NATURE

SATURDAY, JANUARY 10, 1925.

CONTENTS.

Editorial and Publishing Offices:
MACMILLAN & CO., LTD.,
ST. MARTIN'S STREET, LONDON, W.C.2.

Editorial communications should be addressed to the Editor.
Advertisements and business letters to the Publishers.

Telephone Number: GERRARD 8830.
Telegraphic Address: PHUSIS, WESTRAND, LONDON.

Science and Administration in East Africa.

THE members of the East African Parliamentary Commission, who returned to England on December 23, have not only a remarkable itinerary to record even in these days of rapid travel, but also, according to reports received from the five territories which formed the subject of their inquiries, a notable performance of work under peculiarly trying conditions. Moreover, their visit has been of special significance to scientific workers. Major Church, whose warm advocacy in Parliament of the claims of science to the greater support of the country and his exposition of the function of science in economic development led to his being charged with the responsibility of reporting on the scientific and medical services, and Mr. Ormsby-Gore, Chairman of the Commission and again Under-Secretary of State for the Colonies, have both given abundant proof in their public utterances of their appreciation of the scientific aspect of the problems of East Africa; their realisation of the imperative need for the augmentation of staffs of existing scientific departments; for the re-establishment of scientific institutions which have been either abolished or neglected; and for the provision of funds for a campaign against the greatest scourge in equatorial Africa, the tsetse fly.

To scientific workers in East Africa particularly, the visit of the Commissioners must have been a source of lively satisfaction, for they have been given little sympathy or encouragement in recent years. The appointment of the Economy Committee in Great Britain in 1922 for the purpose of reducing expenditure in public departments set an example to the colonial governments. But while the recommendations of Sir Eric Geddes for the crippling of our home research and technical services were tempered by the growing consciousness among all classes in England of the important part played by science in industrial development, those of his prototypes in East Africa met with no such opposition. Science departments were regarded generally as luxuries, and were either "axed" completely or made aware that their continuance for more than another year or two was contingent upon some tangible and material proof of their economic value. For example, the discovery of a rich gold reef by a geologist singly responsible for the geological work of a territory the size of Great Britain might be accepted as a justification for his continued existence. A suggestion by the victim that the discovery of minerals of economic importance was not his primary function, or that such discoveries could best be attained by a scientific and systematic examination of the geology of the country, would probably have been regarded, by the legal luminaries and administrative officers to whom was generally entrusted the task of reducing expenditure, as a confession of incompetence.

Although in the past year the ravages of disease among the human and animal population; the desiccation of certain hitherto fertile tracts of country due to unrestricted forest fires; the impoverishment of the soil and the increase of insect pests due to indiscriminate grass burning; the urgency of the need for local supplies of fuel for transportation, industrial and domestic needs other than timber; the clamour of settlers and natives and merchants for the protection of economic crops against insect pests and diseases due to their partial realisation of the grave consequences attendant upon the neglect of precautionary measures, have had their effect upon the governments concerned, there remains yet considerable confusion of thought regarding the application of science to these matters. The mentality still exists which would starve research in tropical diseases because there had been no recent calamitous outbreak, would contemplate with equanimity the expense entailed by the forced removal of a population of tens of thousands from an infected area when disease overtakes it, but once the immediate catastrophe was past, relapse into the old indifference to research.

It is fortunate indeed that men like Mr. Ormsby-Gore and Major Church were members of the Commission. They made a special point of meeting every scientific officer available. In Northern Rhodesia the Commissioners met Dr. May, who for many years has advocated heroic measures against the tsetse fly. Dr. Dixey, the Nyasaland geologist whose recent discovery of dinosaur remains has aroused considerable interest in Great Britain, and whose discovery of coal in the Chiromo district with indications of a coalfield of vast dimensions is of the utmost importance to East Africa, was able to show Major Church round the scenes of his labours and to indicate what was urgently required in the nature of a geological survey. In Tanganyika Territory, Dr. Shircore and Dr. Butler discussed with him their divergent views regarding the relation of yaws and syphilis. Dr. Scott was able to show the tremendous improvement that he has effected in the sanitation of Dar-es-Salaam, which promises to eradicate malaria in this district. The Commissioners travelled far through the fly belts with Mr. Swynnerton, whose heroic measures against the tsetse fly will establish his reputation as one of the foremost personalities in East Africa. They visited also the Veterinary Research Laboratory at Mpapwa, where Mr. Hornby is conducting a series of remarkable experiments to test the relative values of hyper-immunisation and immunisation. In Uganda they met the important staff, consisting of Dr. H. L. Duke, Dr. G. D. H. Carpenter, and Mr. Fiske, whose investigations into sleeping sickness must form the basis of any campaign for the removal of this human scourge. There also Mr. Wayland and Mr. Simmons, the geologists, gave them a greater insight into their purely scientific work and their discoveries of oil in the vicinity of Lake Albert Nyanza than the mere perusal of their official report would indicate; and in Kenya Colony they were able to judge dispassionately of the effect of decentralisation of the research laboratories which followed the departure of Mr. V. H. Kirkman to Zanzibar. The visit to the Amani Institute in Tanganyika a few days before departing from Mombasa convinced Mr. Ormsby-Gore and Major Church also of the urgent necessity of carrying on the work started by Zimmerman under the German Government.

The Chairman of the Commission, in his public utterances, brought home to our East African communities the grave character of tsetse fly domination. Many of his hearers learned for the first time that two-thirds of Tanganyika Territory is practically unfit for human or animal occupation through the activities of this insect and that in every territory visited the fly is increasing. He suggested that the malign influence of the tsetse fly upon tropical Africa is such as to merit the endowment by all civilised countries of a group of research workers to investigate the special problems connected with it.

Major Church did well to remind his Mombasa audience of the disadvantages of parochialism, giving the decay of the Amani Institute as an illustration of his theme. Although that great botanical research institute is ideally situated for carrying out work of vital importance to the whole of tropical Africa, intercolonial jealousies have led each colony to evade making a contribution to its upkeep, with the inevitable results. He tentatively suggested also that the new bacteriological laboratories at Entebbe in Uganda might be supported by at least four of the East African territories owing to its advantages of situation. In the same way, the Veterinary Research Laboratory at Nairobi in Kenya Colony, founded by Mr. Eustace Montgomery, Veterinary Adviser to the Lake territories, might be considered as the principal veterinary research centre for East Africa, although the claims of the Mpapwa Laboratory in Tanganyika to this distinction must be considered. Both Dr. Walker at Nairobi and Mr. Hornby at Mpapwa have much to gain by association under one roof. On the other hand, the establishment of a research centre for investigations in connexion with tropical diseases other than sleeping sickness would be extremely costly, and in view of the present divergent lines of inquiry, and the sharp conflict of opinion regarding the nature of certain diseases, it is doubtful whether any advantage is to be gained at present by centralisation. But apart from any decision of the five territories to pool their resources to establish research centres catering for the needs of their populations, the need for co-ordination and co-operation between them is imperative.

Unfortunately, while it is admitted that the departments entrusted with human and animal pathological research are inadequately supported, in certain territories there is practically no provision for research of any kind. Not even the important discoveries of Dixey of Nyasaland, or the work of Wayland and Simmons in Uganda, have sufficiently stirred the imaginations of the members of the legislative councils in the other three territories to stimulate them to create geological departments. Very little systematic research is being done in these same three territories in connexion with their economic crops, namely, cotton, maize, coffee, sisal, and wheat, while the possibilities of cinchona plantations have never been explored.

It is hoped that this state of affairs will soon be remedied. Backed by the authority of the Commissioners, there should no longer be any excuse for timidity on the part of colonial governors, most of whom personally are fully alive to the necessity for scientific research, and it is sincerely hoped that they will without delay put bold estimates for its prosecution before the members of their respective Legislative or Executive Councils.

Roman Britain.

(1) *The Roman Occupation of Britain: being Six Ford Lectures delivered by F. Haverfield, now revised by George Macdonald, with a Notice of Haverfield's Life and a List of his Writings.* Pp. 304 + 9 plates. (Oxford: Clarendon Press; London: Oxford University Press, 1924.) 18*s.* net.

(2) *Roman York: the Legionary Headquarters and Colonia of Eboracum.* By Gordon Home. Pp. 204 + 30 plates. (London: Ernest Benn, Ltd., 1924.) 12*s.* 6*d.* net.

THE Ford lectures of the late Prof. Haverfield were delivered at Oxford in 1907. At the time of their delivery it could be said by the man best qualified to judge, "that the enquiry into the history and character of Roman Britain, with all its defects and imperfectations, has been carried much farther than the enquiry into Celtic or Saxon Britain, much farther, too, than the enquiry into any other Roman province; and that our scientific knowledge of the island, however liable to future correction and addition, stands by itself among the studies of the Roman Empire." It was, in fact, high time for such a summary of results and retrospect of the course of inquiry as might be expected from a Ford Lecturer.

(1) Haverfield was not, however, the man to publish immature work. To those who were privileged to hear them, even if they had worked with him and for him—and he had more truly founded a school than any of his contemporaries at Oxford—the lectures of 1907 were a revelation. Yet publication was postponed; the multifarious calls upon the time of a man as efficient and thorough in university business as he was in the direction of research; then the War, the separation of a man capable of great friendships from continental colleagues, the loss of so many of those younger men whom he had trained and inspired; the more urgent need, as it might well seem, to press on with current work and raise a new generation of workers—all this intervened; and the lectures appear now as a memorial volume, at once of their distinguished author and of the three hundredth anniversary of the Camden chair of ancient history, which he filled with such vigour and distinction.

The necessary revision of the drafts, and expansion on the lines projected by Haverfield himself, has been admirably done by Dr. George Macdonald. He has also supplied an intimate study of the man and his life's work, and a bibliography which, though limited to signed articles, and exclusive of book reviews, fills no less than eighteen printed pages, and runs to as many as twenty-five items within a year.

The study of Roman Britain is an instructive example of the ups and downs of scientific method. Quite early in the Saxon period, the standing monuments of Roman occupation, and the folk-memories of the surviving British, challenged attention and comment; and on the British side of the breach, there were traditions older still, of the coming of the Romans, and of Celtic resistance to them. There has never, therefore, been a time when the written history of our island did not begin with a retrospect of "Roman Britain." In the twelfth and thirteenth century renaissance, which gave us the University of Oxford, it was the same, with the new habit added, of describing the monuments, deciphering inscriptions—more generally practicable then than now, for a generation whose learned language was still Latin—and attempting to identify places and lines of communication.

The achievements, in this direction, of men like Henry of Huntingdon and Geoffrey of Monmouth, have been the sport and the despair of their successors; and their ghosts still haunt the local antiquary. Even quainter disservice was done by renaissance scholarship abroad, whereby, in great ignorance of topography or philology, "obsolete names of cities are set forth in modern fashion," as in Servetus's edition of Ptolemy. But the "Britannia" of William Camden was recognised, at its first publication in 1586, as opening a new era of research. It stands, indeed, by the side of the classics of natural philosophy as a pioneer of the "advancement of learning." Camden, however, though a great antiquary, was not a first-class scholar, nor of scrupulous accuracy or self-criticism among the temptations which beset all historians; and some of his fertility of imagination and slovenliness in detail has descended upon lesser men, along with his inspiration to systematic field-work. Other handicaps were, the disfavour into which the Society of Antiquaries fell under James I.; the divorce between this London institution, after its revival in Stukeley's time (1717–18), and the literary scholars of the Universities; and Stukeley's disastrous acceptance of the forged "Richard of Cirencester," which perverted almost everything that was written on Roman Britain for a full century after 1757.

It was only gradually that the Roman occupation of Britain came to be conceived either as a well-marked and peculiarly self-contained phase of British history, or as the local aspect of Roman imperialism. The antiquarianism of the eighteenth century was the hobby of a few able amateurs. Camden's classification of antiquities by counties stimulated much county-history and local patriotism of a competitive kind, but few attempts to comprehend either whole periods or natural regions. Then, the new movement in scientific thought which escorted revolutionary politics, and the æsthetics of romance, found one of its characteristic expressions in a philosophy of history, and another in the regional studies appropriate to an age of nationalism. While Niebuhr, Ranke, and Mommsen were making history scientific, the squirearchy and the "literary and philosophical" societies made archæology popular and fashionable. The "new rich" of the early nineteenth century, as of the seventeenth and the twentieth, followed the fashion; became collectors and sometimes connoisseurs; and the "classical" education administered, for similar reasons, to their sons perpetuated an acquaintance with Cæsar's work, at all events, as part of the outfit of a gentleman, though at some risk to his knowledge of either history or geography.

Probably there are few countries even now in which an interest in "Roman remains" lies so near the threshold of consciousness as in Great Britain; few, however, in which the notion of them as a subject of scientific investigation was, until recently, so remote even from the minds of many scholars.

The last generation of explorers, indeed, has had almost as much to undo as to create. The ancient divorce between university scholarship and regional field-work, which was notorious in Camden's time, persisted within living memory; and in so far as they have been reconciled, it has been the work of two men, Haverfield himself and Pelham, his predecessor in the Camden chair. The characteristic English disbelief in anything systematic or expert, whether in training or in research, is hard to defeat: the distance between London and the older Universities hampers intercourse between literary scholars and the national storehouse of antiquities; the "little learning" of the local antiquary, and his invincible antipathy no less to the use of modern languages than to continental methods of study, have stultified the benevolence of local patrons, which has often been generous.

For this state of things there are two remedies, illustrated respectively by the two books under review: better scholarship and wider background of knowledge on the part of the local field-worker—and a book like that of Mr. Home on Roman York would have been impossible twenty years ago—and closer and more persistent supervision and correlation of the field-worker by what may justly be described as a head-quarters staff. For, as Haverfield frankly concludes, in his first Ford lecture, the reforms effected by Mommsen in particular in Roman history during the nineteenth century "make it doubtful if the amateur can in the future do any real good." "The whole subject has become much harder"; for "to-day, unfortunately, accuracy is necessary"; and if we seem not to know now a good many things which passed current with antiquaries half a century ago, it is due to ruthless critical revision by a few men of scientific training and experience.

This critical revision of our knowledge of Roman Britain has various opportunities and lines of approach, as the lectures in question demonstrate. The older historical geography dealt with political boundaries "described pictorially on a series of vari-coloured sheets": it is quickly giving place to a "wider and a wiser view" of the relations of geography and history, which "begins by studying the physical features of the world by themselves," and only afterwards attempts, in the light of this knowledge, "to deduce the influence of these physical features in any special period." Only so, as in the first lecture of this series, can the strategy, for example, of Claudius's campaign of conquest in A.D. 43 be appreciated, and the later operations of Agricola, Hadrian, or Ulpius Marcellus, and the revolt of Boadicea, or of the North in the days of Commodus, be set in their true perspective. Only so, too, in the light of continental work on the defences of other frontiers of the Empire, on the Rhine or the Danube, and in North Africa, can the details of legionary camps and outposts of "auxiliary" troops be seen as parts of an orderly scheme of occupation; and as Mommsen himself admitted, "you have such wonderful inscriptions in your North country; no land tells us more about the Roman army."

Similarly, the "Romanisation" of Britain is a special and exceptionally interesting example of a process which was going on, during the same four centuries, in many border-lands. Here again geographical conditions impose rather austere control; all higher culture being "confined to a definite area in the centre, the east, and the south of the province. . . . Outside that area we may search practically in vain for traces of civilised life. Inside it we shall find almost nothing else," for when the military situation was once defined, troops were not stationed where they would be out of action in case of trouble on the border. There are qualifications to be made, of course; "in particular, the midlands were thinly and poorly peopled," yet in Cambridgeshire, as Dr. Cyril Fox has shown more recently, it was during the Roman occupation that we can first trace any serious exploitation of the forest-ridden clay-lands. There is a practical moral to this midland emptiness: "amidst its great woodlands, and on its damp and chilly soil, agriculture can expand only under the exceptional conditions of a Napoleonic war. Pigs, sheep and cattle may flourish better. . . ." So history repeats itself, and Roman Britain in its broad geographical features was very much the Britain that we know.

More important was the life of the towns; and though the first novelty of Haverfield's pioneer essay on

"Ancient Town-planning" is necessarily lacking here, the sections on the origin, structure, and outlay of Roman British settlements, the adaptation of Roman architectural conventions to British weather and Celtic habits, and the pottery and other characteristic elements in the furniture of the houses so developed, receive much fresh illustration and commentary, and no less perpetual challenge to traditional misconceptions, whether the true story be available as yet or not.

Especially is this suspension of judgment, out of very fullness of knowledge, apparent in the last of these lectures, entitled "Roman Britain and Saxon England," and dealing with a state of things of which the writer frankly says that "if for one reason it is not to be called a period, for another reason it can hardly be called history. Like the Bronze Age or the Early Iron Age, it lies outside the ordinary range of historians. . . . The early history of the English has been written often enough. But it is mostly fiction. . . . The Celtic world is equally unhelpful; . . . I do not think I have exaggerated the general uncertainty which overhangs large parts of Celtic studies. Here assuredly is no place for a respectable historian." In Roman Britain, on the other hand, even for so severe a sceptic, "ancient history comes to the rescue of modern": archæological detail purging the rhetoric and errors of Gildas. But "legend is history personified in fiction. When we can test legends, the general history . . . proves usually to be true. . . . The heroic age remains—without the heroes." "The struggle between Roman British culture and Saxon barbarism was on a small scale, truly, compared with the great continental movements of that age; but it was evenly balanced and fiercely disputed."

Here archæology confirms legend utterly. "The conquerors were destroyers." "No case is known where Saxons dwelt in a Roman villa"; either by sack and burning, or by evacuation, the Roman towns ceased to exist as settlements as completely as those of the Danubian frontier of Noricum. Geographical criticism comes to the rescue here too. The Saxons' advance "went on to the same point as did Rome's," and coots laid their eggs in the baths of Aquæ Sulis. "Like Rome's, it came to a stand at the foot of the hills" in more violent conflict with the Saxon Chronicle than with the surviving Britons. But "for the rest, the Roman has passed from Britain as though he had never been." Once more, however, this is the regional and, therewith, ephemeral view. "Had Rome failed to civilise, had the civilised life found no period in which to grow firm and tenacious, civilisation would have perished utterly." But geographically Britain is an over-straits counterpart of what then was Gaul; while Britain became England, Gaul became France; and in the longer story of the Romanisation of the Frank, and through him of Saxon and Angle overstraits, we have the link which makes the history of Roman Britain a chapter consecutive with our own.

(2) That a book like Mr. Home's "Roman York" should find a public or a publisher at all, is testimony to the real and wide interest in Roman studies, to which allusion has already been made. When we consider, as the author notes in his preface, that the total number of original references to York in extant Latin literature is five, and that the remainder of its history must be reconstructed, like a mosaic, from "indirect references and inferences" in the light of epigraphic and archæological evidence, we realise the amount of laborious compilation which goes to a book of more than two hundred pages. It is indeed, at the same time, rather more than a history of Roman York, and a good deal less than a survey of Roman Britain, but the preface disarms criticism of its discursiveness, and indeed Mr. Home has only applied to a particular site the method of illustration from other places and districts which gives their value to the Ford lectures.

