil, production can be adapted to changing circumstances and emergencies, whereas a depleted soil is unequal to the demands made upon it. For this reason, he has taken steps to encourage measures of soil improvement, and in the technical matters arising out of his proposals for increasing the use of lime and basic slag, he has not hesitated to call upon the special knowledge that Rothamsted can provide. He reminded his audience that in addition to the material basis of farming, the soil and the livestock upon it, there must also be knowledge.

Sir John Russell, who followed, took up this theme and showed how the object of the work at Rothamsted has always been this search for information. Some of the investigations undertaken seemed at first to be remote from practice, and indeed appeared to have no immediate application. Sir John proceeded to quote many instances in which such investigations have turned out to be of the greatest practical importance and usefulness. He mentioned some of the recent Rothamsted work on lime and basic slag that will be of special service in advisory work arising out of the new proposals. In spite of the difficult six months just passed, Broadbalk, now in its ninety-fifth year, looks surprisingly well, and its interest has been enhanced, rather than diminished, by the fallowing system adopted in recent years. The visitors also inspected the modern rotation experiments on present-day problems in soil fertility.

The proceedings were favoured by ideal weather conditions.

Empire Agricultural Officers

On June 3, the annual gathering of agricultural officers on leave from various parts of the Empire took place at Rothamsted at the invitation of the Imperial Bureau of Soil Science, when some eighty visitors representing seventeen Empire countries were present. This function not only enables members of the overseas agricultural services to make contact with the staff of the Soil Bureau and Rothamsted workers, but also affords a valued opportunity for meeting each other. In his speech from the chair, Lord Bledisloe outlined the development of the Soil Bureau during the eight years of its existence. He mentioned that the year's output of scientific papers in soil science now amounts to the astonishing figure of about 4,000, written in twenty different languages. The task successfully undertaken by the Bureau is to sort out this great mass of literature and make available to workers in any part of the Empire the latest information from all countries relating to soils, crops and manures. The Technical Communications published by the Bureau are in increasing demand, and several of them have attained the status of standard and authoritive works. Lord Bledisloe, speaking with special knowledge of agricultural conditions in several Dominions and his own close acquaintance with agricultural research, expressed his conviction that money spent in the investigation of farming problems is an investment that will give returns out of all proportion to the original outlay.

The visitors spent the morning examining the plots on the Rothamsted farm and the afternoon was devoted to a tour round the laboratories.

Science News a Century Ago

Wheatstone and Cooke's Electric Telegraph

ON June 13, 1837, Wheatstone and Cooke took out their first patent for "giving signals and sounding alarms at distant places by means of electric currents". Among the instruments included in this patent were those with five needles and two needles. An experimental line was laid down between Euston and Camden Town in 1837, but the first actual working telegraph was that joining Paddington and West Drayton, a distance of 13 miles, erected in 1838–39. Examples of these early telegraphs are preserved in the Science Museum.

J. D. Forbes and von Buch

IN the journal of his travels on the Continent in the summer of 1837, J. D. Forbes in his notes on the six weeks he spent at Bonn wrote under the date June 15 : "Finding Von Buch here, I left my letter from Professor Jameson for him, and in the course of an hour he called. I was instantly charmed with the kindness and simplicity of his manner ; and as I could see at once that his character for singularity was not one which I had any reason to fear, we were immediately excellent friends".

The next day the two met again, and had conversation on Forbes's views on the Pyrenean springs and von Buch's paper on craters of elevation. "As to the history of the elevation theory," wrote Forbes, "he states that he first pointed out four elevations belonging to different epochs—all of which are well seen in Grimm's map of Germany—chiefly by tracing the river courses. The mountains about the Elbe form one system, the Taunus mountains another, the Schwartz-Wald a third, and the Alps a fourth ; . . . pointing out, as evidence of the comparatively recent elevation of the latter, the inclination of the tertiary strata. He gives Elie de Beaumont the credit of finding exact criteria for determining epochs of elevation, but attached little probability to the theory of parallelism on a large scale, considering the systems of chains rather overdone. . . ."

On the afternoon of June 16, Forbes observed the magnetic dip at Poppelsdorf and afterwards called on Prof. Gustav Bischoff "who showed me the sheets of his work on the temperature of the earth. . . . He has also made experiments on the cooling of spheres of melted basalt, having a diameter of twenty-seven inches, and others of nine inches. . . . Von Buch came in as we were examining these".

"Bischoff also took a piece of basalt, split in two, and tied together with twine . . . fixed it in the axis of a cylindrical mould, and poured molten iron round it. This latter in cooling, (according to him) expanded and squeezed the basalt. Certainly, many dykes of basalt in iron appeared, very prettily, but I doubt the squeezing, especially from the large cavities left in the iron, a fact which was also noticed by Von Buch : whether there would be squeezing or not, would be somewhat difficult to predict. . . ."

Captain George Back's Expedition

ON June 15, 1837, a general meeting was held of the subscribers to the fund raised for sending Captain Back in search of Captain John Ross. The meeting was held in the rooms of the Royal Geographical Society in Regent Street, Admiral Sir George Cockburn being in the chair. The total amount subscribed had been £6,578 19*s*. 10*d*. and after paying all expenses there remained a balance of £619 4*s*. 6*d*. The meeting agreed with the suggestions put forward by the committee that three pieces of presentation plate should be given to Captain Back, Captain James Clark Ross, and Captain Maconochie, the last of whom had organized the expedition, and that £300 should be given to the Sailors' Home, Wells Street. The Hudson Bay Company had waived the whole of its claim for the amount due for supplies provided for the expedition in North America.

Dalton on the Constitution of the Atmosphere

BEFORE adjourning for the long vacation, the Royal Society met on June 15 to hear the reading of several papers, among which was one by Dalton entitled "Sequel to an Essay on the Constitution of the Atmosphere published in the Philosophical Transactions for 1836 ; with some account of the Sulphurets of Lime". An abstract of the paper said : "The author communicates in this paper an account of the investigations on the constitution of the atmosphere, which have engaged his attention during a long period of years. He enters into an examination of the comparative advantages of the three methods which are most in use for analysing common air, namely, firing it with hydrogen in Volta's endiometer, or abstracting the oxygen by means of nitrous gas and quadrisulphuret of lime ; and details the precautions to be taken in the employment of each of these methods, and the degree of accuracy to be expected from the results under different circumstances. He then relates numerous experiments made on air obtained from great heights, from which he is led to the conclusion that the proportion of oxygen to azote in the atmosphere on the surface of the earth is not precisely the same at all places and times. . . ."

National Board of Health

AN editorial in the *British Annals of Medicine* of June 16, 1837, contains the following suggestion : "The necessity for the establishment of a Board of Public Health is forcibly pointed out by the fact that the mortality during the last 10 years—years of peace too—has been progressively increasing in Sweden, Prussia, France and England. This is proved by authentic documents. Are medical men aware of this fact ? Not generally, we believe, because no public board exists to bring the conviction home to their minds. Yet—amidst all the splendid discoveries of the century, the accession to our knowledge of disease and the new remedies which have sprung into existence—people die faster than they did 10 years ago. A vast opposing tendency yet to be investigated, carries us against the full tide of improvement. . . . So soon as public attention can be brought to bear on the question, a Board of Health will, we do not doubt, be established. As Dr. Möhry has remarked, the apathy of the College of Physicians, and of the persons accidentally at the head of the profession, has alone retarded the establishment in this country of something corresponding to the *Conseils de Salubrité*, and *Sanitätsbehörde* in France and Germany. . . . There would be three principal functions of the new institution ; to publish a report of the state of public health every year ; to provide or point out measures for its amelioration ; and to control the medical profession. . . . The Annual Sanitary Report, if properly executed, would be an invaluable contribution to science."