Mr. Home, however, is evidently not a trained scholar; his Greek, and occasionally even his Latin, needs revision; his references are few, usually without "chapter and verse"; in a note on p. 59, all three titles refer to the same document! His English equivalents for Roman official titles are not always happily chosen; his account of the status of *municipium* and *colonia* is obscure; and he refers on p. 76 to "Huns" in connexion with the events of A.D. 284. But he evidently knows York and its neighbourhood intimately; he has had the valuable help of Dr. Collinge, Mr. Foord, and other local colleagues; he writes vigorously and easily, with a certain touch of imagination which gives coherence to a needlessly large mass of detailed information; and he has illustrated his book with a number of excellent photographs. He does not often mention Haverfield's work, and occasionally falls foul of his opinions; but, knowingly or not, he has learned in his school, and his essay is living testimony to that revolution in method of which the Ford lectures are an epitome.

Astrophysics without Mathematics.

Modern Astrophysics. By Prof. Herbert Dingle. Pp. xxviii + 420 + 46 plates. (London and Glasgow: W. Collins, Sons and Co., Ltd., 1924.) 30*s.* net.

THE science of astrophysics has long since attained its majority. It no longer needs the protecting presence of some more ancient science. But it is at present almost devoid (at least in the English language) of that ordinary possession of a science—a literature. It needs its classics, its authoritative treatises, its workaday text-books, its popular expositions; its works of compilation and its works of criticism; its manuals of theory and its manuals of practice.

Prof. Dingle's "Modern Astrophysics" is a pioneer attempt at a popular exposition of the subject. As stated in the preface, the book is intended for the general public, and the popular appeal has been given first place. It is also comprehensive. Its aim is to put a reader without specialised knowledge—either of physics, astronomy, or mathematics—in possession of the main range of facts of observation and of the theories propounded to explain them.

It may be said at once that not only is there no book in English which even attempts this, but also that Prof. Dingle's book does, in some measure, achieve its aim. The demands on the non-technical reader are probably greater than the author supposes, but he who is prepared to face four hundred pages of somewhat amorphous prose is conducted twice round the whole existing observational material, is taken to some of the bounds of existing knowledge, and is invited to peer into the recesses of the unknown. The author conscientiously faces what he considers to be difficulties, and the book abounds in independent criticism, though not all of it is wise. The popular reader will, however, feel that the greatest respect has been paid to his intelligence and that he has been brought to the point at which he can at least frame sensible speculations of his own.

In collecting and arranging the facts, Prof. Dingle has expended a great deal of labour—labour which will make easier the future tasks of others. For he has at least got the subject into a whole, though rather a shapeless one. In this connexion it may be regretted that references are almost non-existent. If these had been given even only for the tables, the professional reader would have been greatly helped and the non-professional one would not have been unduly terrified.

The book would have been improved if the frequent dramatic and rhetorical passages had been omitted, for they do not succeed in stimulating the reader's emotions more than would simple narration. The author says that lack of space has compelled him to be brief. This shows want of self-criticism. The book suffers from diffuseness. Repeatedly, loss of clarity arises from excess of explanation. Though possessing the literary form of a popular work, it has the dimensions and price of a treatise, and if it had been reduced by one-third and better provided with sub-headings, it would have been more readable and nothing of value need have been omitted.

When one of the objects of the book is to give non-mathematical explanations, it is disappointing to find how often opportunities have been missed. It is possible to show very simply, in words, how the angular diameter of a star is connected with its surface brightness, how observations of eclipsing binaries yield determinations of stellar densities, and how the detection of a moving cluster plus a single determination of radial velocity affords good determinations of parallax. These are three simple, fundamental relationships. Yet, in Prof. Dingle's text, the first is confused with the irrelevant introduction of parallaxes and the linear diameters of stars, the second is stated but not argued out at all, and the third is omitted save for a reference to the fact that if the parallax of one star of a moving cluster can be found, those of the remainder follow from the common velocity. Similarly, no attempt is made to show why the period of a Cepheid should be expected to decrease with increasing density.

This is the more to be regretted in that occasionally the author conveys much in little in an illuminating epigram; for example, when he remarks that though novæ have been classed as variable stars, "it is doubtful if such a classification expresses anything more than the philosophical instinct which craves after generalisation"; or again, referring to the maintenance of energy, "evidently one of the star's most important functions, the expression of its instinct for self-preservation." And such a sentence as "A variable star is not the opposite of a fixed star" is a charming example of tenderness for the difficulties of non-professional brethren.

Serious criticism can be made of the actual substance of parts of the book; for example, the chapter on the sun contains a long description of the "rival theories" of the gaseous photosphere and the cloudy photosphere, and the author gives the impression, and evidently himself believes, that there is little to choose between them. Surely this is to speak the language of a previous decade. It may be regarded as completely certain, and is believed on reasoned grounds by the overwhelming majority of astrophysicists, that the photosphere does not consist of opaque incandescent clouds. The author largely ignores the accepted results of mathematical investigations, whilst attempting to traverse the same ground verbally; for example, the difficulty that has usually been encountered as soon as we leave the primrose path of verbal argument and introduce a few numerical estimates of orders of magnitude is that the sun's boundary ought to be more sharp than it is, not less sharp.

Again, in dealing with the interior of a star, the author shows how radiation is handed on from layer to layer, and gives an indication of how the effect has been calculated by Eddington. But in dealing with the sun he remarks "how the energy distribution would be related to the huge range of temperature from the sun's centre to its surface is indeed a difficult problem. It may be doubted whether the relation would have much analogy with that observed when an isothermal surface is the radiator." This is not the only failure on the part of the author to assimilate different researches. Though there is an introductory section on the theory of spectra, much of the book is written in the language of the era before a theory of spectra existed. It would surely be easier for the ordinary reader to understand resonance radiation and fluorescent radiation rather than to be mystified by "non-temperature" radiation. Referring to Wolf-Rayet stars, Prof. Dingle says: "The Wolf-Rayet stars seem to be the only ones that behave as a wholly gaseous mass should. Their spectra are intelligible—a faint continuous spectrum due to the light from the high-pressure interior which has managed to escape, with a bright-line spectrum from the surface layers impressed on it." Prof. Dingle is entitled to state his intuitive expectations, but he is not entitled to ignore the fact that mathematical analysis shows that a star is to be expected to give an *absorption* spectrum; the difficulty is to explain the existence of bright lines. On another page, however, he remarks that "Wolf-Rayet radiation in general is inexplicable." His attempted explanation of a bright-line spectrum—that when a star reaches its maximum temperature its central parts are cooling whilst the outer ones are getting hotter—is inadmissible, as a simple calculation shows.

The author suggests that the hypothesis of the synthesis of helium from hydrogen is in keeping with the observed fact that the second spectrum of a nova has weaker hydrogen lines and stronger helium lines. He apparently fails to realise the exceedingly small mass of gas required to produce even intense lines, an amount utterly negligible compared with the amount either of hydrogen or helium probably available.

Prof. Dingle says that, as we do not know the precise conditions inside a cooled star, we are at liberty to make any assumption that is not inconsistent with the little we do know. He then says that diffuse nebulæ may arise from a catastrophic outburst in a cooled star, for "we cannot say this is impossible so long as the heat energy inside the star is sufficient to account for the gravitational potential energy of the nebula, and there is every reason to believe it is sufficient." But why this meticulous respect for the first law of thermodynamics when the second law is being thrown overboard? It is simply not fair to the non-technical reader to make unorthodox suggestions of this kind without fully pointing out the nature of their unorthodoxy.

The looseness shown by these examples is unfortunately present in much of the reasoning. Astrophysicists will use the book for its summaries of facts, but they will be little helped by its theoretical discussions.

E. A. M.

Blood Pressure in Early Life.

Department of Applied Statistics: University of London, University College. Drapers' Company Research Memoirs: Studies in National Deterioration, XI. Blood Pressure in Early Life: a Statistical Study. By Dr. Percy Stocks, assisted by M. Noel Karn. Pp. iii+88. (London: Cambridge University Press, 1924.) 12*s*. net.

DR. PERCY STOCKS' work entitled "Blood Pressure in Early Life" immediately strikes one as filling a long-experienced gap in medical literature. Systolic and diastolic blood pressures are, indeed, taken to-day almost as a routine in the medical examination of a patient, and yet it is notorious how hazy are the conceptions of the normal range of these at different ages. Especially is this the case in childhood and adolescence. The principal aims of the author, therefore, were to investigate the behaviour of blood pressure during the period of puberty and adolescence, to ascertain the normal range of systolic, diastolic, and pulse pressures at ages from 5 to 40, and to examine the interrelation between these pressures and their correlation with pulse rate, physical development, muscular strength, respiratory and psychological factors, social-class and athletic habits of life.

The study, which is a statistical one, has brought out some extraordinarily interesting results, a few only of which we shall be able to mention. The systolic pressure is found to rise as a simple function of age from 5 to 11 years. The onset of puberty causes a well-marked accentuation in the gradient until at the age of 19 a maximum of 130 mm. of mercury is reached. From 19 to 40 years of age no further change is detectable. The comparatively rapid rise in systolic blood pressure during puberty is shown to occur even when the figures are corrected for body weight, which itself is positively correlated with blood pressure. The diastolic pressure, on the other hand, shows an interesting divergence from the course of the systolic pressure in its rate of rise with age. In this case puberty at first hinders the rise, but this delay is followed at the age of 18 by a rapid rise to a maximum during the 21st year. Following this the diastolic pressure tends to fall somewhat up to the 37th year.

It is evident from the age curves of systolic and diastolic pressures that the pulse pressure increases rapidly at the onset of puberty to reach a maximum about the age of 18, after which it decreases equally rapidly into the 21st year, afterwards increasing again as a simple function of age.

These are some of the facts elicited by this statistical study. Their explanation is, however, difficult. Dr.

Stocks incriminates the endocrine glands in the causation of the rapid rise in systolic pressure during adolescence. Experimental evidence, however, is against any pressor action by the sexual hormones, although the possibility of the suprarenal and pituitary bodies being in part or in whole responsible for this change cannot be excluded.

The product of pulse pressure and pulse rate is found to show a well-defined maximum during adolescence, the curve representing this product plotted against age showing a superficial resemblance to that of rate of growth in body weight. The author tentatively agrees to the contention that this product is "an index of cardiac energy expended per minute in maintaining the circulation," and even gives this contention the dignity of an algebraic notation. We would point out that the static factor in the total energy expenditure of the heart per minute is proportional to the product of the minute output and the mean blood pressure during the ejection phase of cardiac systole, and is in no direct manner connected with the pulse pressure. Indeed, the fallacy of Erlanger and Hooker's suggestion was conclusively demonstrated by Miss Skelton, who found that the relation between the pulse-pressure pulse-rate product and cardiac output in the isolated mammalian heart varied enormously, in one experiment between 7 to 1 and 47 to 1.

Again, in adults, assuming that the average output per beat remains fairly constant up to middle life, and assuming that the pulse-pressure pulse-rate product is an index of the energy expended by the heart per minute, the author draws the conclusion that the mean product of pulse rate and pulse pressure is directly proportional to the mean work done by the heart per minute, which is merely one of the author's original assumptions. Figures calculated on the further assumption, that the mean output per beat in large groups of growing individuals is proportional to the mean body weight of these groups, show that if the assumptions are justified one "must conclude that, when the age of 16 has been reached, the average heart is performing as much as or more gross work per minute than in adult life, in spite of the fact that it has presumably not attained full size."

Thus many of the conclusions of the author are based on the assumption that the pulse-pressure pulse-rate product is an index of the work done by the heart. We repeat that the experimental evidence strongly militates against the validity of this assumption. This, of course, merely throws doubt on the interpretation of Dr. Stocks' statistical figures. The results obtained are of absorbing interest and undoubtedly will be of great value to medical officers of schools and of insurance companies.

Our Bookshelf.

The Physiology of Photosynthesis. By Sir Jagadis Chunder Bose. Pp. xx+287. (London: Longmans, Green and Co., 1924.) 16*s.* net.

LIKE all the monographs from the Bose Research Institute, this book contains an account of new and very ingenious experimental methods, and a discussion of experimental results, so detached from the general current of plant physiological literature that it becomes difficult, if not impossible, to assimilate the new data into the fabric of the science. Practically all experimental work is carried out upon the water plant *Hydrilla verticillata*, a most ingenious bubbling method being used to record the rate at which oxygen is released during photosynthesis.

The presence of nitrogen in the gas expelled from the plant is prevented by supplying the plant with water free from nitrogen, but where experiments are being carried out in varying concentration of carbon dioxide, the earlier work of Devaux on the intercellular atmosphere of water plants suggests caution in the interpretation of experimental data. This method, however, rendered automatic by an electromagnetic device registering on a recording drum, should prove of very great value and have many applications.

Two other methods are also used; in one the rate of oxygen discharge is recorded by the change of buoyancy in a submerged shoot attached to a torsion balance; in the other the bubbles are removed by raising the shoot out of water and then measuring afterwards, either in a torsion balance or a precision chemical balance, the change in *density* of the submerged shoot due to the accumulation of assimilates which are assumed to be carbohydrates. Osmotic and turgor changes with their consequent effects upon the volume of the intercellular air system are apparently disregarded in this method.

As to experimental results, Prof. Bose has previously briefly reported in NATURE (July 21, 1923) the great sensitiveness of the process of photosynthesis to certain substances, as, for example, to the slight amount of nitric acid carried down into the ponds after thunderstorms. Many of his more general conclusions will probably not attract so much attention as the new experimental methods he has employed.

The Cultivation of New Zealand Plants. By Dr. L. Cockayne. (New Zealand Practical Handbooks.) Pp. 139+21 plates. (Auckland, Christchurch, Dunedin, Wellington, Melbourne, and London: Whitcombe and Tombs, Ltd., n.d.) 4*s.* 6*d.*

DR. COCKAYNE deserves the thanks of those interested in horticulture, both in New Zealand and in the British Isles, for his excellent practical book on New Zealand plants. It is of particular value for plant lovers in New Zealand, since he brings to their notice the many remarkable native plants which gardeners are so apt to neglect. It is too often the case abroad to find that residents, who may have come from the Home Country, desire only to grow those plants they have known at home, and often neglect almost entirely the native plants of their country of adoption. Similarly, in Botanic Gardens in the Colonies, it is common to find the native flora very largely neglected and mainly a fine collection of exotic plants under cultivation. From Dr. Cockayne's book it is clear that in the New Zealand Botanic Gardens, care is taken to cultivate as representative a collection of the native plants as may be possible, and the efforts which have been made in this direction are worthy of the highest praise.

Now, thanks to Dr. Cockayne, the same aim is made possible and easy to all lovers of gardens in New Zealand, as he gives full particulars as to how the plants should be procured and cultivated, and then goes on to give lists with good descriptive accounts and methods of propagation of the native trees, shrubs, Veronicas, herbs, climbing plants, and ferns suitable for gardens. There is also an interesting and very useful chapter on native plants suitable for town gardening. The book is well illustrated with a number of excellent plates, and is also furnished with a map and full index.

Das Wildseemoor bei Kaltenbronn im Schwarzwald: ein Naturschutzgebiet. Von Dr. Karl Müller. Pp. vi + 161 + 19 Tafeln. (Karlsruhe i. B.: G. Braun G.m.b.H., 1924.) 3 marks.

THE author spent thirteen years in the study of a particular piece of moorland vegetation in the vicinity of Baden-Baden. The area is noted for its beauty and Dr. Müller loves the region—a fact that contributes largely to the fascination of the book. But not only is the Wildsee a lovely spot, it is also of peculiar interest to the student of natural history. In his preface the author states that "the fauna and flora of the moor is that of the arctic-alpine type comparable to an island in the midst of a sea of central European forest produced by the ecological singularity of its position." Beginning with the history of the moor from the eighteenth century, he traces its life from 1780 when its water supply was originally tapped. Later, the peat itself was exploited. During these periods the drained areas were utilised for afforestation schemes with conspicuous failure—sphagnum growing most freely on those areas from which the peat had been taken and reconverting them into bogs. Since the Armistice, the author's knowledge of the region and his numerous articles upon it have assisted in the postponement of further projects to use the peat, and we are glad to learn that this beauty spot will be converted into a national preserve. The book is well illustrated with photographs and maps, but unfortunately it does not possess an index, though it has a good table of contents. It will appeal to all lovers of Nature and to students of ecology, for it contains much useful material attractively arranged regarding moorland vegetation. It was written originally to interest all who know and love this beautiful part of the Black Forest.

Lectures on the History of Physiology during the Sixteenth, Seventeenth and Eighteenth Centuries. By Sir Michael Foster. Second impression. Pp. vii + 306. (Cambridge: At the University Press, 1924.) 15*s*. net.

THE history of the development of medicine has been told many times, but rarely can we gather from these accounts a clear history of the two subjects, anatomy and physiology, on which the whole structure of the art of healing is built. The late Sir Michael Foster's book consists of the "Lane Lectures" delivered at the Cooper Medical College in San Francisco in the year 1900, and gives a detailed narrative of the progress of physiology in the sixteenth, seventeenth, and eighteenth centuries. In one important detail it differs from many books of its type; it is the history of the science, not merely of the lives of scientific workers. Yet there is included much about the personal histories of the pioneers of physiology which stimulates additional interest in the fruits of their labours. The author sees in the efforts of Vesalius against the blind dogma of the Middle Ages the foundations of modern physiology and modern anatomy; Harvey's great work he regards as the direct outcome of those efforts. The influence of advancing knowledge of physics and chemistry is demonstrated in the ideas of Borelli, Paracelsus, and Franciscus Sylvius. In Mayow's realisation of the functions of what is now called oxygen there is an illustration of how a great scientific discovery may be completely ignored in its time and find recognition after more than a century.

The value of this book may be indicated by quoting the author's own words. "It is one of the lessons of the history of science that each age steps on the shoulders of the ages which have gone before." The student will find in the book an account of this progression which will enable him better to understand the physiology of his own age.

Antiques: their Restoration and Preservation. By A. Lucas. Pp. viii + 136. (London: E. Arnold and Co., 1924.) 6*s*. net.

MR. LUCAS's book forms a very useful introduction to the modern methods of treatment of antiques, on which subject very little literature has hitherto been available, but, being written in as non-technical a manner as possible, it is likely to be of more value to the antiquarian than to the chemist.

The work is divided into four chapters, the first two giving a general outline of methods of treatment, while the third is devoted to their application to specific materials. A short final chapter gives certain simple tests for determining the composition of objects.

While the majority of the methods recommended in the third chapter have been found to be safe, yet certain of the author's methods would be dangerous in inexperienced hands. For example, the treatment of papyrus by soaking in water is somewhat drastic. Papyrus, which is composed of small sheets stuck together, is very liable to disintegrate under treatment, and it is safer to cover it with moist blotting-paper. But in most cases the author has confined himself to describing methods of proved utility, which, provided the fragility of antique objects is borne in mind, can be used by any one. He is, however, rather unduly fond of the use of paraffin wax, to which cellulose acetate is usually to be preferred.