Societies and Academies

Dublin

Royal Irish Academy, March 16.

J. J. DOWLING and T. G. BULLEN : Precision measurements with a radial deflection oscillograph. An extra pair of concentric cylindrical electrodes are fitted between the XY deflector plates and the screen of a gas focused cathode ray oscillograph. Employing a double frequency circular time-base locked to the signal frequency a so-called Radioid curve (resembling a Limaçon) is obtained, and the position of the node around the time base circle serves to measure phase or frequency differences in high-frequency circuits. Another application is as a 'time-comparator', and comparisons of the periods of a pair of two-seconds pendulums to within five parts in a million are easily obtained while the pendulums are executing twenty swings. An improved design of tube is described and further work promised.

Paris

Academy of Sciences, April 26 (*C.R.*, 204, 1225–1284).

SAMUEL EILENBERG : Slight enlacement.

SHIING-SHEN CHERN : The geometry of a differential equation of the third order.

NIKOLA OBRECHKOFF : The zeros of some classes of polynomials.

PIERRE CHEVENARD, LOUIS HUGUENIN, XAVIER WACHÉ and ALBERT VILLACHON : New alloys of the elinvar type susceptible of structural hardening. Balance springs of elinvar, although giving excellent thermal compensation, are much softer than ordinary steel springs. The elinvar alloy can be modified by the addition of titanium. The modified alloy, while retaining the thermal advantages of elinvar, has an elastic limit approaching that of carbon steel.

MLLE. JACQUELINE HADAMARD : The dielectric constants of cyclohexane and benzene. The results given, for which an accuracy of 0·1 per cent is claimed, are compared with those of earlier workers.

GEORGES NADJAKOFF : The nature and origin of the large ions of the atmosphere. The author's observations are in agreement with the view suggested previously by other workers, that there is a close connexion between the number of large ions and the smoke of towns.

J. J. PLACINTEANU : The wave function of the photon.

MARCEL LAPORTE : The production of white light by the electrical luminescence of xenon.

JULES FARINEAU : An attempt at photometry in the field of the soft X-rays ; application to the study of the free electrons of metals.

GEORGES MANO : The relation between the kinetic energy and path of the protons. The case of artificial transformations.

ROGER CHATELET : A thermo-chemical application of thermal analysis to the formation of the eutectoids of the binary alloys.

CLÉMENT COURTY : The diamagnetism of iodine solutions. An attempt to trace a connexion between the colour of iodine solutions in different solvents and their magnetic properties.

ERNEST TOPORESCU : The action of sulphur on silver.

ALBERT PORTEVIN, GEORGES CHAUDRON and LÉON MOREAU : Contribution to the study of the role of gases in metals. Study of the variations in hardness, conductivity and crystalline parameter after removing the gases at the ordinary temperature, after annealing and after charging with hydrogen by electrolysis. Data are given for iron and aluminium.

HUBERT FORESTIER and RICHARD LILLE : The variation of catalytic activity of ferromagnetic oxides at the Curie point. The experiments cited establish a clear relation between the magnetic transformation point and the catalytic properties of ferromagnetic oxides.

GEORGES DENIGÈS : Reactions for the identification of tellurium of microchemical order.

PAUL RENAUD and GEORGES COSTEANU : Anomalies in the action of steam or of ammonia on sodium or hæmatoxylin.

MAXENCE MEYER : The formation of hydrocarbons in the thermal decomposition of the α-ethoxyacids.

PANOS GRAMMATATICAKIS : The action of mixed organomagnesium compounds on the phenylhydrazones of the symmetrical alkylphenylhydrazines. A new method of preparation of the symmetrical alkylphenylhydrazines.

PAUL FLEURY and MLLE. SUZANNE BOISSON : The action of periodic acid on lactic and pyruvic acids. In the oxidation of lactic acid by periodic acid, two primary products are aldehyde and methyl alcohol.