Some of the tests given in the fourth chapter may be misleading to those ignorant of science. The methods given for determining specific gravity are likely to be very inaccurate. Since the object must in any case be weighed in air, it would be better to instruct the reader how to weigh it in water, for only thus can an opinion be formed as to the genuineness of, for example, a coin. But these are comparatively small faults in a book which all antiquarians will be well advised to study.

R. A. M.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

On the Luminescence of Solid Nitrogen and Argon.

A LETTER in NATURE of November 15, by Prof. L. Vegard, contained some statements regarding the work of Dr. Shrum and myself on the luminescence of solid nitrogen and argon that would appear to warrant some comment from me. Prof. Vegard's article appears to give the impression that our discovery that the band near $\lambda = 5577$ Å in the spectrum obtained with solid nitrogen had a triplet structure with none of the components coinciding with $\lambda = 5577{\cdot}35$ Å was made after he himself had noted this fact. A reference to our paper will show that it was received by the Royal Society on June 14 of this year. A brief notice of it appeared in NATURE of July 5.

Although we had found late in January 1924, or early in February, that this band, now called N_1 by Prof. Vegard, had a triplet structure, we withheld publication of that fact until a paper by Prof. Vegard should appear, because we inferred from reports appearing in the daily press about that time that he had found the auroral "green line" in the spectrum of solid nitrogen bombarded by electrons.

As no reference to the triplet structure of the N_1 band was made in Prof. Vegard's paper that appeared in *Comptes rendus* of March 31, or in that published in the issue of NATURE for May 17, we communicated our paper to the Royal Society on June 14. On June 17 Prof. Keesom communicated to the International Congress on Refrigeration a paper by Prof. Vegard summarising the latter's work, and dated May 13, with an appendix dated May 31, and a further one dated June 13. In this paper, and in its appendices, again no reference was made by Prof. Vegard to the structural character of the N_1 band.

At the conclusion of the presentation of Prof. Vegard's paper at the Congress of Refrigeration, I was permitted to show our photographs of the solid nitrogen spectrum in which the structure of the N_1 band was revealed. In a paper in *Comptes rendus* of July 7, Prof. Vegard referred for the first time to the fact that the N_1 band had a triplet structure. This paper, therefore, confirmed our results, although no mention was made in it of our work, which was known to Prof. Keesom and others from the Leyden laboratory, on June 17. Again, in a later paper in *Comptes rendus*, July 21, Prof. Vegard dealt with the subject, but again made no reference to our work. It is not conceivable that Prof. Vegard observed prior to June 13 that the N_1 band had a triplet structure with none of the components coinciding with the auroral green line, and withheld that very important fact in the communications of his results obtained up to June 13.

Prof. Vegard suggests that differences between his observations and ours "may be partly due to the fact that their [McLennan and Shrum's] experimental material was very limited, and that their arrangements made it difficult to overlook the experimental conditions and to vary them in a known way." Prof. Vegard apparently is not familiar with the equipment available in the Physical Laboratory at Toronto for researches of this character. Many scientific workers who have visited our laboratory will be able to support me in the statement that our cryogenic and spectroscopic equipment is amply sufficient to meet far more exacting demands than those presented in this investigation. A casual glance at the reproductions of our photographs will show, too, that we were not ignorant of the best methods of using this equipment. Again, Prof. Vegard states that our difference of opinion does not originate from a difference with regard to experimental facts. If we, with our experimental arrangements, were able to obtain the same experimental facts as he did, then he can scarcely maintain that our experimental arrangements made it difficult to "overlook and vary the experimental conditions."

The experimental facts in so far as we know them are as follows:

We found that when solid nitrogen was bombarded with electrons, the luminescent band N_1 consisted of three "broad lines or narrow bands," with mean wave-lengths $\lambda = 5654$ Å, $\lambda = 5617$ Å, $\lambda = 5556$ Å. Prof. Vegard has found the same structure, but with the mean wave-lengths $\lambda = 5649$ Å, $\lambda = 5611$ Å, and $\lambda = 5555$ Å.

We found that solid nitrogen, when made to phosphoresce by electronic bombardment, gave a spectrum consisting in the visible region of a single broad line or narrow band at $\lambda = 5231$ Å. Prof. Vegard finds the wave-length of this narrow band to be $\lambda = 5229{\cdot}4$ Å.

We found that the spectrum of pure solid argon, when the latter was bombarded by electrons, contained a band consisting of two components, one of them being strong with a mean wave-length at $\lambda = 5607$ Å, the other faint with a mean wave-length at $\lambda = 5648{\cdot}3$ Å.

Prof. Vegard, when bombarding solid argon containing varying amounts of solid nitrogen with electrons, found a spectral band consisting of several components with the strongest member having a mean wave-length at $\lambda = 5604$ Å.

We found, when great precautions were taken to purify the argon we used, and only when such precautions were taken, that solid argon phosphoresced after bombardment by electrons, and that the phosphorescence spectrum of argon so treated consisted in the visible region of two broad lines or narrow bands at $\lambda = 4750$ Å and $\lambda = 5300$ Å. This phosphorescence spectrum of solid argon Prof. Vegard does not appear to have observed as yet.

Prof. Vegard takes the view that the results of his solid nitrogen-argon experiments indicated the oscillation of the principal maximum in the N_1 band between $\lambda = 5555$ Å and $\lambda = 5604$ Å.

We feel inclined, however, to the opinion that his results in this connexion indicate that he obtained a mixture of the N_1 components of the nitrogen band and of those of the argon luminescence band in the same region. It may be added that, in our experiments, we found the positions of the components of the N_1 nitrogen band, as well as that of the N_2 phosphorescence band, the same with the solid nitrogen at the temperature of liquid helium as at the temperature of liquid hydrogen.

Our experiments, it may also be stated, did not preclude the possibility of the components of the N_1 nitrogen band originating in the cold vapour of nitrogen in contact with the solid nitrogen. Prof. Vegard is of the opinion that these component spectral bands originated in the solid nitrogen only. He concludes that if the particles of the bombarded solid nitrogen were gradually reduced to molecular dimensions, the N_1 composite band would degenerate finally into the very fine auroral line $\lambda = 5577{\cdot}35$ Å. It is difficult for me to follow him to this conclusion.

It seems to me that in an examination of the validity of Prof. Vegard's theory, some useful information might be gained by directing more attention to the *phosphorescence* of solid nitrogen. Does the phosphorescent nitrogen band $\lambda = 5231$ Å appear in the spectrum of the aurora or not? If it can be shown that it does, we shall know that the region in which the auroral light originates is at or near the temperature of liquid hydrogen. If this line does not appear in the spectrum of the aurora, and if the temperature of the region in which the radiation constituting auroral spectra originates is at or near the temperature of liquid hydrogen, then why does the phosphorescence nitrogen spectral band not appear along with other nitrogen bands found in auroral spectra by Vegard, Rayleigh, and others.

From Kayser's "Handbuch" one finds that a line or narrow band has been found by different observers in the spectrum of the aurora with the following wave-lengths: $\lambda\lambda = 5269$ Å, 5205 Å, 5200 Å, 5233 Å, 5210 Å, 5239 Å, 5207 Å, 5228 Å, 5235 Å, 5166 Å, and 5230 Å; but the list of wave-lengths observed by Vegard in the auroral spectrum and given by him in his paper in the *Phil. Mag.* of July 1923 contains no wave-lengths between $\lambda = 4708 \cdot 7$ Å and $\lambda = 5578 \cdot 2$ Å. It appears, however, from a statement in one of his more recent papers, that he has observed in the spectrum of the aurora a trace of a line near $\lambda = 5230$ Å. Cario has found a band in the spectrum of oxygen near $\lambda = 5230$ Å, and Prof. A. Fowler recently pointed out to me that Ångström and Thalen found a negative band in the spectrum of nitrogen at $\lambda = 5227 \cdot 5$ Å. It seems very desirable, then, to repeat the observations on the auroral spectrum to see if there is any trace of a wave-length at or near $\lambda = 5231$ Å. If such a spectral line or band should be found, it would be well to have a very exact determination of the wave-length made in order to decide whether the corresponding radiation originates in nitrogen in the gaseous or solid state, or in some other element in one or other of its states.

It is unfortunate that Prof. Vegard's brilliant prediction has not as yet received experimental confirmation. It would appear that neither he nor we have as yet obtained with nitrogen or argon, or with mixtures of these two elements at the temperatures of liquid hydrogen or helium, by the use of any agent, a spectrum that includes a broad line or narrow band within a region of 10 Å on either side of the famous "green line," the wave-length of which, according to measurements made with great precision by Babcock, is $\lambda = 5577 \cdot 35$ Å. Nevertheless, Prof. Vegard's theory has been most stimulating, and has led already to the discovery of valuable and important experimental results. J. C. McLENNAN.

The Physical Laboratory,
University of Toronto, December 6.

The Life of Lord Rayleigh.

HISTORY should have as little fiction attached to it as possible and evidence should be tendered in time, by those who can speak with knowledge: this view may not be in accordance with practice: none the less, it may be advocated as desirable doctrine, especially as it has the authority of the author of *Zadig*—a saint recommended for worship by Huxley —who has said:

On doit des égards aux vivants,
On ne doit aux morts que la vérité.

All who knew the late Lord Rayleigh even distantly—he was very difficult of approach—will agree with Sir J. J. Thomson (NATURE, December 6, p. 814) that his son has written his Life with remarkable skill and sense of proportion—but some of us can scarcely admit that nothing more is to be said even of the discovery of argon. The account given is only partial, in no way a complete presentation of the episode; among others, we should like to have heard the views of Gordon, the discoverer's devoted laboratory servitor. Lord Rayleigh was but young at the time of the achievement and cannot have been aware of the state of feeling among chemists—nor, probably, was his father; he will not know in what reverence we held his father's work. I suppose I was behind the scenes as much as anyone, the more as I was president of the Chemical Society, was thoroughly acquainted with Ramsay and his ways and, as is well known, an intimate friend of Sir James Dewar.

At the annual general meeting of the Chemical Society, on March 27, 1895, I made the following statement, to the fellows, in my address:

> Your Council have decided to appoint Lord Rayleigh our Faraday lecturer and to request his acceptance of the Medal in recognition of the important service which he has rendered to Chemistry by his discovery of Argon.

I handed the medal to him from the presidential chair—he accepted it. His guarded remarks in acknowledgment are on record in our Journal. The Chemical Society advisedly took the view that it was *his* discovery. There was the strongest possible feeling among chemists that his name alone should have been associated with the discovery. I may add that, on the same occasion, I had the pleasure of calling upon Ramsay to make public his startling discovery of helium in cleveite, which was thereupon confirmed by Crookes.

Lord Rayleigh has dealt mainly with his father's electrical work (on the ohm and the ampere) and that on argon. Probably some of his readers are disappointed that he did not also summarise his activity in other directions—particularly his work on oil films and capillarity, which is proving to be of special interest and importance in our field. If I be not mistaken, the part he took in such inquiry is not sufficiently recognised. If another edition be called for, let us hope that a chapter on his work in general will be added.

As to my friend Sir Joseph Thomson's gibe at chemistry—what is chemistry? I hold that chemistry and physics are inseparable disciplines, the parting line a broad valley through which both flow; the pity is that physicists so rarely stray from their own region up the chemical slope, that their vision is so little adjusted to our country. They suffer, indeed, from *Chemo-myopia*, not *Chemotaxia*; I fear the fault is congenital. Apart from Regnault and Rayleigh, I believe determinations of the density of gases are all but entirely the work of chemists—has not Sir Joseph heard of Avogadro's theorem and of one Cannizzaro, a chemist and Roman Senator? The determination of gaseous density is *the* fundamental operation in chemistry, as he will see if he consult a bygone classic, Cooke's "New Chemistry." Gaseous density is the foundation stone of our entire numerical system. What, however, can they know of chemistry who only physics know? Indeed, there would seem to be a constitutional aversion from our science in the mind of the physicist—he lacks the necessary freedom of outlook to appreciate our numberless excursions. The slowness with which Lord Rayleigh saw the treasure beneath his feet was probably owing to the fact that he had little real chemical feeling; Ramsay at once appreciated the value of the find when asked to inspect the ground and took shares without hesitation. HENRY E. ARMSTRONG.

55 Granville Park,
Lewisham, S.E.

The Adhesive Apparatus of the "Sucking-fish."

EARLY in 1923 I published in NATURE a short summary of my views regarding the mechanism of the so-called suckers of certain hill-stream fishes and of the "Sucking-fish"—Echeneis.[1] My conclusions were based on an examination of the living specimens of two Indian genera of hill-stream fishes—Pseudecheneis and Glyptothorax, and on a study of preserved material of Echeneis. Later, at my request, Major R. B. Seymour Sewell, Surgeon-Naturalist to the Marine Survey of India, very kindly performed a series of experiments on living specimens of Echeneis. Quite recently (September 1924) I have been able to conduct a few experiments on the "Sucking-fish" in the Marine Aquarium at Madras. In carrying out these experiments I received great help from Dr. Sundara Raj, acting director of the Madras Fisheries, and Prof. H. Parameswaran, of the Presidency College.

The following experiments were carried out to test the sucker-theory of the disc of Echeneis :—

A smooth tin sheet perforated all over with minute holes was taken, and the fish was allowed to attach itself to it. It was observed that the animal was not able to stick to such a surface. This experiment was repeated with a piece of fine-meshed wire gauze in place of the tin sheet with similar results. A wooden plank with a large number of parallel grooves running across it at short distances was next used in place of the wire gauze. The fish could not adhere to such a surface.

In all these experiments it was observed that the fish did not like to have its sucker placed against either very rough or perforated surfaces. Moreover, on such surfaces it secreted large quantities of mucus from its disc. In all probability this secretion helps the fish in sticking to rough surfaces.

The fish was next allowed to adhere firmly to a smooth sheet of glass or a tin sheet. It was found that on such smooth surfaces the fish could be made to slide forwards and sideways without any difficulty, but a certain amount of force was needed to pull it backwards or vertically upwards. When pulled vertically upwards, the resistance against the pull is due to the sucker formed by the disc ; while the force resisting the pull in the backward direction is due to the mechanical frictional device formed by the innumerable backwardly directed spines on the lamellæ of the disc. When lifting the fish vertically upwards it was observed that the entire outer rim of the disc formed a big sucker. In the next experiment the formation of the sucker by the entire rim of the disc was rendered impossible by introducing a number of match sticks under the rim. It was then discovered that a double series of secondary suckers were also formed between the transverse lamellæ on the two sides of the central axis of the disc. This was confirmed by gradually lifting the disc from behind, when it was noticed that each secondary sucker gave way with a hissing sound. It may also be noted that the secondary suckers produced between the lamellæ are not only independent of the outer large sucker, but are also independent of each other.

The disc of Echeneis, when in action, is therefore composed of an outer sucker formed by the rim, and of the two rows of secondary suckers formed in the grooves on either side of the central axis. At the same time, the spines on the lamellæ prevent the fish from slipping off whenever the animal is pushed backwards. The usefulness of the spines for attachment comes into play when the "Sucking-fish" is adhering to such fast-swimming animals as sharks and whales.

The case of the hill-stream fishes is somewhat different. They have only to contend against a rapid-running current constantly flowing in one direction. Suckers under such conditions are probably less useful than non-slipping frictional devices. This is beautifully illustrated by the various Indian species of the genus Garra.[2] In species of the genus which live in lakes and comparatively still water (*e.g. G. mullya, G. gravelyi*), the mental disc is large, and the adhesive pads on the under surface of the paired fins are feebly developed ; while in species which inhabit rapid-running streams (*e.g. G. kempi, G. gotyla*) the disc on the under surface of the head is somewhat reduced and the non-slipping adhesive apparatus on the under surface of the paired fins is well developed. The frictional device in the case of these fishes is more useful than a vacuum sucker. The strength of a vacuum sucker is limited according to its area, while friction increases with pressure, and this in hill-streams increases with the rapidity of the current. The similarity in shape between the anterior dorsal profile of the hill-stream fishes and the anterior ventral profile of Echeneis shows similarity of purpose, which is to utilise the force of the current for increasing the pressure on the disc and thus to make it stick more firmly to the substratum.

SUNDER LAL HORA.

Indian Museum, Calcutta,
November 4.

IN order to test the action and mechanism of the disc in Echeneis and determine whether or not one function of the disc is to act as a sucker (in the strict sense of the word) a series of experiments were carried out.

Specimens were allowed to attach themselves to clean sheets of glass, and the disc was then examined and compared with the surface of the disc when unattached. In the unattached state the rim of the disc is soft and flexible, and around the anterior half of the disc the margin is distinctly raised while the posterior half is flat ; the transverse ridges, on which are numerous posteriorly-directed spines, lie flat against each other and present a practically continuous surface. When the animal has attached itself by the disc, the smooth flexible margin can be seen to be closely pressed against the glass. The ridges running transversely are now separated from each other by narrow spaces, the two series, *i.e.* right and left, being separated by a median soft band that passes backwards in the middle line. At the posterior end this ridge stops short, so that the terminal posterior part of the sucker is occupied by a single large cavity the floor of which is depressed. Air bubbles can be seen between the ridges and in this posterior chamber, and such bubbles may be seen passing along each side of the median partition into the posterior chamber.

When thus attached, if the posterior rim of the sucker is separated from the glass, air immediately enters the posterior chamber, and by slowly pulling the sucker away, each pair of spaces between the transverse ridges can be opened separately, each giving way with a slight sucking noise ; each compartment of the disc thus appears to act as a separate sucker.

When once the fish has attached itself by the disc it can be moved forwards or sideways easily, the disc sliding over the plate, but on attempting to pull the

[1] Hora, NATURE, 111, p. 668 (May 19, 1923). See also Rec. Ind. Mus. 25, pp. 587-591 (1923).

[2] Hora, Rec. Ind. Mus. 22, pp. 533-587, pls. xxiv.-xxvi. (1921).

fish backwards, the spines on the ridges of the disc come into action and tend to prevent any backward movement; and if the disc is forcibly pulled backwards the spines can be heard scraping over the surface. If pieces of twine are placed across the disc they prevent the formation of the necessary partial vacuum and the disc fails to adhere, while if the disc is adherent, the introduction of a finger-nail or the blade of a knife between the disc and the surface to which it is adhering allows air to enter and the hold of the sucker is immediately destroyed.

Further experiments were conducted in order to try to determine the strength of adherence of the sucker. In the first experiment, an Echeneis was allowed to attach itself to the enamel surface of a dish and a hook attached to a spring balance was passed through the gill from one side to the other. By standing on the dish and pulling on the spring balance, the amount of force could be fairly accurately measured, and in the specimen experimented with the fish withstood a vertical pull of more than 30 lb. before the hook tore through the tissues of the body. Further experiments were conducted by allowing the fish to attach itself to an enamel iron tray, the tray being fastened securely by rope to the spring balance. The balance was attached to a stanchion, and the head of the fish was seized in a towel to prevent the fingers from slipping on the skin of the fish. Two examples withstood a vertical pull of 34 and 35 lb. respectively before the sucker was pulled away from the surface.

It seems to me that there is little doubt that the disc acts as a true sucker by the creation of a partial vacuum, while the spines are of use in preventing this sucker from sliding on the surface of attachment. Thus, during life, when the Echeneis attaches itself to some other larger fish, it is owing to the partial vacuum formed that the disc adheres to the surface, while the spines prevent the Echeneis from being swept backwards by the rush as the large fish makes its way through the water.

A further point worth noting is that when the sucker is in action and the Echeneis is attached to any object, all movement of the fish, except that of the mouth and gills that is necessary for respiration, seems to be suspended and inhibited. The fish hangs absolutely motionless, and in the case of a fish that had been well hooked, it was found that by allowing the fish to attach itself to a glass plate it would hang motionless, while the hook and a great part of the fish's jaw was cut out! It appears that the action of the sucker causes inhibition of all movement of the body and tail.

In order to avoid any misapprehension, may I be allowed to add that my experiments were carried out at the suggestion of Dr. Hora, and the above notes, recording the results, were forwarded to him last January.

R. B. SEYMOUR SEWELL.

R.I.M.S. *Investigator*.

Transmission of Stimuli in Plants.

AN article appears on the above subject in NATURE of October 25, in which reference is made to Mr. Snow's experiments (Proc. Roy. Soc., Series B, Vol. 96, No. 678) in support of Ricca's theory that conduction of stimulus in Mimosa is brought about by the transpiration-current in the wood carrying a hypothetical stimulating substance. Mr. Snow joins two cut pieces of stem of *Mimosa pudica* by a tube filled with water and applies a flame to the lower half of the stem with the result that the leaves of the upper half of the stem undergo a fall; hence it is concluded that stimulus is conducted across the water-gap. Mr. Snow also finds that the transpiration-current in Mimosa travels at about the same rate as the conducted excitation. I have carried out numerous experiments with this plant relating to the supposed transmission across a water column, taking the precaution that the heated air from the flame did not excite the upper leaves: other modes of stimulation were also used which were less open to sources of error. In no case did I find any evidence of the transmission of stimulus through the tube filled with water. This is confirmed by the results obtained by Prof. Koketsu last year, who found that when the petiole of Mimosa was cut into two halves, and rejoined by a water-tight tube filled with water, stimulus applied on the distal half was never conducted across the gap. (R. Koketsu, Journal of Department of Agriculture, Kyushu Imperial University, Vol. 1, p. 55, 1923.)

I also applied a chemical stimulant, and made a simultaneous determination of the rate of the transmitted excitation and of the rate of transport of the stimulating substance. The two rates are of very different order, that of the transmission of excitation being at least a hundred times the quicker. The slow transport of a stimulant or of a hormone, and the rapid transmission of excitation, ought not to be confused with each other. I have carried out numerous experiments which prove conclusively that the transpiration-current has nothing to do with the conduction of the excitatory impulse.

In the article referred to above, I find no reference to my earlier researches on the transmission of excitation in Mimosa, some of which were published so far back as eighteen years ago ("Plant Response," 1906; "An Automatic Method for the Investigation of Velocity of Transmission of Excitation in *Mimosa*," Phil. Trans., 204, B, 1913; "Irritability of Plants," 1913; "The Dia-heliotropic attitude of Leaves as determined by Transmitted Nervous Excitation," Proc. Roy. Soc., B, Vol. 93, 1922). These researches proved conclusively that the *conduction is a phenomenon of propagation of protoplasmic excitation*. This was proved by numerous experiments of a crucial character. Among these may be mentioned the characteristic polar effect of a constant current in protoplasmic excitation. I have shown that an excitatory impulse is initiated at the cathode at "make" and at the anode at "break" of the current, the excitatory impulse being afterwards transmitted to a distance. I have shown, moreover, that the interposition of an electrotonic block arrests the excitatory impulse in the conducting tissue of the plant as in the nerve of the animal. The above results have since been fully confirmed by Koketsu. The characteristic effects described above disprove the theory that the transpiration-current is concerned in the conduction of excitation. A full account of my more recent experiments will, I hope, be published shortly.

J. C. BOSE.

Bose Institute, Calcutta,
November 20.

An Approximation to the Probability Integral.

REFERRING to Prof. H. C. Plummer's letter on "An Approximation to the Probability Integral," published in NATURE of October 25, the following alternative way of representing the normal error function by simple approximation may be of interest. The original demonstration of this has been given by me in Physical Department Paper No. 8, "A Method of Curve Fitting."

Let us write as follows the equations for the three variables x, y, z.

$$z = \frac{a}{\sigma\sqrt{2\pi}} e^{-x^2/2\sigma^2}, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$y = \int_0^x z dx. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the above-mentioned paper it is shown that the equation

$$z = k\left(\frac{a^2}{4} - y\right)^{0{\cdot}8111} \quad . \quad . \quad . \quad . \quad (3)$$ [1]

gives a very close approximation to the values taken from a Probability Integral Table. The numerical value of k may be computed from

$$k\sigma = \frac{1{\cdot}2318}{a^{0{\cdot}6222}}. \quad . \quad . \quad . \quad . \quad (4)$$

Now, eliminating z from equations (1) and (3):

$$y = \sqrt{\frac{a^2}{4} - 10^u}, \quad . \quad . \quad . \quad . \quad (5a)$$

$$u = \log_{10}\frac{a^2}{4} - 0{\cdot}2677\left(\frac{x}{\sigma}\right)^2. \quad . \quad . \quad (5b)$$

Putting $a = 1000$, the following Table shows the degree of accuracy of the equations (3), (5), and of Prof. Plummer's formula.

From Prob. Integral Table.			From (3).	From (5).	Plummer's.
	z.	y.	z.	y.	y.
0·0	398·9	0	400·1	0	0
0·5	352·1	191·5	351·8	189·0	191·5
1·0	242·0	341·3	240·6	339·2	342·0
1·5	129·5	433·2	129·7	433·1	435·1
2·0	54·0	477·3	56·3	478·3	478·8
2·5	17·5	493·8	19·9	494·7	488·4
3·0	4·4	498·6	5·6	499·0	..
3·5	0·9	499·8	1·3	499·9	..

Prof. Plummer's formula may be used only up to $x/\sigma = \sqrt{6} = 2{\cdot}449$ (see NATURE of October 25). It is simpler than equation (5), and is to be preferred whenever values of y are quickly wanted for small values of x. However, in practical statistical work, problems may arise requiring one of the variables x, y, z to be determined in terms of one of the other two over the whole of the range of x. For such cases, when great accuracy is not wanted, the above equations may be useful to replace Probability Integral Tables. The variable x may be computed in terms of y from equations (1) and (3).

S. KRICHEWSKY.

Physical Department, Cairo.
November 22.

[1] The exponent of equation (3) has been previously given as $=0{\cdot}7864$ and has been recently corrected to $0{\cdot}81111$ by more rigorous methods. Equation (4) has been correspondingly corrected.

The Word "Scientist" or its Substitute.

I DO not think exception can fairly be taken to the adoption into a living language of any word that (1) contributes to convenient expression and (2) violates no rule or custom in etymology. Contemporary speech has this advantage over a dead language as a vehicle of thought, that it can be adapted to changing circumstance, whether that be effected by modifying the meaning of old vocables or by the addition of new ones. The invention of printing did much to arrest colloquial change and to standardise speech, but a useful measure of elasticity still prevails. Examples in point are the verbs "to burke" and "to boycott," which it would be very inconvenient to discard.

It has been pointed out by correspondents in NATURE that there is plenty of analogy in sound English for the formation of "scientist" from "science." Sir E. Ray Lankester objects to the term because there is no precise definition of science; but surely we all know what is meant by a "man of science," for which term "scientist" seems a neat synonym, standing aptly in antithesis to "sciolist"—one who has a smattering of some branch of knowledge.

HERBERT MAXWELL.

Monreith, Whauphill,
Wigtownshire.

WRITING as a student of the history of words, "scientist" can never become a *permanent* part of any language, for its quantity is "impossible." It has a destructive effect in a sentence, and when spoken the last syllables must be gobbled. "Naturalist" may be gobbled fairly easily; few people notice it; but "scientist" is difficult. So perhaps it scarcely matters whether the word receives or not the approval of the dictionaries; words which we instinctively feel are repulsive drop out of use.

The only possible salvation for the word is for its advocates to introduce the more correct pronunciation "sciéntist," that is, middle syllable accented. Thesist, logist, are alternatives which suggest themselves; the latter would be in conformity with "biology" and the many other "logy's."

REGINALD A. FESSENDEN.

45 Waban Hill Road,
Chestnut Hill, Mass.,
December 15.

The Spectrohelioscope.

I AM very glad to learn from NATURE of November 8, p. 683, that Mr. F. Stanley is also engaged in developing a spectrohelioscope. In my long focus (13 feet) instrument, where the slits are rather narrow and hence close together in order to give sufficient light with the requisite purity, the motion of the spectral line is practically equal to that of the slit for the small displacements from the optical axis involved. Thus it is possible to avoid the use of such deflecting prisms and gearing as Mr. Stanley employs. I have not yet attempted, however, to design a short focus instrument.

A rotating disc with radial slits makes a natural appeal to the instrument designer and I used it for my first (unsuccessful) experiments, made on Mount Wilson with the 30-foot spectroscope of the 60-foot tower telescope many years ago. It will serve very well with a moderate number of slits when there is sufficient light, but the high purity required for observations of the hydrogen flocculi complicates the problem. For example, in order to obtain with a disc the purity and brightness I now command with an oscillating bar (carrying two sets of five slits each), it would be necessary to use about 400 radial slits, each 0·003 inch wide and with errors of spacing less than 0·001 inch. This can, of course, be done, and I shall probably try it, but the simple oscillating bar suggested itself as an easy means of making a rigorous test of the method for the exacting task of observing the hydrogen flocculi against the brilliant disc of the sun.

GEORGE E. HALE.

Pasadena, California,
December 3.

The Compton Effect.

Apropos of the very readable review of the discussion on scattered X-rays at the Toronto meeting of the British Association, published in Nature of October 25, p. 627, we presume your readers may be interested in a bit of new evidence on the reality of the Compton effect, obtained since that meeting.

We have photographed spectra of scattered molybdenum rays, with the tube and scatterer in one room and the spectrograph in another. The walls near the tube and scatterer were lined with lead, and the room was large enough to remove all other substances exposed to the rays to a safe distance. By that we mean a distance such that even if those substances converted all the radiant energy falling on them into tertiary rays, the total quantity of tertiary radiation sent by them to the " scatterer " would be reduced by the inverse square law alone far beyond detection. We built two such sets of apparatus, in different parts of the room, and used them at different times. One was a molybdenum-target tube with a sulphur scatterer. The other was another molybdenum-target tube, of a different form, with an aluminum scatterer, using a spectrograph of quite a different design.

Both of these sets of apparatus gave spectra like those described previously by Ross (*Phys. Rev.*, Nov. 1923; Proc. Nat. Acad. Sci., July 1924), which had given the first definite proof of the existence of the Compton effect in elements other than carbon. These new spectra showed the Compton-theory lines as strong as in the earlier spectra. Apparently, therefore, these experiments confirm the calculations described at the Toronto meeting showing that those spectra were quite free from any contamination by box effects, and were due only to the elements to which Ross ascribed them.

D. L. Webster.
P. A. Ross.

Stanford University, California, U.S.A.,
December 3.

Arsenic in Oysters.

Your contributor J. S. G. in reviewing (Nature, December 20, p. 913) the report of the Ministry of Agriculture and Fisheries on oyster mortality in 1920, and dealing with the finding of 3·7 parts of arsenic per million in oysters from certain beds, comments on the serious questions raised by such a fact. A definite pronouncement is needed as to what constitutes danger. As is well known, the Royal Commission on Arsenical Poisoning, in its report in 1903, recommended that no substance used in the preparation of food should contain more than 1/100 grain per lb. This recommendation has been adopted as a standard for years, and many prosecutions have been successful for quantities but little in excess of this. Surely the position is now more illogical than ever it was. Is the fishmonger to be prosecuted if his oysters have three parts per million (1/50 grain per lb.), and, if not, why should a grocer be charged if, say, a baking powder—probably not made by himself—contains this amount?

Results of investigation in Sweden show that fish may have an arsenic content up to four parts per million, and my own analyses of fish sold in the London market, recently communicated to the Society of Public Analysts, confirm this figure in respect of certain plaice. The administration of the Sale of Food and Drugs Acts in this respect requires revision.

H. E. Cox.

The Laboratory, 11 Billiter Square,
London, E.C.3,
December 24.

Convective Equilibrium.

There is a rather important consideration connected with the production of cloud in rising air currents (which does not seem to have received attention) the effect of which is to check uprushes of moist air.

I have often noticed the cumulus clouds over a distant thunderstorm suddenly cease to rise and then disappear. Whilst rising, their outlines are clear and sharp. As soon as the rise ceases their outlines become indistinct. Then a partially transparent veil appears where the cumulus cloud was, and this fades away entirely, leaving the atmosphere clear. Why do cumulus clouds fail to rise so high as one would expect them to?

Warm saturated air near the earth's surface (warmed by proximity to the ground) rises and steadily cools. After the dew point is reached important results follow. Condensation takes place and the air is then prevented from falling in temperature so much as it would do if it were dry, and this would accelerate the velocity of the up current if it were not for the presence of the *newly formed cloud particles*. The small drops forming the cloud are floating in the air and contributing their quota to its weight. Condensation thus tends to check the ascent of the mass. When the particles become large enough to descend quickly, as rain or hail, they draw the air down with them and the uprush ceases.

The mechanical effect of falling rain, and the great density of a cloud due to the weight of the water particles in it, do not seem to have been generally considered.

R. M. Deeley.

Tintagil, Kew Gardens Road,
Kew, Surrey,
December 24.

Molecular Dimensions of Celluloid.

The results of the experiments carried out by the Bureau of Standards, Washington (Nature, December 13, p. 861), are of very great interest, but would be still more valuable if the exact composition of the " celluloid " were known. There is often confusion in referring to such words as " celluloid," " celloidin," " collodion " and the like. Celluloid, the basis of photographic film, is certainly a manufactured mixture of variable composition, containing among other ingredients a considerable percentage of camphor, the main ingredient being, of course, a soluble cellulose trinitrate, though cellulose acetate is now being increasingly used on account of its non-inflammability. It is, therefore, scarcely correct to speak of the " molecular complex of celluloid " (unless " complex " is intended to cover a mixture of two or more compounds). " Celloidin " is the trade name of a carefully purified and soluble cellulose nitrate, probably approaching to a single chemical substance. Collodion is, of course, a solution of nitro-cellulose in acetone, ether-alcohol, or some other organic solvent.

Henry Garnett.

3 Lea Road,
Heaton Moor,
near Stockport,
December 14.

Erratum.—In Nature of October 4, p. 499, a letter from Prof. Was. Shoulejkin on " A New Method of Investigating Sea Waves " is subscribed " Technical High School, Moscow." The work described was carried out at the Physical Institution of the Science Institution of Moscow.

The Theory of Evolution since Darwin.[1]

By Prof. E. W. MACBRIDE, F.R.S.

NO event in the intellectual world has had such a profound influence on the mental outlook of mankind in general as the publication of the " Origin of Species " by Charles Darwin in 1859. A distorted version of the theory embodied in it figured largely in the propaganda that led to the Russian revolution. Nothing could be more interesting to the student of the history of thought than to analyse the ideas and assumptions involved in this theory and to trace their fate in the subsequent course of scientific criticism.

The outlines of Darwin's theory are familiar to all, and the technical terms which he introduced—" natural selection," " the struggle for existence," " the survival of the fittest "—have passed into everyday language. It is to be noted that he termed his theory " Descent with modification," and that the name " evolution " was first applied to it by Herbert Spencer. This is significant, because in Herbert Spencer's mouth, evolution denoted a general theory of everything that was going on in the universe, of which the development of animals and plants formed only a small part. We must emphasise, therefore, the fact that Darwin takes life and the fundamental properties of living things for granted; he does not strive, like Spencer, to explain them in terms of matter and motion.

Founding on what he could observe of the nature of life as he saw it, Darwin strove to argue back to what was the condition of the living world in past times and what are the influences at work in modifying it at the present time. He assumed that animals and plants are continually giving rise to small variations in all directions and that these variations are inheritable. This is the positive idea which is embedded in what is generally called the theory of " natural selection." For " natural selection," the death of the many and the survival of the few, is only the pruning knife and can of itself originate nothing new. These random variations Darwin attributed not to chance but to the environment, which, as Darwin imagined, produced an instability in the hereditary qualities of organisms which are exposed to new conditions. But besides this effect, Darwin asserted that the results of increase in size of organ by use and its diminution in size by disuse were inheritable. He was driven to this assumption by the endeavour to account for the disappearance of unused organs in such cases as that of cave-animals which had lost their eyes. He could not conceive how small decreases in size in organs which were no longer used could give their possessors the victory in the struggle for existence. Darwin imagined, further, that the struggle for a bare existence was in many species accompanied by a struggle to find a mate, a struggle which was undertaken by the male; and he attributed the varied peculiarities of colour, shape, ornaments, and voice which distinguished males from females, to the preference of the female for the most attractive male. The results of this supplementary struggle were termed by Darwin " sexual selection." This part of the theory has not met with general acceptance, and we can therefore leave it out of account.

The difficulties in his theory which Darwin felt most acutely were the beginnings of useful organs from small useless rudiments and the origin of the sterility which usually exists between allied species. The first difficulty has been largely removed by the progress of embryological research, for it has been shown that new organs do not originate from useless rudiments but by the modification of older and simpler organs, but the second difficulty has not been fully solved even to-day. Believing as he did that allied species were only more sharply differentiated local races of the same species, Darwin yet had to admit that local races were mutually fertile, and that allied species when crossed were sterile. He pointed out that sterility could arise between members of the same species as it has done in the case of the primrose, and he imagined that sterility was, so to speak, a by-product of increasing divergence of constitution.

Of course, as Darwin himself pointed out, many naturalists before him had interpreted the likenesses of allied species as evidence of blood relationship, and had even put forward speculations as to the course of evolution; but the weak point in all these theories had been that they did not point to causes now in existence by which evolution could have been brought about. The first effect of Darwin's theory was to convince scientific men, especially naturalists, that evolution really had occurred and that it was in fact still going on. The feature in the theory which secured their assent was " natural selection." It seemed to his contemporaries that Darwin had laid hold of a fact which, when once attention was directed towards it, no one could deny, namely, the death of the many and the survival of the few. It is true that Lamarck had previously relied on natural causes, such as the effects of use and disuse and the production of new habits as the agents in evolution, and I myself believe that he was in great measure right, but the changes of habits in animals which he postulates are phenomena which are slow in their effects and do not constantly occur, and they are not easily observable within the limits of a human life. Moreover, the facts which now give support to Lamarck's views were not known when he wrote, nor even when Darwin wrote, and hence Lamarck failed to produce an effect on contemporary thought at all comparable to that which was effected by Darwin.