JACQUES DUCLAUX : The transparency of the air in the arctic regions.

PAUL BECQUEREL : The death of the plant cell by freezing in liquid nitrogen at −190° C.

MARCEL MASCRÉ and RENÉ PARIS : Scoparine (scaparoside) from *Sarothamnus scoparius*.

AUGUSTE CHEVALIER and PIERRE VIGUIER : The double origin of cultivated rice and the West African centre of rice dispersion.

JEAN RENAUD : A *Saccharomyces* presenting curious anomalies in its sexuality due to a dikaryotic state of its zygospores.

STEFAN NICOLAU and MME. LÉONIE KOPCIOWSKA : Data on the coloration and morphology of some viruses in animal tissue.

ALEXANDRE BESREDKA and LUDWIK GROSS : The immunization of the fowl against sarcoma by the intracutaneous method.

PAUL VALADE : The cancer-forming activity of methylcholanthrene.

Amsterdam

Royal Academy (*Proc.*, 40, No. 4 ; 1937).

R. WEITZENBOCK : Remarks on trivectors.

F. M. JAEGER and L. BIJKERK : Investigations on the complex salts of the racemic and optically active cyclohexanediamines with trivalent cobalt and rhodium (4). Crystallographic properties of the optically active tri-cyclohexanediamine cobalt salts and on ethylenediamine-cyclohexanediamine cobaltic salts of this series.

J. LE HEUX and A. DE KLEYN : Investigations on the isolated eye muscles of the rabbit during the positional nystagmus in acute alcohol poisoning.

T. L. DE BRUIN, J. N. LIER and H. J. VAN DE VLIET : The Zeeman effect of doubly ionized cerium, Ce III.

T. L. DE BRUIN : New terms in the spark spectra of argon, A II and A III.

J. POPKEN : An arithmetical property of certain integral functions (3).

C. VISSER : Certain infinite sequences.

N. H. SWELLENGREBEL, A. DE BUCK and H. KRAAN : Further investigations on 'healthy' human carriers of *Plasmodium vivax* in North Holland.

Capetown

Royal Society of South Africa, April 21.

Archæology of the Oakhurst Shelter, George. (1) A. J. H. GOODWIN : Course of the excavation. (2) A. J. H. GOODWIN : Disposition of skeletal material. (3) M. R. DRENNAN : Skeletal material. (4) J. F. SCHOFIELD : The pottery.

P. W. LAIDLER : An unusual grooved stone.

Geneva

Society of Physics and Natural History of Geneva,

March 4.

R. WAVRE and G. BILGER : Some polygons of similar attraction. The authors establish that a homogeneous regular polygon creates at every point external to the plane the same single layer logarithmic potential as the radii of the circumscribed circle touching the angular points.

March 18.

E. C. G. STUECKELBERG : (1) The analogy between the retarded potential of quantum physics and classical physics. A Lagrangian function is established by a system of n particles which react with each other. This function is invariant, but has the defect of containing higher potentials. However, the interaction terms can be expressed in quantum physics, where they give the necessary relations. (2) The establishment of the formula of retarded potential in quantum physics. By means of electrodynamics with multiple time quanta, the interaction terms established in the previous communication are here established rigorously to the second approximation in e.

E. CHERBULIEZ and J. JEANNERAT : Researches on casein. The authors have improved the method of separation of casein into various fractions. They show that one of these fractions, δ, is identical with a protein body, proteose, isolated by Hammarsten from the products of the action of rennet on casein. Hence proteose appears to be an original constituent of the milk and not a cleavage product due to the action of rennet on casein.

F. CHODAT : The enzyme decoloration of the petals of the poppy.

E. BRINER and E. PERROTTET : (1) The problem of the removal of ozone from air and from oxygen. The properties of air and oxygen free from ozone. The authors describe the method employed for removing ozone from ordinary air and ordinary oxygen. They point out the differences in oxidizing power of air and of oxygen free from ozone. (2) A method of analysing very dilute ozone : the determination of the ozone concentration in the air of Geneva. The application of the method described at the meeting of February 4, 1937, by the same authors has given for the concentration of ozone in the air of Geneva the value 7×10^{-8} with an error of $\pm 2 \times 10^{-9}$.