Huxley in England and Haeckel in Germany were the great champions of the evolutionary principle against theological opponents. Haeckel[2] made to the theory one great addition which is of far-reaching consequence: it had been dimly perceived by Darwin and is hinted at in the " Origin of Species," but it was first clearly enunciated by Haeckel. In his " General Morphology," first published in 1866, he stated that " Ontogenesis, or the development of the individual, is a short and quick repetition (recapitulation) of phylo-

[1] From a lecture delivered at King's College, University of London, on November 28, for the Board of Studies on the History, Principles, and Methods of Science.

[2] My friend Prof. Dendy has pointed out to me that Haeckel derived his " Law of Biogenetics " from Meckel, who wrote in 1827, but as Meckel was opposed by von Baer, the great comparative embryologist of the day, his views were ignored.

genesis, or the development of the tribe to which it belongs determined by the laws of inheritance and adaptation." This in shortened form, "Ontogeny is a recapitulation of Phylogeny," was called the "Fundamental Law of Biogenetics."

The validity of this law Haeckel sought to establish by numerous examples, and he has been accused, apparently with justice, with misrepresenting some of the facts in order to bring them into conformity with it. He is said to have made four separate drawings of the same embryo and to have labelled them with different names so as to show the identity of the early embryonic form in four different groups. This was detected, and together with the wildness of much of Haeckel's speculation tended, especially in Germany, to produce a strong reaction against most of Haeckel's teaching. But in spite of Haeckel's unscrupulous action he was in many respects a far-reaching genius. Though some naturalists have affected to disbelieve in the biogenetic law, in practice, in their morphological reasoning all assume its validity. Even Haeckel's special theory, namely, the derivation of the higher animals from a single hypothetical hydra-like ancestor, which he called the "gastræa," has received more and more confirmation as embryological research has proceeded.

Haeckel never thought that variations could be due to chance; on the contrary, he attributes them all to differences in nutrition. Some which we call "directly adaptive," like the effects of use and disuse, which alter the flow of blood to an organ and so change its nutrition, show their effects in a single generation, whilst others which he calls "indirectly adaptive" only make themselves felt in the next or succeeding generations. He says that the "superiority of Englishmen is due to their being fed on excellent beef; but the beef results from the cattle being grazed on rich clover pasture. Clover is fertilised by wild bees, but wild bees are decimated by field-mice, which are kept in check by cats. Cats are usually kept by old maids, and therefore this peculiarity of unmarried females is the original cause of British pre-eminence."

Huxley, though he contributed immensely to the building up of a scientific zoology on the basis of Darwin's theory, added nothing to the theory itself, but one of his sayings may be quoted as showing his insight into the matter at stake. "We have got our theory of evolution; what we want now is a good theory of evolution." We shall find that practically all subsequent criticisms of what for brevity's sake we may term Darwinism turn on these two questions, namely: (1) What is the nature and cause of variation? (2) Why are species usually mutually sterile?

We have already directed attention to the fact that the development of animals and plants from simpler ancestors was included by Herbert Spencer in his theory of evolution, a theory intended to explain everything that was going on in the universe on the basis of the laws of mass and motion. But Spencer laboured under all the disadvantages which beset those who write treatises on subjects with which they have only a second-hand or superficial acquaintance. It is true that in his "Principles of Biology" he sees clearly that the vital point at issue in Darwinism is the origin of variations, and he throws his whole weight on the side of the inheritability of the effects of use and disuse. But his arguments are not convincing, since they always involve a "petitio principii"; he never seems to see that a thorough experimental examination of the subject is necessary, but considers that his point is proved by vague plausible suggestions. He suggests that animals are made up of "physiological units," and that these units become modified in response to changed conditions and are then passed into the germ-cells and make their effect felt in the next generation. This conception resulted from his comparison of the regeneration of the limb with the self-completion of a broken crystal in its mother liquor, a conception which we now know to be utterly futile and misleading. No doubt Spencer did much to popularise the idea of evolution with the general public, but it is impossible to point to one addition of any scientific importance which he made to the theory of evolution.

It is true that Darwin in a later and far too imperfectly known book, "The Variation of Animals and Plants under Domestication," put forward a suggestion somewhat similar to that of the "physiological units" to explain the origin of variation. This is the theory of "pangenesis." According to this hypothesis, every part of the body of animal or plant is continually throwing off "gemmules" which are endowed with the power of multiplication and with the capacity, in suitable circumstances, of growing into the likeness of the part from which they came. These gemmules are carried from part to part by the circulation, and they accumulate in the germ-cells. They become altered in character in accordance with the alteration of the part from which they come, and when sufficient of the altered gemmules have accumulated in the germ-cells, the alteration becomes hereditary. Darwin's gemmules, however, are living units endowed with the characteristic vital properties of growth and reproduction, whereas Spencer's physiological units are merely large organic molecules.

The next important event in the history of the theory of evolution is the advent of Weismann, who in 1885 published his "Essays on Heredity," which were later consolidated into a book termed "The Germ-Plasm." Until his time it had been tacitly assumed that the effects of use and disuse could be inherited; and the only reason for refusing to assign to them the exclusive importance attributed to them by Lamarck was the difficulty which many naturalists felt in explaining some evolutionary changes by the accumulation of the results of efforts on the part of the animals or plants which exhibited them. But Weismann challenged the validity of the whole of the Lamarckian doctrine, and this he did on two grounds: first, that the evidence in favour of acquired characters was not sound, and secondly, that on account of the structure of what he calls the "germ-plasm," acquired qualities could not be transmitted. He therefore sought to explain the origin of variations by what I can only term the accidents besetting the ripening of the egg. Since all the father's potencies in heredity are contained in the sperm-head, which is a condensed mass of chromosomes equal in number to those in the ripe and unfertilised egg, the real bearers of hereditary powers must be the chromosomes.

If now we suppose that a single chromosome, which Weismann terms an "idant," nay, even a portion of a chromosome which he calls an "id," is theoretically capable of carrying out the entire development of the species, but that different "ids" would give rise to slightly different types of development and that the actual development of the egg is a compromise between the potencies of the various "ids," we arrive at the Weismannian explanation of variations. If it is mere chance which group of "ids" are thrown out of the nucleus when the number of chromosomes is halved at ripening, then obviously all sorts of different groups may be left in the make-up of the various eggs and various sperm-cells; and these different groups will give rise to different inheritable variations of which natural selection will preserve those best adapted to the circumstances of the animal.

But why should the various "ids" differ from one another? To this question an extraordinary answer was given by Weismann. When a cell divides, the nucleus divides first and each chromosome gives rise to daughter chromosomes by longitudinal splitting, which then by nourishing themselves at the expense of the protoplasm grow as big as their parent. During this growth, variations in nutrition of the daughters are the causes of variations in their structure which are handed on to their posterity at the next division! So that the very principle which Weismann rejects at the beginning of his hypothesis, he reintroduces in dealing with the chromosomes, namely, external circumstances giving rise to inheritable variation. Let us now glance for a moment at the way in which the theory is worked out in detail. The fertilised egg begins its development with a selected group of "ids" derived in equal proportions from the father and the mother; each of these "ids" contains within it the entire potency to bring about the development of the adult form. When the egg-cell divides, the division, though apparently an equal one, is in reality unequal—one daughter retains the "ids" undisturbed, but in another the "ids" have already undergone decomposition and are now represented only by "determinants," each of which has only the potency of causing the development of that part of the body to which this daughter-cell will give rise. As development continues, the determinants suffer further decomposition until at length they are resolved into "biophores," which have only the capacity of conferring on the cell in which they lie the power to become a muscle-cell, a nerve-cell, a gland-cell, etc. The descendants of the other daughter-cell of the original two, however, receive unbroken "ids" and eventually give rise to the germ-cells of the next generation. Their lineage is called the "germ-track."

On the Weismannian hypothesis, then, the germ-cells are separated from the body at the very beginning of development and are not afterwards influenced by what happens to the body during its growth; and Weismann, with true Teutonic thoroughness, having formed this conception of the structure of animals, declares it be *a priori* impossible that acquired characters, *i.e.* new habits, could affect the germ-cells, since, as he states, he can form no conception of how the change in structure resulting from a changed habit can be represented in the nucleus of the germ-cell. The experiments alleged to prove the inheritance of acquired qualities which were available to Weismann were supposed examples of the hereditary transmission of mutilations; these he sought to explain away, and he cut off the tails of mice and bred from these maimed animals, and showed that the offspring did possess well-developed tails.

Weismann's doctrines obtained wide acceptance, and were regarded as finally disproving the environmental origin of variations, and the position remained practically unchanged until the beginning of the twentieth century.

We may now pause for a moment to take up the question of the sterility between species. In 1897, Romanes, a Cambridge man who had migrated to Oxford, published a book entitled "Darwin, and after Darwin." In this book he considers the question of how two divergent species could have arisen from one, and he points out that it is inconceivable that this could have happened by the natural selection of accidental variations, unless two portions of the original species had become isolated from one another so that cross-breeding was prevented. Romanes puts forward the idea that owing to accident (*i.e.* unknown causes) physiological variations occurred, so that two portions of the same stock became mutually sterile. He thinks that geographical isolation is insufficient to account for the appearance of sterility, because species occupying adjacent territories are often mutually sterile, and he cannot conceive how what seem to him slight differences in climate can effect so great a change as sterility, but as to the causes of the appearances of this "physiological isolation" he gives no hint whatever. No further light was thrown on this subject until the last year or two, when Goldschmidt took up the question of the mutual fertility of local races of the same species when crossed. He showed in the case of the gipsy moth that when two races from widely separated localities are crossed, sterile intersexes are often produced which render the continued propagation of the mixed race difficult. His results therefore support Darwin's hypotheses of sterility as a by-product of increasing divergence of constitution.

The twentieth century began with two events of decisive importance for the theory of evolution; namely, the rediscovery of Mendel's researches by Correns in 1900 and the publication by Johannsen in 1903 of a paper entitled "The Bearing of Pure Lines on the Theory of Inheritance." Mendel's researches, which were carried out at the same time as Darwin's work and published soon after the publication of the "Origin of Species," appeared in local journals of too limited circulation, and attracted no notice from the general scientific world. They had in Mendel's mind no relevancy to the formation of species whatsoever, but were concerned only in discovering the laws governing the distribution of paternal and maternal characters amongst hybrid offspring. Mendel in his experiments always chose varieties sharply separated from one another by definite clearly-cut characters. The rediscovery of Mendel's work gave a great impetus to the carrying out of experiments in crossing all kinds of variants from the normal with the type. In these experiments Bateson in England and Morgan in the United States took leading parts, and it was soon discovered that those sudden deviations from the normal which turn up without assignable cause in most breeds of domestic animals and varieties of cultivated plants obey the Mendelian rules when crossed with the types. Johannsen's results proved the non-inheritability in the case of beans of those small random deviations from the normal in all directions on which Darwin had laid such stress, and these results were independently confirmed by Jennings, who worked on Protozoa, and by Agar, who studied small Crustacea (1912).

In consequence of these discoveries, biological opinion veered round in favour of regarding the conspicuous aberrations commonly known as sports as the raw material on which natural selection had worked. Darwin, it is true, had considered the question of whether sports might not be the starting-point of new species, and had decided that they could not be so on account of the rarity of their occurrence. He thought that the chance of such a sport mating with its like, even if highly successful in the struggle for existence, was infinitesimal, and that if it did not mate with its like its characters would be "swamped by intercrossing." But if the deviation were in a direction favoured by natural selection, it might be assumed that it would make itself felt in lesser degree even when its orginal possessor crossed with the type, and the first generation descendants might still survive on account of its lessened manifestation. Many have claimed that this argument is strongly reinforced by the discovery of the laws of Mendelism. For if the deviation behaved as a dominant when crossed with the type, all the first generation of the descendants of such a cross would show it in as strong a manner as its orginal possessor; and even if it were recessive it would appear in undiminished strength amongst one-fourth of the second filial generation. The real objection to regarding sports as the initiators of new species lies deeper. An animal does not survive on account of one organ. In its growth from the egg to the adult, it runs the gauntlet of many dangers, and a strong development of some one organ might determine its survival at one period of its existence, but if the deviation occurs very rarely the chances against that particular animal reaching the critical stage at all are enormous.

De Vries, the director of the botanical gardens at Amsterdam, carried out between the years 1886 and 1899 a series of cultures of the garden plant *Œnothera Lamarckiana*, commonly known as the evening primrose. He showed that every year a number of sports turned up, usually about half a dozen in 10,000 specimens, sometimes as many as three in a hundred, and that this sports usually, though not always, bred true when crossed with their like. De Vries called these sports "mutations," and imagined that he had surprised a species in a "fit of mutation," and that new species were not produced by a slow process of differentiation but by sudden jumps, so that they began their existence complete in all their details, as Minerva sprang from the head of Jove. The De Vriesian doctrine joined with Mendelism, and became the dominant doctrine of evolution and heredity for most of the first quarter of the twentieth century, and probably counts amongst its adherents a larger number of biologists than any other doctrine at the present time. It was first seriously put forward by Bateson in 1894 in a book entitled "Materials for the Study of Variation," in which he figured and recorded a large number of examples of monstrous deviations from the normal, and laid down two doctrines, one of which is undeniably true, whilst the second is really the De Vriesian theory. The first was "variation is evolution"; the second, the "discontinuity of species is due to the discontinuity of variation."

The De Vriesian view has reached its climax in a book termed "Age and Area" by Dr. Willis, a distinguished botanist. This book was published two years ago; in it Dr. Willis supports the idea that species originate in sudden inexplicable jumps which occur only rarely. This idea is difficult to distinguish from the pre-Darwinian doctrine of special creation. No wonder that Haeckel, who lived long enough to encounter this view in its early presentation by Bateson, said: "If views like this are to be accepted, it would be better to return to Moses at once."

(*To be continued.*)

Biographical Byways.

By Sir ARTHUR SCHUSTER, F.R.S.

INTRODUCTION.

THERE are things seldom referred to in obituary notices and sometimes omitted even in more ambitious biographies. They tell the tale of peculiarities or weaknesses, which the writer fears may detract from the merits of the man he has set out to praise. The biographer believes, with some show of justice, that his main object is to give a record of work accomplished and not a psychological analysis of character. But eccentricities, or even decided failings, form part of a man's personality. The extent to which his teaching carries conviction and affects the scientific outlook of his time, depend as much on his personal attributes as on the merits of his researches. We destroy the balance of a just valuation, if we ignore those shades of character or temperament which act as handicaps to the full fruition of his work.

It has been my good fortune to be acquainted personally with many of the men who laid the foundations of the science of the nineteenth century, and I have retained a vivid memory of such intercourse as I had with them. In writing down some of my recollections I have tried to outline personalities in a sympathetic spirit. If human frailties are sometimes exposed, I hope that the limits of allowable candour have never been transgressed, and that, apart from the personal factor, the incidents related may be found to contain some substantial contributions to the history of science during the middle period of last century.

1. URBAIN JEAN JOSEPH LEVERRIER (1811–1877).

Towards the end of December 1874, or nearly in the year 1875, I received an invitation from the Royal Society to take part in an expedition which was being organised to observe the total solar eclipse of April 1875 in Siam. Norman Lockyer was expected to act as leader of the expedition, which was to start in February; the time for preparation was therefore short. Ultimately Lockyer, who was then acting as secretary to the Royal Commission on Science Teaching presided over by the Duke of Devonshire, did not obtain the leave of absence he had expected, and I was put in charge. I had never had any experience in mounting or dismounting astronomical instruments, or indeed in using them, and one of the appliances on which we depended—a large siderostat—was under construction and not expected to be ready before the eve of our departure. It was essential that I should get some knowledge of the instrument, and more especially of the process of silvering the mirror, which was to be a foot in diameter. A similar siderostat was in use at the Paris Observatory, where M. Adolphe Martin had found a simple and convenient method of silvering large surfaces of glass. I was therefore sent to Paris, the consent of Leverrier, the famous director of the Observatory, having been obtained.

I first called on Cornu to ask advice on some optical questions that had arisen and, needless to say, I met with a most friendly reception both from him and other scientific men, notably Jamin. When Cornu heard that I was to call on Leverrier, he shook his head and said: "Je ne sais pas si M. Leverrier est l'homme le plus détestable à Paris, mais je sais que c'est l'homme le plus détesté." This was not encouraging, and it was in fear and trembling that I entered the Observatory.

I was received by one of the assistant observers, C. Wolf, who remarked with a look full of sympathy: "You will find M. Leverrier in a very bad temper: he has just returned from a funeral." I was then barely twenty-three years old, and naturally looked upon Leverrier (who was then sixty-four) as one of the formidable veterans of science. I was ushered into the "Presence," received with a searching look and the abrupt question: "Qui êtes-vous et que voulez-vous ici?" I mildly answered that I understood Mr. Lockyer had written to explain the purpose of my visit. "So he has," said Leverrier, "but I want to hear it from you." After I had replied to the best of my ability, I was dismissed with the remark: "I have already given instructions that every assistance should be given you."

I spent an interesting and instructive week practising Martin's silvering process, which has the great advantage that the surface comes out polished, except for a thin veil which is easily removed without appreciable friction. When it was time to return home, I suggested to M. Wolf that it might be sufficient for me to write a letter of thanks to Leverrier without troubling him with a personal call. But Wolf would not hear of this, and I was shown again into the "lion's den." Leverrier was sitting at his desk, and by his side stood a trembling young assistant to whom he continued to speak, taking no notice of me. I listened to a conversation of which I remember the main points without pretending to literal accuracy.

LEVERRIER. And so you tell me, that after trying for a whole week you have not yet found the mistake in your calculations?

ASSISTANT. No.

LEVERRIER. You have, of course, applied the correction for . . . (I did not catch the details).

ASSISTANT. Yes.

LEVERRIER. Did you apply it with a plus or a minus sign?

ASSISTANT. Plus.

LEVERRIER. It ought to be minus. That is your mistake. Go and correct your calculations.

After the assistant had left the room, Leverrier chuckled. "I knew all along," he said to me, "what his mistake was, but I wanted to see whether he could find it out by himself"; and to my great surprise he continued, "come and have a walk round the garden." All traces of peevishness and severity had disappeared, and for half an hour or more he became a most interesting and encouraging talker. A new reflecting telescope was just being erected in the grounds of the Observatory, and he explained the uses to which it might be put, inviting me, whenever I felt inclined, to come and work with it.

He then began to speak on a subject on which he evidently felt very strongly. Great preparations had been made in the previous year, and much money spent, on fitting out expeditions to observe the transit of Venus, which had just taken place on December 8, 1874. The French Government had followed the example of other countries, but against Leverrier's advice. It was, of course, well known to astronomers that he preferred other methods of determining the solar parallax; but the Government would not listen to his advice. "Que voulez-vous?"—France had recently been defeated in war, and if she did not share in international work, the Government was afraid that its action might be misinterpreted and believed to be due to sulkiness or want of funds. But Leverrier strongly expressed his opinion that the money was all wasted, and that neither this nor the subsequent transit of 1882 would add anything of value to our knowledge. In this he proved to be perfectly right.