April 29.

F. CHODAT and R. CORTESI : Study of buffer action in culture fluids. The authors have observed fluctuations in buffer action in the culture of *B. mycoides ;* a minimum is reached at the end of the first week, as soon as the gelatine medium is liquefied.

P. ROSSIER : A simplification in the calculation of the magnitude of a star, relating to a receiver with two sensibility maxima. The author replaces the usual formula by the simpler one of Stirling, some factors of which can be neglected in certain cases. This leads to a marked simplification in the calculations.

R. SULZER : The comparative study of the attitude of an animal deprived of a labyrinth and the attitude of a normal animal standing on a slope. The author studies the disturbances of equilibrium produced in the guinea pig by the unilateral suppression of the labyrinth.

G. TIERCY and CH. GOLAZ : The law of the variation with altitude of wind velocity. The note gives three numerical tables established with the view of controlling the theory by observation.

Melbourne

Royal Society of Victoria, April 8.

ELIZABETH A. RIPPER : (1) Some Stromatoporoids from Griffith's Quarry, Loyola, Victoria. This locality has yielded four species : *Clathrodictyon regulare* (von Rosen). *C.* aff. *chapmani* Ripper, *Stromatopora typica* von Rosen, and *S. bücheliensis* (Bargatsky), as well as a *Stromatoporella* not here described. This fauna has little in common with that of the Yeringian limestone at Liludale, and the abundance of *C. regulare* and *S. typica*, two Wenlock species, suggests that the limestone should be placed on a lower horizon than that of Lilydale, which contains a fauna with Devonian affinities. (2) The Stromatoporoids of the Buchan District, Victoria. Fourteen species and varieties are described, of which the following are new : *Actinostroma stellulatum* var. *distans*, var. nov., *A. contortum*, sp. nov., *Clathrodictyon convictum* var. *delicatula*, var. nov., *Hermatostroma episcopale* var. *buchanensis*, var. nov. The assemblages have, in the main, definite Devonian affinities, though typical Wenlock species, for example *Clathrodictyon regulare* (von Rosen), persist and make up a small proportion of the fauna.

G. BAKER : Orthite in some Victorian granitic rocks. The study of the distribution of orthite in some granites and associated xenolithic rocks in Victoria indicates that orthite has a contamination mode of origin in most Victorian examples, and a pyrogenetic mode of origin in a few cases.

F. CHAPMAN : Cherty Limestone with *Planorbis hardmani* Wade from the Mt. Elder Range, Kimberley District, W.A. The cherts were originally laid down in early Pleistocene times in shallow lakes on a peneplain, and are now perched on hills nearly 1,000 feet above sea-level. The Mollusca of the lake fauna comprise *Planorbis hardmani*, *P.* aff. *essingtonensis*, and a probable new species of *Bullinus*. Other indigenous organisms of this lake deposit are : calcareous algæ allied to *Cladophora* and *Charophytes*, freshwater sponge-spicules, Ostracoda and probable insect remains. There is also evidence that minute Foraminifera had been carried into this lake deposit by wind agency from the coast-line, the nearest point to which now lies about 160 miles to the north.

Forthcoming Events

[*Meetings marked with an asterisk are open to the public.*]

Tuesday, June 15

EUGENICS SOCIETY, at 5.15—(at the Linnean Society, Burlington House, W.1).—Prof. W. G. Miller: "Veterinary Eugenics".*

Wednesday, June 16

WARBURG INSTITUTE, at 5.30.—Dr. Otto Brendel: "Roman Emperor Worship" (succeeding lectures on June 23 and 30).*

Appointments Vacant

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

ASSISTANT WOMAN INDEXER at the Bureau of Chemical Abstracts, 46 Finsbury Square, E.C.2—The Secretary (June 15).