2. JOHN PRESCOTT JOULE (1818–1889).

I once asked Joule what he felt like when he heard that one of his papers was rejected by the Royal Society. "I was not surprised," he answered, "I could imagine these gentlemen in London sitting round a table and saying to each other: 'What good can come out of a town where they dine in the middle of the day.'"

There are some interesting and somewhat puzzling circumstances connected with the fate of that paper, which was communicated to the Royal Society by one of its secretaries, Peter Mark Roget, on October 16, 1840. Under the title "On the Production of Heat by Voltaic Electricity," it contained the account of an experimental investigation which had led Joule to formulate his all-important law, that the heat generated in a conductor by an electric current is proportional to the product of the resistance and the square of the current. The paper was read on December 17, and in due course a short abstract appeared in the Proceedings which gave the final result arrived at, and hence secured Joule's priority. It is, therefore, not quite correct to say that the paper was rejected. The difficulty arose in connexion with its publication *in extenso*. The paper was short—it would not have taken up more than four or five pages in the Proceedings—and it was perhaps considered that such far-reaching results could not be proved by the comparatively few experiments conducted by Joule. Criticisms were also made on the ground that previous investigations on the same subject were not mentioned. On March 11, 1841, the communication was committed to the Archives.

A paper carrying the title "On the Heat evolved by Metallic Conductors of Electricity and in the Cells of a Battery during Electrolysis" shortly afterwards appeared in the *Philosophical Magazine*. It bears the date March 25, 1841, and its introductory paragraph concludes with the sentence:

> "I hope therefore that the results of my careful investigation on the heat produced by voltaic action are of sufficient interest to justify me in laying them before the Royal Society."

This remark has naturally led to the belief (definitely expressed by Osborne Reynolds in his extensive memoir on Joule published by the Manchester Literary and Philosophical Society) that the paper, as printed in the *Philosophical Magazine*, is the one declined by the Royal Society. This, however, is not the case. The difference in the title is significant. The Royal Society paper deals with solid conductors, covering only the ground which in the *Philosophical Magazine* appears as "Chapter 1," and is there followed by a second chapter, twice as long, dealing with electrolysis and adding considerably to the range and importance of the results. Even in the first part the two papers are not identical, only a few short paragraphs being unaltered—though it must be admitted that the alterations are not material. It is not at all certain whether the complete paper as it appeared in the *Philosophical Magazine* would have been declined by the Royal Society; but it is perplexing that the reference to the Royal Society has been left standing in the altered and extended paper.

We cannot suppose that Joule deliberately wished to convey a wrong impression, and only one explanation seems to me to offer itself. It may be surmised that some correspondence took place in the three months between the date at which the paper was read and that at which it was committed to the Archives. On being informed of the objections raised, Joule may have prepared a more complete account to be substituted for the paper originally submitted; but the Royal Society having finally declined to print the original paper *in extenso*, he was quite likely to forward the amplified version to the *Philosophical Magazine*, the reference to the Society in the opening paragraph being left standing by an oversight.

It is not my desire to acquit the Royal Society of all blame, but mitigating circumstances might be urged. Joule's experiments no doubt appear conclusive to us, but the very simplicity of his experimental arrangements, and the comparatively few numerical results given, may have raised doubts which were perhaps excusable. The heat generated was determined by the rise of temperature of a measured quantity of water in which a coil of uncovered wire was inserted, and no cooling correction was applied. Though Joule gave good reasons why these simplifications did not affect the result, such cavalier treatment of the minor sources of error may have shocked the academically-trained mind as showing want of respect for the dignity of the problem. It is seldom that referees can rise to the standard of Stokes, who, in reporting on a communication by an eminent man of science possessing great intellectual powers not always assisted by clearness of expression, gave his judgment as follows:—"The first part of the paper I can understand but do not agree with; the second part I cannot understand, but as the results arrived at may be important I recommend that the paper be published in the Philosophical Transactions."

Joule's later work is so intimately connected with the determination of the mechanical equivalent of heat, that the importance of his investigations in other domains is apt to be overlooked. The two volumes of his published researches show that he was by no means a specialist, but only those who knew him personally are aware of the extent of his knowledge and broadness of interests ranging over nearly all branches of physics. He was a pupil of Dalton, who had refused to instruct him in chemistry before he had learned the elements of mathematics. It was perhaps in recollection of his first teacher of science that Joule once remarked to Balfour Stewart: "If I were a young man I would concentrate my attention on atomic weights." When I first became acquainted with Joule, he was a little more than sixty years of age and in full vigour. The meetings of the Manchester Literary and Philosophical Society in those days will always remain in the memory of those who were fortunate enough to attend them. It was the custom then, and I believe is still, to devote the first half-hour to a discussion on any subject brought forward by some member, spontaneously or at the invitation of the president. A regular attendant, Joule was at his best on these occasions. He also made his presence felt at the council meetings as a confirmed conservative opposed to all changes. His health began to fail about 1882, but in November 1885 he dined at my house to meet the late Lord Rayleigh, who had come to Manchester on purpose to make his acquaintance. "I believe I have done a few little things but nothing to make a fuss about," he said, shortly before his mental powers began to fail.

After Joule's death, I was asked by his family to examine his apparatus and instruments—mostly constructed by his own hands—and I was fortunate to rescue his historical thermometers, which were lying covered with dust in an old stable attached to his residence. I was thus enabled to determine the difference between the scale value of the thermometer used by Joule and that of the standard of the Bureau international des Poids et Mesures. It appeared in the investigation that the glass of Joule's thermometers is more suitable to its purpose than the glass afterwards employed in England. The depression of the zero after being raised to a definite temperature is much smaller and more nearly approaches that of the hard glass used by French makers. These thermometers were presented by Joule's son to the Manchester Literary and Philosophical Society: most of the remainder of Joule's apparatus is preserved in the Physical Laboratories of the University of Manchester.

Current Topics and Events.

WE are glad to see that noteworthy recognition is accorded to science in the New Year honours list which was issued last week. The appointment of Sir James G. Frazer and of Sir Ernest Rutherford to the Order of Merit, an order which was "designed as a special distinction for eminent men and women" and is limited to twenty-four members, will give particular pleasure to scientific workers everywhere. Included among the honours are also the following:—*Knights*: Prof. John Adams, Professor of Education, University of London, 1902–1922; Prof. R. H. Biffen, Professor of Agricultural Botany, Cambridge University; Mr. G. R. Clarke, Director-General, Posts and Telegraphs, India; Dr. Hari Sing Gour, Vice-Chancellor, Delhi University; Mr. W. B. Hardy, Secretary of the Royal Society; Prof. F. Gowland Hopkins, Professor of Bio-Chemistry, University of Cambridge; Principal J. C. Irvine, Principal and Vice-Chancellor of the University of St. Andrews; Mr. F. Truby King, Director of the Child Welfare Division of the Department of Health, Dominion of New Zealand; Dr. T. M. Legge, Senior Medical Inspector of Factories; Mr. B. Longbottom, Chairman, British Electrical and Allied Manufacturers' Association; Maj.-Gen. R. C. Macwatt, Director-General, Indian Medical Service; Mr. E. W. Petter, President, British Engineers' Association; Dr. H. J. Waring, Senior Surgeon, St. Bartholomew's Hospital, Vice-President, Royal College of Surgeons, Vice-Chancellor of the University of London, 1922–1924. *C.I.E.*: Mr. H. G. Billson, Chief Conservator of Forests, and Member of the Legislative Council, United Provinces, India. *D.B.E.* (*Civil Division*): Miss L. B. Aldrich-Blake, Dean of the London School of Medicine for Women.

AMONG the mechanical inventions which revolutionised the cotton industry was the cotton gin of Eli Whitney, whose death took place on January 8, 1825—a hundred years ago. The flying shuttle of Kay, the spinning jenny, the water frame and the mule of Hargreaves, Arkwright and Crompton, together with the power loom of Cartwright, increased enormously the rate of spinning and weaving, but the cleaning of the cotton fibre from the seed was still largely done by hand. Whitney's great invention was made in 1792, and in about ten years the export of cotton from the United States rose from less than 200,000 pounds to more than 40,000,000 pounds per annum. The essential parts of Whitney's machine consisted of a grid on to which the seeds were fed, a revolving wooden cylinder studded with wire teeth which tore the fibre from the seed, and a revolving brush which in turn removed the fibre from the wires. One such machine would do the work of 50 men engaged in hand-picking. Whitney was born in 1765—the same year as Fulton—and was the son of a farmer of Westboro, Massachusetts. He worked as an artisan, made money by teaching, and in 1792 graduated from Yale. When about to take up a post as tutor, a chance conversation led to his tackling the problem of cotton cleaning. His gin soon came into use and he had to establish his claims by much litigation. Later, he founded a factory at New Haven for the manufacture of firearms and was the pioneer among Americans in the modern methods of making large numbers of interchangeable parts. He died at New Haven, leaving a part of his fortune to Yale for the purchase of books on physical and mechanical science.

THE year 1924 was a year of notable centenaries, but one of scientific interest seems to have been overlooked, namely, that of the establishment of the first physiological laboratory in Europe. The man who opened this laboratory in the year 1824 was Johannes Evangelista Purkinje, at that time professor of physiology and pathology at the University of Breslau. It has been suggested that it was through Goethe's influence that Purkinje was appointed to this chair, for the author of "Faust" reckoned him as one of his friends, their common interest being in subjective visual phenomena. The reception given by the Prussians to the Bohemian Purkinje was far from cordial, but by his amiability and scientific gifts he lived down all opposition. The laboratory of 1824 was not housed in any building belonging to the University but in the professor's private dwelling. Doubtless the establishment of this first of European physiological laboratories attracted no attention from the contemporary journalists; but when we reflect on the immense benefits which have accrued to medical science from the findings in the laboratories of practical physiology—insulin being one of the latest—all men of science will like to remember that it is just a few days more than one hundred years ago that the academic pursuit of that subject was inaugurated. So far as we can gather, the instruction in Purkinje's laboratory was chiefly in histology, a subject which the anatomists willingly allowed the physiologists to teach for the next century. It is only now that that portion of the physiologists' burden is being cast, where it belongs, upon the shoulders of the morphologists.

A WRITER who signs himself "Poetarum Minimus" enters a plea in the *Scientific Worker* for December for the exercise of poetic expression in scientific fields, and as an example he submits a contribution having for its theme the evolution of stars from "tenuous mists" up to maturity and down to decay. Thus,

> A hundred million million years they scatter largesse of their rays.
> They spend their substance royally throughout the measure of their days.

The verses from which these lines are taken have commendable dignity and sound, and they are reminiscent of the style of Erasmus Darwin, who essayed similarly to express views of the stellar universe current in his time in the sonnet, "Roll on ye stars, exalt in youthful prime." Tennyson surpassed all other poets in the application of scientific truth to poetic purpose, and his astronomical allusions are particularly fine, as, for example, in "This world was once a fluid haze of light," and "Regions of lucid matter, taking form: Brushes of fire, hazy gleams."

Several years ago the geological course of events, from "Nebula to Man," was described in verse in a sumptuous quarto volume by the late Mr. Henry Knipe, and Mr. Alfred Noyes has given us his beautiful epic, "The Torch-bearers," which is on a much higher plane. Poetry, however, is something more than accurate description in verse form. It should, of course, have a certain beauty of sound when spoken, but its main function is the creation of stimulating thoughts which appeal to the human heart rather than to the intellect. While, therefore, we may believe that the wonders of modern science furnish rich material upon which poetic imagination may be worthily exercised, we cannot forget that emotion is independent of knowledge, and that, as Coleridge said, science seeks to know and communicate truth—acceptable or not—but the chief purpose of poetry is to give pleasure. The difference is aptly expressed by Sir William Watson in one of his epigrams, thus:

Science and Art, compeers in glory,
Boast each a haunt divine.
"My place is in God's laboratory,"
"And in His garden mine."

A NEW Research Institute for the improvement of crops, at which special attention will be paid to cotton and to the fundamental problems underlying the production of this crop in India, was formally inaugurated at Indore in Central India on November 24 last. The foundation of this new Institute has been rendered possible by the provision of a valuable site of 300 acres by the Indore Durbar, by a grant of two lakhs of rupees (about 15,000*l.*) for capital expenditure by the Indian Central Cotton Committee, and by an annual contribution of 120,800 rupees a year (a little more than 9000*l.*) for current expenses in addition to the income derived from the land at the disposal of the Institute. This annual grant has been provided jointly by the Indian Central Cotton Committee and by seven of the Central India States (Indore, Dhar, Jaora, Datia, Rutlam, Dewas Senior Branch, Narsingharh and Sitamau). The control of the Institute has been vested in a governing body of six members with the agent to the Governor-General in Central India as president. Three members of the board of governors are nominated by the Indian Central Cotton Committee, one by Indore Durbar, and two by the rest of the contributing States. The Director of the Institute will act as agricultural adviser to the States, and will in this way come in direct touch with the Malwa plateau, one of the most important cotton tracts in India.

THE experimental area which will be at the disposal of the new Research Institute, has been leased by the Indore Durbar to the Institute for 99 years at a nominal rent of 20*l.* a year; it embraces all the types of black cotton soil met with in India, and is very favourably situated for research work on crops. It is close to the city of Indore, now rapidly growing in importance as a commercial, manufacturing, and educational centre, and to the cotton mills. The maintenance of an up-to-date library on crop-production and the training of post-graduate students, selected by the Indian Central Cotton Committee, will be features of the Institute. Mr. Albert Howard (formerly Imperial Economic Botanist at the Agricultural Research Institute, Pusa) has been appointed Director of the Institute and agricultural adviser to States in Central India, and Mrs. Howard (formerly second Imperial Economic Botanist at Pusa) will be employed as physiological botanist at Indore.

ON Tuesday, January 6, Sir Oliver Lodge gave the first of his series of seven fortnightly talks on "The Ether of Space and its Functions," under the auspices of the British Broadcasting Company at 2LO, which was relaid to many stations in the British Isles, and also to the longer wave high-power station 5XX at Chelmsford. The first talk was entitled "First Notions about the Ether. How Matter is held together, and how we see it." After describing the functions and uses of the ether, Sir Oliver went on to explain that matter was discontinuous, consisting of isolated particles not in contact, and was only held together by cohesive forces existing in the ether. He supposed that if we could magnify a piece of matter to an impossible extent, it would have an appearance something like the midnight sky, where the separated pieces of matter are similarly held together or united into systems by the force of gravitation—which also is a function of the ether. So that the ether is a great unifying entity, without which there would be no cosmos, but chaos. Sir Oliver concluded his first talk thus: "You cannot imagine empty space being thrown into vibration; there must be something in space which vibrates, and that 'something' extends to the furthest visible object, and constitutes a unifying and connecting mechanism, through which all our information is obtained. . . . We have as yet very little acquaintance with the universe; sometimes we seem to know a great deal, at other times we realise that we hardly know anything. The mystery of it all escapes us, and the possibilities of it are beyond our conception: many of them we could not apprehend if they were explained to us, we have not the terms or ideas to understand them. Meanwhile we grope along as best we can, and do our daily work with a keen expectation of the future; and he is wisest who denies least of the mystery which surrounds us and the possibilities ahead. To assert, requires knowledge; to deny, requires much more knowledge. Let us be satisfied with positive knowledge so far as it has been vouchsafed to us, and leave negations to the self-sufficing and the omniscient. We can deny the self-contradictory and the absurd, but in the unknown and the mysterious, denials have no legitimate place: our business is carefully and cautiously to ascertain what is. We are surrounded by infinity, infinities of various kinds; and the wealth of existence is such as to justify a faith in our highest conceptions, a hope in the possibilities which lie before us, and a charity which enables us to do our daily work and to love our fellow men."

GREENWICH weather observations, which give approximately the average conditions over England, show that 1924 was generally wet and unseasonable. The winter months were mostly mild, while the summer months were mostly cool, and all months were wet with the exception of February, March, and August. The mean temperature for the year 1924 was 50°·6 F., which is 0°·5 F. in excess of the normal. The warmest month was July, with the mean temperature 63°, which is 0°·5 below the normal; the coldest month was February, with the mean temperature 37°·3, being 2°·5 below the normal. December was 3°·5 warmer than the average, and there were only three nights, December 9-11, with frost in the season. June, July, and August were the only months with a temperature of 80° or above, the highest reading being 89° on July 12. The lowest temperature during the year was 21° on February 15. Rain fell on 168 days, yielding a total of 31 inches, which is 7·5 inches more than the average for thirty-five years. The wettest month was July with 4·20 inches, which is 2 inches more than the normal. The driest month was February with 0·65 inch, and this was followed closely by March with 0·69 inch, the deficiency for the two months combined being 2 inches. 1924 is the wettest year since 1903, when the annual measurement was 35·54 inches. In the dry year of 1921 the total rainfall at Greenwich was only 12·50 inches; in 1923, a normal year, the measured rain was 23·86 inches. Sunshine was generally deficient, the only months with an excess of sunshine being January, March, July, and December. Records of temperature and rainfall for London now extend over about the last 200 years, but careful examination of these fails to give any cycle or periodicity which can help in the prognostication of the weather for a coming year or season; 1925 has started with exceptionally wet and boisterous conditions.

ONE of the recommendations made last year by the Departmental Committee on the Fertilisers and Feeding Stuffs Act, 1906 [Cmd. 2125], was the appointment of a committee to consider the articles to which revised legislation should apply. This advisory committee, which has power to co-opt, has now been constituted as follows: Lord Clinton (chairman), Mr. E. G. Haygarth Brown, Dr. Charles Crowther, Mr. J. Garton, Mr. C. W. Higgs, Mr. Arthur Holgate, Mr. Thomas Kyle, Mr. Alexander Main, Lieut.-Col. R. L. Norrington, Mr. J. W. Pearson, Mr. R. R. Robbins, Sir E. J. Russell, Mr. John Speir, Mr. George Stubbs, Dr. J. F. Tocher, Prof. T. B. Wood, and Mr. H. J. Johns, of the Ministry of Agriculture and Fisheries, 10 Whitehall Place, S.W.1 (secretary). According to the terms of reference, the committee is to draw up schedules prescribing the fertilisers and feeding stuffs to which the proposed legislation on the lines of the Report of the Departmental Committee on the Fertilisers and Feeding Stuffs Act, 1906, should apply, and methods of defining and stating the constituents and the "worthless" and "deleterious" commodities are to be considered.

AN interesting new departure in tourist travel is the motor tour across the western Sahara to Timbuctoo. Citroën Cars, Limited, the organisers of the tour, have sent us an itinerary of the route. The cars leave Colomb-Bechar, the railway head of the Algerian railway, and pass by Igli and Beni-Abbes to Adrar, the centre of the Tuat region. Thence they pass by Taouriot and strike almost due south, reaching the Niger at Burem. From Burem the traveller can continue in cars to Gao or Niamey, with the alternative of going by motor boat to Kabara and car to Timbuctoo. From Colomb-Bechar to Timbuctoo by this route is 1700 miles, and the time occupied is eight days. Timbuctoo is thus brought within twelve days of London, and there is a trans-Sahara service twice weekly. Modern hotels have been constructed at Colomb-Bechar, Beni-Abbes, Adrar and Timbuctoo, while at other stopping-places the company maintains camps. It is of interest to note that this motor service to Timbuctoo has been instituted within a year of the centenary of the first European traveller entering what was then the mysterious city.

MR. MURRAY MACGREGOR, district geologist, has been appointed assistant director of the Geological Survey in Scotland in succession to Dr. Walcot Gibson.

IN connexion with the Liverpool Section of the Society of Chemical Industry, Sir Max Muspratt, Bart., will deliver a Hurter Memorial lecture in the chemistry lecture theatre of the University of Liverpool on Friday, January 16, at 8 o'clock, on "Chemistry and Civilisation."

PROF. G. T. MORGAN, professor of chemistry in the University of Birmingham, has been appointed Superintendent of the new Chemical Research Laboratory of the Department of Scientific and Industrial Research at Teddington. Prof. Morgan was awarded the Research Medal of the Worshipful Company of Dyers in 1922 for his work on the co-ordination theory of valency in relation to adjective dyeing; and he is the author of numerous original papers in various branches of chemistry published by the Chemical Society, Society of Chemical Industry, and other societies.

APPLICATIONS are invited for some junior assistantships at the National Physical Laboratory, Teddington. Candidates must possess a good honours degree or equivalent qualification in physics, engineering or electrical engineering, and preferably with some experience in research. Application forms can be obtained from the director of the laboratory. They must be returned to him by, at latest, January 17.

ACCORDING to the New York correspondent of the *Times*, the trustees of the Metropolitan Museum of Art have announced the gift to that institution by Mr. John D. Rockefeller, junior, of 16,000 shares in the Standard Oil Company of California. These shares are worth approximately 200,000*l*. The gift is made unconditionally, but the donor suggests in his letter to the trustees that it should be added to the endowment fund.

ON Tuesday next, January 13, at a quarter past five, Prof. A. Fowler will begin a course of two lectures at the Royal Institution on the analysis of spectra, and on Thursday, at the same hour, Mr. Julian Huxley will deliver the first of two lectures on the courtship of animals and its biological bearings. The Friday evening discourse on January 16 at 9 o'clock will be delivered by Sir William Bragg on the investigation of the properties of thin films by means of X-rays, and on January 23 by Dr. A. W. Crossley, on science and the cotton industry.

The third meeting of the Society for Experimental Biology was held at Cambridge on December 19 and 20, the different sessions being held in the Schools of Zoology and Physiology. Members were entertained at lunch in Caius College by Prof. J. Stanley Gardiner, and a dinner was held in Christ's College. The programme included a paper by Prof. J. Barcroft on "Hæmoglobin as an Example of the Evolution of a Chemical Mechanism," a discussion by Dr. H. H. Dale on "The Nature of the Active Substances in the Posterior Lobe of the Pituitary Gland," and a symposium on "The Rôle of Electrolytes in the Organism," in which Messrs. A. J. Clark, J. Gray and J. B. S. Haldane took part. A number of other papers of interest were presented. Fifteen new members were elected.

Sir Napier Shaw has published privately a "Kalendar for 1925" arranged in weeks, showing the seasons and the international days for observation of the upper air. This is followed by a detailed list of the daily observations of solar and terrestrial radiation made in England during 1924. For each day of the year there is shown the sun's declination, the measurement of solar intensity at Kew Observatory between 11h. 30m. and 12h. 30m., the observations at South Kensington and Rothamsted of the maximum intensity of radiation from sun and sky and of the total radiation during the day upon a horizontal surface, and measurements of incoming and outgoing long-wave radiation made at Benson on cloudless evenings. These observations are given in weeks with the unusual but convenient arrangement of giving on each page two weeks which are separated by an interval of six months. The addition and subtraction of observations separated by six months gives the even and odd harmonics of the radiation curve separately. The Kalendar thus contains in readily accessible form much information of great value to meteorologists and others interested in solar and terrestrial radiation.

Two books of ethnological interest are announced for publication by Messrs. Seeley, Service and Co., Ltd., namely, "The Menace of Colour," by Prof. J. W. Gregory, dealing with many of the interracial problems of the day, and pointing out the dangers of the rising tide of colour and how they may be met or avoided, and "Vanishing Tribes of Kenya," by Major Orde Browne, Senior Commissioner of Tanganyika, a record of the habits and customs of the tribes inhabiting the slopes of Mount Kenya.

Our Astronomical Column.

The Absorption of Light in Open Star Clusters.—Dr. P. ten Bruggencate, in the *Zeitschrift für Physik*, October 31, describes an investigation of the colour indices of stars of the open clusters N.G.C. 1647, of Præsepe and of the Hyades, and deduces that these clusters consist almost entirely of dwarf stars, as is to be expected on the assumption that they have developed from globular clusters and are of great age. Colour-brightness diagrams were prepared, in which the catalogued stars were plotted, and, with certain assumptions as to the value of the parallax, graphs corresponding to the stars of the general stellar system, as determined at the Mount Wilson Observatory, and described by Seares, were drawn on the same diagrams. It was found that in neither case did the stars of the cluster agree with the graph. This was also true of the colour-brightness diagram of the Pleiades, which has already been described by the author.

In the case of N.G.C. 1647, an analysis of the diagram leads to the conclusion that the abnormal distribution of the stars in it is due partly to the assumed parallax being too large, and partly to absorption due to extensions of the dark nebulosity in Taurus, which lies between the cluster and the earth. In the case of the two other clusters and of the Pleiades, there is general absorption due to nebulosity inside the clusters. The Pleiades form a younger cluster than the others, which contains a number of A and F stars, and the nebulosity in it is bright and connected with the bright stars, being formed from material recently given off by these stars, which are in an unstable state of development. In Præsepe and the Hyades the clusters are older, and the internal nebulosity has become dark.

No nebulosity is found in globular clusters which are relatively young, the stars not having reached the stage of development mentioned above; the first sign of instability in these clusters is the occurrence of variable stars. These and the super-giants develop to O type stars and planetary nebulæ, and it is to be expected that the remains of these nebulæ will be found in old star groups or open clusters.

Observations of Algol Variables.—An important paper by Col. E. E. Markwick (B.A.A. Journal, vol. 35, No. 2) contains discussions and light curves of six Algol variables from observations by himself and other members of the Variable Star Section. They are a good example of the useful results that can be obtained in this field by careful and long-continued visual estimations of magnitude. The curves for three of the stars show secondary minima: U Ophiuchi, loss of light at secondary minimum 0·2 mag., RW Tauri 0·2 mag., Z Vulpeculæ 0·1 mag. They did not succeed in detecting the secondary minimum of Algol, but the curve of principal minimum is shown in great detail. The total number of observations used is 2630: they begin in 1906 for most of the stars, 1899 for Y Cygni. The period found for Algol is 2·867265 days, which is 3·9 sec. shorter than that given by Chandler. The range of the observations used is 5336 days or 14·6 years. The commencement and end of the principal eclipse are more rounded than those on Stebbins's curve. The effect is to make the total duration of eclipse 14·09 hours, which is longer than is generally given, but Col. Markwick defends his curve on theoretical grounds.

Research Items.

HUMAN REMAINS FROM ANCIENT GOLD MINES IN RHODESIA.—In view of the conflicting interpretations of archæological evidence in Rhodesia, any human remains to which any degree of antiquity can be attributed are likely to be of importance for the early racial history of that area. Considerable interest therefore attaches to a report by Sir Arthur Keith on two skeletons from ancient gold mines which appears in Vol. xxiii. of the Proceedings of the Rhodesian Scientific Association. The Que-que skeleton was found 4½ ft. below the surface in a filled-in working. It is represented by numerous fragments, all weathered, partly dissolved and fallen into pieces. They are only lightly mineralised, yet have the appearance of having been buried for a considerable time, possibly a thousand years or more. Such characters as can be recognised indicate a young female of about 18 years of age, of an estimated height of 5 ft. 1 in., and belonging to the negro race. The second specimen, from Belingwe, is a pure negro type, male, between 20 and 30 years of age. The skull is small, length 182 mm., breadth 124 mm., index 68, cranial capacity 1220 cc. A remarkable feature is the projection of the alveolar bone 9 mm. beyond the nasal spine and its wide simian nasal grooves. The skull is mineralised to a certain extent, and somewhat more ancient than the Gwanda woman (described in the Proceedings of the Rhodesian Scientific Association, Vol. xxi.) whose stature has now been calculated as 4 ft. 9 in. These measurements are comparable with those of two skeletons of ancient man from Rhodesia described by Dr. F. C. Shrubsall in *Man* in 1909. Thus all the remains we have from ancient ruins or mines in Rhodesia are of the Negro or Bantu type, and show no trace of Arab, Egyptian, Bushman, or Hottentot strain.

PALÆOLITHIC INDUSTRY IN NORTHERN CHINA.—T. de Chadin and F. Licent (Bull. Geol. Soc. China, iii. 1924, p. 45) record the discovery of palæolithic floors at three different places in Inner Mongolia. The floors are found at the base of the Loess and in the Loess itself, and the associated mammals include rhinoceros, hyena, gazelle, antelope, horse, bison, elephas, etc. The implements are made of quartzite, psammite and silicified limestone, and appear to be of Mousterian or early Aurignacian type. In the same regions in which palæolithic implements are found in the Loess, evidence of neolithic man is shown by the presence on the surface of the earth of polished axes, arrow-heads, knives and borers.

VITALITY OF JELLY-FISH.—It is a remarkable fact that many of the lower animals are able to live for a long time without food, maintaining themselves during the period of starvation at the expense of their own tissues and not merely by means of reserve stores of fat or other substances. A very interesting case of this kind is described by Messrs. de Beer and Huxley in the *Quarterly Journal of Microscopical Science* (vol. 68, part 3). They found that the common jelly-fish, *Aurelia aurita*, can be kept alive without food in laboratory aquaria for as much as thirty-eight days, during which time they undergo a progressive decrease in size, accompanied by loss of morphological and histological differentiation. The bell begins to shrink first, the oral arms later. Tentacles and thread-cells disappear and the gastro-vascular cavity closes up, the final result being a very small, shapeless mass. The bell continues its pulsations until an advanced stage of the process has been reached. It will come as a surprise to many naturalists that such a delicate organism as Aurelia can remain alive for so long under such unfavourable conditions.

THE BORING MECHANISM OF TEREDO.—The manner in which the ship-worm bores into timber has been a matter of conjecture and dispute among naturalists for at least two centuries. Some have supposed the soft fleshy foot to be capable of rubbing away the fibres of the wood, perhaps with the help of a solvent or softening secretion; others have regarded the valves of the shell, with their file-like rows of teeth, as the instruments of boring. It has been reserved for Mr. R. C. Miller (Univ. California Publ. Zool., xxvi., No. 4, pp. 41-80, 4 pls.) to give what appears to be a conclusive answer to the question. After a detailed account of the structure of the shell, the foot and the muscles connected with them, he discusses the possible methods of boring that have been suggested. He points out that the foot is covered with columnar epithelium, becoming glandular and ciliated near the edges and obviously unfitted for abrasive action. The possibility that some solvent enzyme may be secreted is not altogether excluded, although an analysis of shavings from the inside of the burrow showed no significant difference in composition from sound portions of the same block. The presence of "tool-marks"—scratches corresponding with the serrations of the shell—on the inside of the burrow, which has often been denied, is admirably demonstrated by a series of photographs. Finally, by laying bare the inner end of the burrow and sealing a cover-glass over the opening, the author succeeded in watching the Teredo at work. It was found that the movements of the animal in the burrow were effected chiefly by means of the suctorial and surprisingly mobile foot. Boring was seen to be accomplished by rhythmical movements of the valves of the shell, which were "held in position by the combined action of the foot attached to one wall of the burrow and the dorsal fold of the mantle pushing against the opposite wall."

THE CHANGING COLOUR OF THE MINNOW.—The minnow (*Leuciscus phoxinus* sive *Phoxinus lævis*) is one of those species of fish in which the male in the breeding season assumes brighter colours. Mr. Leo Abolin's communications (Beeinflussung des Fischfarbwechsels durch Chemikalien, Pt. I. Infundin- und Adrenalinwirkung der Melano- und Xanthophoren der Elritze; Pt. II. Annahme mannlicher Erythrophorenfarbung durch das infundinisierte Weibchen der Elritze, Nos. 119 and 120 *Mitteilungen aus der Biologischen Versuchsanstalt in Wien, Zool. Abt.*, under the direction of H. Przibram) give a most interesting explanation of the mechanism by which this is brought about. From the first paper we learn that the colour of the fish is mainly due to black and yellow pigments contained in cells termed melanophores and xanthophores respectively. These cells are situated in two layers of the skin, a deeper and a more superficial. Injections of minute doses of weak solutions of adrenalin contract the melanophores and cause the fish to assume a pale yellow colour; the effect passes off in about two hours. Injections of similar doses of post-pituitary extract cause expansion of the melanophores of the under layer and of all the xanthophores. The grey colour of the fish becomes greenish and the belly, which is normally silver, becomes golden yellow. If the fish is blinded the injection causes intense expansion of the melanophores in the sensitive regions of the body (the lips, gill-covers and the sub-branchial region of the head), but the xanthophores do not expand. The same result is obtained if the sympathetic system is destroyed; only in this case both layers of melanophores are expanded and the xanthophores are contracted; in fish which lie on a dark surface the same result follows under normal conditions. From the second paper it appears that in the superficial layer of the skin on the lips, the bases of the fins, and on the belly, there is contained a certain amount of red pigment embedded in cells termed erythrophores. When the male becomes ripe, these erythrophores become widely expanded, as do the melanophores and xanthophores. This same result can be obtained in ripe females and in small unripe males and females in which no trace of red is externally visible, by the injection of somewhat strong doses of post-pituitary extract (one-tenth per cent.). It is therefore obvious that the bodies of males and females, so far as pigment is concerned, have the same structure, and that the secondary sexual characters of the male are due to the action of the distinctively male hormone on this common groundwork.

YEASTS, FATS, AND ALCOHOL FROM SEAWEED.—We have recently received from Prof. Nadson, of the Principal Botanical Garden, Leningrad, a short typewritten communication from which it appears that the possibility of the commercial utilisation of seaweeds is at the present moment occupying the attention of Russian chemists. The paper in question is entitled "Seaweeds as a Source of obtaining Yeast, Fat and Alcohol," and contains a brief summary of the results obtained by Prof. Nadson working in collaboration with Messrs. A. G. Konokotina and G. K. Burgvitz. The authors claim to have succeeded in growing both bakers' and brewers' yeasts (of the type of *Saccharomyces cerevisiæ* I., Saatz, Frohberg, etc.) upon autoclave extracts of *Laminaria saccharina*, and from the results obtained they conclude that it should be profitable to produce compressed bakers' yeast and dry "Nährhefe" of high protein content in this way. They have, moreover, apparently succeeded in producing an abundant growth of "fat" yeast, *Endomyces vernalis* Ludw., upon minced and boiled *Laminaria saccharina*; cultures kept at 6-8° C. for sixteen days are stated to have produced a yield of 6·22 per cent. of fat upon the medium as compared with a normal content of only 0·3 to 1 per cent. of fat in the untreated weed. On the strength of these results, the authors recommend the use of seaweed as a substrate for the production of fat for technical purposes. The authors further state that they have produced alcohol from a decoction of *Laminaria saccharina* prepared by soaking 5 per cent. of finely ground material in water for 12-14 hours and sterilising at 110°; on this they have grown three different kinds of yeast: (1) Strains of *Saccharomyces ellipsoideus*, Saatz and Frohberg; (2) yeasts isolated from fermenting sugar-beets—*Saccharomyces betæ*; and (3) torula isolated from the surface of the living *Laminaria saccharina* of Murman. No data are given as to the yields of alcohol obtained, but it is claimed that the distillation of alcohol from algæ gives several valuable by-products. The information supplied in this communication is scarcely sufficient to carry very much conviction. The object of the publication may be best stated by quoting the authors' own words: "We are giving the results obtained by us in order once more to direct attention to the seaweeds of Russia as one of her natural riches awaiting their utilisation."

CRETACEOUS FAUNA AND FLORA OF SHANTUNG.—An extensive series of freshwater deposits formed in a continental basin in Shantung is described by H. C. Tan (Bull. Geol. Survey of China, No. 5, pt. 2, 1923), and is believed to be mainly of Cretaceous age. Numerous fossils have been found in the deposits, and furnish interesting evidence of the plants and animals which, during a long period of time, lived on land or in fresh water. They include dinosaurs, fishes, freshwater molluscs such as Unio, Leptesthes, Cyrena, Bithinia and Valvata; insects belonging to the orders Orthoptera, Blattoidea, Coleoptera, Odonata, Lepidoptera, Diptera, etc. The plants are mainly conifers and cycads. Descriptions and figures of some of the molluscs and insects are given by A. W. Grabau.

SOMERSET OIL-SHALES.—Messrs. H. G. Shatwell, A. W. Nash, and J. I. Graham recently communicated an account of the Somersetshire oil-shales to the Institution of Petroleum Technologists. These shales outcrop for about ten miles along the coast in the vicinity of Blue Anchor, and extend inland for a distance of one to three miles. They form part of an argillaceous and calcareous group of sediments which have been assigned to Lower Lias and Rhætic horizons, and their mode of occurrence is in two basins lying on either side of the Devonian Quantock Hills, the larger basin (Lilstock Basin) being situated on the east, the smaller (Doniford Basin) on the west. Bituminous shales of Lower Lias age have long been known from the opposite South Wales coast, and the authors conclude that the Somerset shales are a continuation of these. The Somerset shales occur in beds varying from 1 to 20 feet in thickness, those of South Wales being much thinner, 6 inches to 5 feet. Proximate analyses of the Somerset shales show that they have an average specific gravity of 2·4, from 70 to 73 per cent. of ash, 3·5 per cent. of moisture, 23 to 28 per cent. of volatile matter (less water), 1·70 to 4·52 per cent. of sulphur, and 13 to 17 per cent. of carbon dioxide. The specific gravity is higher than the Scottish oil-shale, which averages 2·0, while the high carbonate content is unusual. Results of assays and steam distillations carried out by the authors show that the yield of oil from one ton of shale is about half that of the average yield from the same quantity of Scottish shale; the total amount obtained from the Somerset shales varied from 5·00 gallons to 10·4 gallons, ammonium sulphate from 4 to 6 lb., and gas from 1200 to 1900 cubic feet per ton. The crude shale oil has an average specific gravity of 0·939 and sulphur content up to 3·12 per cent.; it is dark brown in colour and has a less sweet odour than that distilled from Scottish shale. On fractionation, naphtha (specific gravity 0·807 to 0·852), kerosene (0·888 to 0·925), and heavy distillate (0·973) are obtained; distillate above 300° C. failed to yield more than a trace of paraffin wax. From the above details it will be gathered that the Somerset oil-shales, notwithstanding the comparatively low sulphur content of the oil obtained from them, do not inspire confidence from a commercial point of view.