TEACHER OF SCIENCE AND TEACHER OF ENGINEERING in the Luton Technical College—The Director of Education, Shire Hall, Bedford (June 15).

TWO DISTRICT FOREST OFFICERS—The Secretary, Forestry Commissioner, 9 Savile Row, W.1 (June 16).

ASSISTANT LECTURER IN MATHEMATICS in the University of Manchester—The Registrar (June 17).

FOUR UNIVERSITY DEMONSTRATORS IN ANATOMY AND ONE UNIVERSITY DEMONSTRATOR IN PHYSIOLOGY in the University of Cambridge—Dr. F. J. W. Roughton, Physiology Laboratory, Cambridge (June 18).

CURATOR at the Royal Botanic Gardens, Kew—The Secretary, Ministry of Agriculture and Fisheries, 10 Whitehall Place, S.W.1 (June 21).

LECTURER IN MECHANICAL ENGINEERING in the Municipal Technical College, Hull—The Director of Education, Education Offices, Hull (June 21).

LECTURER IN PHYSIOLOGY AND ZOOLOGY in the Sunderland Technical College—The Director of Education, Education Offices, 15 John Street, Sunderland (June 22).

ASSISTANT IN AGRICULTURAL BOTANY in the Edinburgh and East of Scotland College of Agriculture—The Secretary (June 23).

BIOCHEMIST in the Royal Victoria Infirmary, Newcastle-upon-Tyne—Dr. Ungley (June 27).

DIRECTOR OF RESEARCH to the British Rubber Research Board—R.G.A., 19 Fenchurch Street, E.C.3 (June 30).

Official Publications Received

Great Britain and Ireland

Armstrong College: Dove Marine Laboratory, Cullercoats, Northumberland. Report for the Year ending July 31st, 1936. (Third Series, No. 4.) Pp. 52+10 plates. (Cullercoats: Dove Marine Laboratory.) 5*s*. [105

Transactions of the Royal Society of Edinburgh. Vol. 59, Part 1, No. 1: A Study of the Vascular Supply to the Carpels in the Follicle-bearing Ranunculaceæ. By Dr. Mabel S. Fraser. Pp. 56. 7*s*. Vol. 59, Part 1, No. 3: Perthshire Tectonics; Schiehallion to Glen Lyon. By Prof. E. B. Bailey and Dr. W. J. McCallien. Pp. 79–118+2 plates. 6*s*. (Edinburgh: Robert Grant and Son, Ltd.; London: Williams and Norgate, Ltd.) [135

Amgueddfa Genedlaethol Cymru: National Museum of Wales. The Fisheries of Wales: Handbook to a Temporary Exhibition, May to October 1937. By Colin Matheson. Pp. 16. (Cardiff: National Museum of Wales.) 2*d*. [135

Association of Special Libraries and Information Bureaux. Select List of Standard British Scientific and Technical Books. Compiled at the request of the British Council. Pp. 35. (London: Association of Special Libraries and Information Bureaux.) 2*s*. to "Book-List" subscribers; 2*s*. 6*d*. to others. [145

The Journal of the Royal Agricultural Society of England including the Farmers' Guide to Agricultural Research. Vol. 97. Pp. 498+cxlviii+12+22 plates. (London: John Murray.) 15*s*. (145

Department of Scientific and Industrial Research. The Investigation of Atmospheric Pollution: Report on Observations in the Year ended 31st March 1936. (Twenty-second Report.) Pp. viii+128+2 plates. (London: H.M. Stationery Office.) 6*s*. net. [185

The British South Africa Company. Publication No. 5: Mazoe Citrus Experimental Station Report for the Year ending 31 December 1935. Pp. xii+74. (London: British South Africa Co.; Mazoe: Citrus Experimental Station.) [185

Department of Scientific and Industrial Research. Summary of Progress of the Geological Survey of Great Britain and the Museum of Practical Geology for the Year 1935. Part 2. Pp. vi+129+7 plates. (London: H.M. Stationery Office.) 3*s*. net. [185

University College, Cork. Agricultural Bulletin No. 4: Oils from Irish Grown Plants. Preliminary Report by Prof. Joseph Reilly and Denis F. Kelly. Pp. v+92+6 plates. (Dublin and Cork: Educational Company of Ireland; London: Longmans, Green and Co., Ltd.) 2*s*. 6*d*. [195

Proceedings of the Royal Society of Edinburgh, Session 1936–1937. Vol. 57, Part 2, No. 8: On the Ciliary Currents on the Gills of some *Tellinacea* (Lamellibranchiata). By Alastair Graham. Pp. 128–134. 6*d*. Vol. 57, Part 2, No. 9: The Gravitational Field of a Distribution of Particles Rotating about an Axis of Symmetry. By W. J. van Stockum. Pp. 135–154. 1*s*. 9*d*. Vol. 57, Part 2, No. 10: The Revised Complete System of a Quadratic Complex. By Prof. H. W. Turnbull. Pp. 155–162. 9*d*. Vol. 57, Part 2, No. 11: The Time Lag of the Vacuum Photo-cell. By Dr. R. A. Houstoun. Pp. 163–171. 9*d*. Vol. 57, Part 2, No. 12: Studies in Practical Mathematics. 1: The Evaluation, with Applications, of a Certain Triple Product Matrix. By Dr. A. C. Aitken. Pp. 172–181. 1*s*. Vol. 57, Part 2, No. 13: Ions and Isotopes. By Prof. James Kendall. Pp. 182–193. 1*s*. (Edinburgh: Robert Grant and Son, Ltd.; London: Williams and Norgate, Ltd.) [205

Seventy-third Annual Report on Alkali etc. Works. Proceedings during the Year 1936. By the Chief Inspectors. Pp. ii+51. (London: H.M. Stationery Office.) 1*s*. net. [215

Other Countries

Proceedings of the Academy of Natural Sciences of Philadelphia, Vol. 89. African and Malagasy Blattidæ (Orthoptera), Part 3. By James A. G. Rehn. Pp. 17–123+plates 8–11. (Philadelphia: Academy of Natural Sciences.) [185

Memoirs of the Geological Survey of India. Palæontologia Indica, New Series, Vol. 21, Memoir No. 4: Fossil Algæ from the Uppermost Cretaceous Beds (The Niniyur Group) of the Trichinopoly District, S. India. By L. Arama Rao and Dr. Julius Pia. Pp. v+49+6 plates. (Calcutta: Geological Survey of India.) 4.10 rupees; 7*s*. 9*d*. [185

Organisation météorologique internationale. Commission regionale 2: Preliminary Report of the Proceedings of the First Conference in Hong Kong, January 13th–21st, 1937. Pp. 105. (Hong Kong: Printed at Victoria Gaol.) [185

Report of the Aeronautical Research Institute, Tôkyô Imperial University. No. 147: Vibration Phenomena in Ternary Wing Flutter. By Katsutada Sezawa, Satosi Kubo and Hirosi Miyazaki. Pp. 129–162. (Tôkyô: Kôgyô Tosho Kabushiki Kaisha.) 45 sen. [185

Education, India. Education in India in 1934–35. Pp. vii+137. (Delhi: Manager of Publications.) 2 rupees; 3*s*. 6*d*. [185

Proceedings of the Animal Husbandry Research Workers' Conference held at New Delhi from the 17th to the 19th February 1936, with copies of Papers presented by the Delegates and a resumé of Discussions thereon. Pp. iii+112. (New Delhi: Government of India Press.) [185

Indian Central Cotton Committee: Technological Laboratory, Technological Bulletin, Series A, No. 36: Spinning Tests on Mixtures of Stable Fibres and Indian Cottons. By Dr. Nazir Ahmad. Pp. ii+17. (Bombay: Indian Central Cotton Committee.) 8 annas. [185

Publications of the Dominion Observatory, Ottawa. Bibliography of Seismology. Vol. 12, No. 12 (Items 3267–3370): October, November, December, 1936. By Ernest A. Hodgson. Pp. 241–266. (Ottawa: King's Printer.) 25 cents. [185

Imperial College of Tropical Agriculture: Low Temperature Research Station. Memoir No. 5: Antiseptic and other Treatments in the Storage of Trinidad Citrus Fruits. By C. W. Wardlaw and E. R. Leonard. With an Appendix: Mycological Notes on Citrus Wastage, by R. E. D. Baker. Pp. 27+11 plates. 2*s*. 6*d*. net. Memoir No. 6: Storage Investigations with Trinidad Citrus Fruits, 1935–36. By E. R. Leonard and C. W. Wardlaw. Pp. 28+8 plates. 2*s*. 6*d*. net. (Trinidad: Imperial College of Tropical Agriculture.) [185

Meddelande från Lunds Astronomiska Observatorium. Ser. 2, Nr 78 (Historical Notes and Papers, Nr. 4): On Greek Cosmogony and Astronomy. By Knut Lundmark. Pp. 36. Ser. 2, Nr. 82 (Historical Notes and Papers, No. 6): Statistical Notes on the Astronomers of Antiquity. By Per Collinder. Pp. 13. Ser. 2, Nr. 83 (Historical Notes and Papers, Nr. 7): Quelques essais d'identification de constellations sur les rochers sculptés en Ostrogothie. Par Björn Svenonius. Pp. 16. Ser. 2, Nr. 85 (Historical Notes and Papers, Nr. 9): Quelques aspects de l'identification de constellations sur les rochers sculptés par rapport à l'histoire des religions. Par Åke Ohlmarks. Pp. 6. (Lund: Astronomiska Observatorium.) [185

Havsforskningsinstitutets Skrift. No. 102: Översikt av isarna vintern 1934–35. Av Risto Jurva. Pp. 64. No. 106: Strom- und Windbeobachtungen an den Feuerschriffen in den Jahren 1934 und 1935. Von E. Palmén. Pp. 57. No. 107: Havsforskningsinstitutets Verksamhet år 1935. Av Gunnar Granquist. Pp. 18. No. 108: Översikt av isarna vintern 1935–1936. Av Gunnar Granquist. Pp. 47. No. 109: Regular Observations of Temperature and Salinity in the Seas around Finland, July 1935–June 1936. By Gunnar Granquist. Pp. 43. No. 110: The Thalassological Cruise, Sept.-Oct. 1936. By E. Palmén and E. Laurila. Pp. 14+v. No. 111: Havsforskningsinstitutets Verksamhet under år 1936. Av Gunnar Granquist. Pp. 16. (Helsinki: Havsforskningsinstitutet.) [195

Bericht über das Geobotanische Forschungsinstitut Rübel in Zürich für das Jahr 1936. Von E. Rübel. Pp. 127. (Zürich: Geobotanische Forschungsinstitut Rübel.) [215

Geofysiske Publikasjoner utgitt av det Norske Videnskaps-Akademi i Oslo. Vol. 11, No. 14: Observations of Tidal Motions of the Earth's Crust made at the Geophysical Institute, Bergen. By J. Egedal and J. E. Fjeldstad. Pp. 30. (Oslo: Grøndahl and Søns Boktrykkeri.) 3.00 kr. [215

Transactions of the Academy of Science of Saint Louis. Vol. 29, No. 2: Atrypæ of the Central Missouri Devonian. By D. K. Greger. Pp. 39–54. 25 cents. Vol. 29, No. 3: Outline of a Theory of Functions of an Abstract Variable. By Solomon Bilinsky. Pp. 55–86. (St. Louis, Mo.: Washington University.) [215