RAINFALL IN MYSORE.—The Meteorological Department of the Mysore Government has issued a Report on Rainfall Registration for 1923, prepared under the supervision of Mr. C. Seshachar, the Meteorological Reporter. There were 226 stations gauging rainfall during the year. The greatest rainfall on any one day was 17·90 inches at Agumbi in the Shamoga District on August 7; the heaviest record in 1922 was 22·16 inches at the same station. In the Kadur District the heaviest fall in 24 hours was 16·85 inches at Byrapur Estate on July 24, and in the Hassan District, at Marnhalli toll-gate, the fall in 24 hours on August 14 was 11·43 inches. In no other District did the fall in 24 hours amount to 5 inches. July was the wettest month on record since 1893, the year in which the

Meteorological Department was organised; the monsoon was unusually active in the western parts of the State. A monthly total of 202 inches was recorded at Byrapur Coffee Estate against a normal of 106 inches. The south-west monsoon period, June to September, was generally wet, the total rain over the State being 26 per cent. above the normal. The north-east monsoon period, October to December, was the driest on record since 1893. The deficiency varied from 66 per cent. in the Mysore District to 89 per cent. in the Chitaldrug District. The seasonal aggregate for the State was 1·95 inches against a normal of 8·04 inches, which is 76 per cent. in defect of the normal. The largest annual total for a single station was 393·64 inches at Agumbi in the Shamoga District, and the smallest was 4·36 inches at Dharmapur in the Chitaldrug District. Rainfall maps are given, which help much to a complete understanding of the work.

A SHUNTED GRASSOT FLUXMETER.—A Grassot fluxmeter may be described as an over-damped moving-coil ballistic galvanometer having practically no controlling force. This instrument is of great value for measuring magnetic flux. Gisbert Kapp, in his book on "The Principles of Electrical Engineering," suggests the possibility of increasing the range of the instrument by using a shunt, but he gives neither the theory of how it would work nor the formulæ that would have to be used with it. In April 1924 Masamiti Sase read a paper to the Physico-Mathematical Society of Japan in which he gives the complete theory of the multiplier, and gives experimental results confirming it. In the instrument he used, the highest permissible value of the shunt was about 8 ohms, which was about one-third of the resistance of the fluxmeter. He shows that it is desirable to use a shunt of manganin wire having the highest allowable resistance. His results prove conclusively that the particular Grassot fluxmeter with which he experimented could be used with a shunt of 8 ohms even when the highest accuracy is required. This greatly increases the range of the instrument.

THE EVIDENCE OF THE PROOF PLANE.—Mr. J. Clark, writing from Kewanna, Manitoba, directs attention to a method of dealing with observations made with the proof plane. When such a plane, without electrical charge, is introduced into the electrostatic field surrounding one or more conductors, one at least of which is charged, it will have, at each point in space, a definite potential V_p, depending on the external charges and the equal charges of opposite signs induced upon it. This potential Mr. Clark calls the potential of the free charge of the proof plane. If, then, the plane is brought up to a large insulated conductor, the total charge of which is zero, without actually making contact with it, the potential V_p will, in general, differ from the uniform potential V_c of the conductor, being greater than V_c on one side of the conductor and less on the other. If now the plane is made to touch the conductor, electricity will flow from the plane to the conductor in the first position, and from the conductor to the plane in the second, so that when the plane is removed from the field, and brought to an electroscope, it will be found to have a negative charge in the first case and a positive charge in the second. It is not always possible to regard the proof plane as temporarily forming a part of the surface which is being tested, since this is often curved, and the method suggested will often prove valuable.

A NEW DIAMAGNETIC PHENOMENON IN GASES.—An investigation of the diamagnetism of hydrogen, nitrogen and carbon dioxide, at different pressures, is described by Dr. A. Glaser in the *Annalen der Physik* for October. A rod of diamagnetic lead glass was suspended inside the experimental tube containing the gas, by a 2-5μ quartz thread attached to a torsion head. Thin threads of paramagnetic cobalt glass were melted on to the rod, so that the whole became slightly paramagnetic. The tube was surrounded by a water jacket by means of which the temperature was kept constant to within 0·01° C. In each observation the torsion head was twisted to bring the light spot to zero. As the pressure was diminished the susceptibility at first diminished in proportion; but in each of the gases this ceased to be the case at a pressure which depended on the nature of the gas and the intensity of the field. Thereafter the rate of diminution of susceptibility with pressure diminished greatly for some time, and in the end, at low pressures, the susceptibility was three times as great as it would have been if the original rate of diminution had been maintained throughout. It is suggested that, at low pressures, the distance between the molecules is such that there is time between collisions for them to become oriented with respect to the field. At higher pressures the collisions are constantly destroying any tendency to orientation which may be produced, so that all the molecules are practically unoriented.

A NEW LEAD ACCUMULATOR.—In the *C.R. Acad. Sci. Paris*, November 24, M. C. Féry describes a lead accumulator which does not sulphate, even if discharged and left uncharged for two years, at the end of which time it can be recharged in the ordinary manner. The author has previously shown that the true reaction taking place in the Planté cell is given by the reversible equation,

$$Pb_2 + H_2SO_4 + Pb_2O_5 = Pb_2SO_4 + 2PbO_2 + H_2O,$$

and that $PbSO_4$ is only formed accidentally when the cell becomes "sulphated." Pb_2O_5 is a black peroxide, and Pb_2SO_4 is greyish black in colour. The spontaneous discharge of the Planté cell is due to the combined action of the electrolyte and of oxygen on the negative plate, according to the equation

$$Pb_2 + H_2SO_4 + O = Pb_2SO_4 + H_2O,$$

and this action may develop into

$$Pb_2SO_4 + H_2SO_4 + O = 2PbSO_4 + H_2O$$

if the cell is left undischarged too long. To avoid this, the author places the negative plate at the bottom of a deep glass jar, and separates it from the positive, and from the air, by a porous material containing the electrolyte. A figure in the original paper shows the cell as it appears when partly discharged, with a layer of black plumbous sulphate on the upper surface of the negative plate. A cell left without recharge for twenty-six months only lost 66 per cent. of its original charge, the loss in the first month being 4 per cent., 83 per cent. of the original charge remaining after four months, while an ordinary cell would have lost the whole of its charge in this time.

ERRATUM.—In NATURE of December 20, p. 909, in a paragraph on the "Cytology of Cotton," referring to the work of Mr. H. J. Denham, it is suggested that the reason for the appearance of Mr. Denham's papers in the Journal of the Textile Institute as well as in the *Annals of Botany* is the importance of the work to the cotton-growing industry of the British Empire. We learn that the work referred to was carried out at the laboratories of the British Cotton Industry Research Association, and permission to publish it in the *Annals of Botany* was given on the understanding that, in common with all papers from the Association's laboratories, it would be published in the Journal of the Textile Institute.

The Japanese Earthquake of 1923.

MR. K. SUDA, of the Imperial Marine Observatory at Kobe, has recently issued a detailed and interesting report on the great Japanese earthquake.[1] It is based on an examination of all the seismograms obtained at Japanese observatories and on an exploration of the central tract from September 10 to October 12. The following is an abstract, not of the whole memoir, but of those portions which have not been fully treated in earlier papers.[2]

It is not often that isochronal lines (the coseismal lines of Mallet) can be drawn for any earthquake. The large number of earthquake observatories in Japan and the radio time-service have enabled Mr. Suda to give a series of such lines, certainly the most accurate of any yet attempted. They are drawn for every ten seconds, except towards the north-east. From the epicentre to a distance of 200 km. the curves are extremely irregular. Beyond this distance they become portions of nearly concentric circles, and the distance between successive curves increases with the distance from the origin. The curves in the north-east of Japan are packed more closely together than in the south-west. The irregularity of the curves is due partly to geological conditions, partly to the form of the focus.

On the same map are shown the initial directions of the shock. These are not always directed towards or from the epicentre, small deviations being experienced at several places, due chiefly perhaps to the geological structure of the country. The initial motions are condensational (or from the origin) at nearly all the observatories to the south-west of the line from Matsumoto to Kofu. Between this line and that from Niigata to Fukushima (that is, in the region including the central zone) condensational and rarefactional movements are mixed together. To the north-east of the latter line the initial motions are again, with few exceptions, condensational. The complexity in the intermediate region may be connected with the nature of the displacement that caused the earthquake. The directions of initial motion are usually normal to the isochronic lines. Smoothing off the local variations in these lines, the space-time curve is drawn. It cuts the time-axis at a point corresponding to 11 h. 58 m. 26 s., the time of occurrence at the epicentre considered as a point. The velocity of the primary waves near the epicentre was 5·5 km. per sec., that of the large waves was constant and about 3·59 km. per sec.

One more result obtained from the study of the seismograms may be referred to. The angle of emergence at Kumagaya was 21°, and at Choshi 13·5°. At Mera the epicentral distance is 46 km., and the focal distance (as determined by the duration of the preliminary tremor) 70 km.; at Tokyo these distances are 82 km. and 98 km. The focal depths given by these four observations are about 50, 35, 54, and 55 km., the average being 48 km.

Mr. Suda adds some interesting details about the changes of level along the coast of Sagami Bay. Since the earthquake, the land has shown a tendency to return to its original level. For example, at Misaki (near the end of the Miura peninsula) the elevation on September 1 was about 760 cm. and the sea-bed between it and a small island in front was laid bare. On September 5 the land here began to sink, at first at the rate of nearly 60 cm. a day, then more slowly, until on September 26 the uplift was only about 140 cm. Similarly, at Tateyama, at the end of the Bo-so peninsula, the uplift on September 1 was 220 cm. Later in the same month it was reduced to 140 cm. The first upheaval and the subsequent sinking were both most marked at the ends of these peninsulas. There is also evidence that, at both points, the land had been slowly sinking for many years before the earthquake. At Shitaura, near the end of the Miura peninsula, the land had sunk about 2 metres during the preceding sixty years, and some of the farms near the shore had become covered by the sea. After the earthquake the land was restored to its former level. The remarkable changes of level in the floor of Sagami Bay have been referred to in NATURE (vol. 113, pp. 473-474). The first soundings were made soon after the earthquake. They have since been repeated with great care, and a comparison of the two maps reveals no great differences. The *tsunami*, or seismic sea waves, were comparatively low. The greatest height attained was 11 metres at Atami, where 155 houses were washed away and about 60 persons were killed. Mr. Suda attributes the small height of the waves in part to the upheaval of the land round Sagami Bay.

One of the most interesting chapters in the memoir is that on after-shocks. Omori had found empirically that the decline in frequency of after-shocks follows a hyperbolic law. The theoretical investigation by Prof. S. Kusakabe shows that the decline takes place according to a logarithmic law, of which Omori's law is a particular case, and that the frequency-curve approaches or deviates from the hyperbolic form according as the time during which the stresses causing the earthquake act is long or short. As the curves for different observatories are nearly hyperbolic, it follows that the great earthquake was tectonic, and not volcanic, in origin. Mr. Suda, however, points out a remarkable feature in all the frequency-curves, whatever the distance from the epicentre. From the first to the third days the curve is hyperbolic; and, after the fourth day, again hyperbolic but with a lessened value of the constant. For example, at Kumagaya, the frequency-curve is at first represented by the equation $xy=327$, but after the fourth day by the equation $xy=242$. Mr. Suda infers that the stresses that caused the earthquake began to operate again after three or four days, and that, in consequence, the frequency of after-shocks was distinctly less after this time. He suggests that these districts will probably be visited periodically by great earthquakes until the stresses causing them are changed.

The after-shocks are divided by Mr. Suda into two classes, which he calls the Sagami-Sakawa family and the Bo-so family, the former occurring in an area to the west of a north-south line passing just to the east of Oshima, and the latter in an area on the east side. After-shocks of the Bo-so family were usually stronger than the others and had a longer vibration-period. The greatest of the Bo-so series was that on September 2 at 11 h. 47 m., which was nearly as strong as the great earthquake. The epicentre lay off Katsuura, on the Pacific side of the Bo-so peninsula, and the first displacement within the focal region occurred in a vertical plane, for the initial motion at every observatory near the epicentre was directed outwards. Mr. Suda points out an interesting relation between the frequency of the after-shocks generally and the atmospheric pressure, the rate of pressure-change and the pressure gradient. The frequency is slight or great according as these conditions are the same as, or contrary to, those which prevailed on the day of the earthquake.

The theoretical portions of Mr. Suda's memoir, interesting as they are, must be passed over more

[1] Mem. Imp. Marine Obs., Kobe, Japan, vol. 1, 1924, pp. 137-239, tables 1-49, and 46 plates.
[2] NATURE, vol. 113, pp. 254, 473-474; vol. 114, pp. 70, 291, 484.

rapidly. He considers that there were two epicentral zones, one directed north-north-east in the central part of Sagami Bay, the other intersecting it and directed north-west. The former coincides with the great region of subsidence in the bay, and this subsidence produced the condensational initial motion around the epicentre. Beneath the second epicentral zone, he thinks, there was a horizontal sliding of a layer of the crust towards the south-west, which would produce condensational motion on the south-west side and rarefactional motion on the north-east side. The vertical displacement beneath the first zone being the more important, the condensational initial motion prevailed at some distance from the origin. Among the secondary causes of the earthquake Mr. Suda includes the typhoon which was passing over the northern part of the epicentral region at the instant when the earthquake occurred. C. D.

Italian Theses on Chemistry and Physiology.

THE twenty-sixth volume of reports, recently issued by the Cagnola Scientific Foundation, covers the period 1917–1923 and deals with the essays on various scientific subjects submitted for adjudication to committees appointed by the Royal Lombardy Institute of Science and Literature. The subjects vary widely in character, but are all of medical or chemical interest. In a few instances only have the theses presented been deemed worthy of reward, and of the successful theses, four only are printed *in extenso* in the present volume.

In "The Chemio-therapy of the Spirilli," which extends to over 160 pages and is accompanied by many plates, diagrams, etc., Prof. Giorgio Castelli gives, in addition to a succinct account of previous knowledge of this subject, the results of his own numerous experiments, which lead to various important conclusions. The slight increase in the efficacy of the curative agents determined by prolonged contact of the latter with the air during the manipulation is insufficient to compensate for the dangerous increase often observed in the toxicity. Oral administration of the arsenobenzenes is not to be recommended, and the use of dimethylamino-tetraminoarsenobenzene, proposed by Giemsa, is not only less effective than that of salvarsan or neo-salvarsan, but is also followed by more frequent and more ready relapse. Increased toxicity is also against the employment of compounds obtained by the introduction of amino and other nitrogenous groupings, sulphur, etc., into the molecule of arseno-benzene. In some cases augmented therapeutic efficacy is attained with compounds of salvarsan with silver or copper salts, but here too the frequency of undesirable after-effects militates against the application of such compounds on a large scale.

Prof. Aldo Perroncito, in a short contribution on the derivation of blood platelets, demonstrates the possibility of the formation of new platelets independently of the elements of all other tissues of the organism. In blood drawn from the jugular vein of the cat or dog, and treated with sodium citrate to render it non-coagulable, and then with pyrodin solution, the number of the platelets becomes doubled or, in some instances, even quintupled after the blood has been kept at 38° C. for about an hour. Exclusion of the intervention of either the corpuscles or the plasma leads to the conclusion that the platelets are capable of reproducing other elements with similar morphological characters.

In his memoir entitled "Catalytic and Enzymic Phenomena," occupying 130 pages and including a very useful bibliography, Prof. Ugo Pratolongo develops a novel interpretation of recent results arising from modern mechanico-static theories of chemical phenomena. He has succeeded in distinguishing in chemical kinetic equations the terms depending on piezometric actions and on medial actions from terms proper to the mechanism of the reaction. The application of this principle to well-known accelerating reactions shows that some of these are definitely catalytic in nature, inasmuch as a change in the mechanism of the chemical reaction, and hence in the critical energy relative to the reaction, is involved; others are of medial character, and others again—heterogeneous catalyses—possibly depend on variations in the active concentration of the reagents induced by the catalyst. The experimental results show that, in the decomposition of hydrogen peroxide, the critical energy assumes values depending strictly on the metal functioning as catalyst and independent both of its method of preparation or state of subdivision and also of the presence of activating or inactivating agents. Thermal inactivation of a catalyst consists of partial destruction of the catalyst, the remaining fraction retaining its activity unaltered.

Under the title "Interchange of Hæmoglobin in Physiological and Pathological Conditions," Dr. Enrico Greppi discusses the different factors operative in the continual destruction (hæmolysis) and re-formation (hæmoglobinogenesis) of the blood-pigment.

Astronomical Observations at Greenwich and the Cape.

THE first impression on seeing the annual volume for 1921 of "Greenwich Observations"[1] is its thinness in comparison with the volumes of two or three decades ago. It appears to be the least bulky in the last hundred years. This is due to a reduction accomplished during Sir Frank Dyson's tenure of office by the suppression of much of the detail of the observations, but enough is still given to show the data on which the tabulated observations depend.

The results are given concisely and orderly, and they represent an immense amount of routine work which is carried on from year to year at Greenwich, a work never in the limelight but, on its astronomical side at least, the basis for the discussion of all the problems of fundamental astronomy.

The observations with the Transit Circle take up the first section, and show the state of adjustment of the instrument from day to day, and then give the observations of right ascension and declination of the sun, moon, and planets, together with the observed errors of the tabular place as given in the Nautical Almanac. More than 17,000 observations were made in connexion with the Transit Circle in the course of the year. The observations of the moon have been combined with the extra-meridian ones taken with the Altazimuth, and give the error both of the Nautical Almanac (Hansen's tables with Newcomb's corrections), and Delaunay (Connaissance des temps,

[1] Astronomical and Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1921.

Radau's tables). It is of interest to note that the latter corrections are much the smaller.

The observed errors of the wireless time signals received from Paris, Nauen, and Annapolis are shown in Table IV. (p. A 16). The range of error, from about 0·2 sec. early to 0·4 sec. late for each of the three observatories, is rather large when the installations for determining and keeping time at the observatories concerned are considered. The observations, however, have not been corrected for errors in the sending. It is not stated to which series of morning signals the Paris observations refer, and it would be of value to any one discussing these results to know whether the beginning, middle, or end of the signal was referred to the local clock beat.

The next section deals with the measures of the photographs of the sun taken at Greenwich, the Cape, Kodaikanal, and Dehra Dun. Thanks to this co-operation, photographs were available for each day of the year. There is a pronounced decline in the mean daily area of spots, corrected for foreshortening, from 1052 (1919), 618 (1920), to 420 (1921). Similar figures for the faculæ are 1729 (1919), 1219 (1920), to 739 (1921), the unit being the millionth of the sun's visible hemisphere. The last section gives the usual details concerning the magnetical and meteorological observations. These form about one-third of the whole volume. Plates show the magnetic disturbances for three consecutive days, May 13, 14, and 15.

In one respect the title, "Greenwich Observations, 1921," does the book less than justice. Except in the annual report at the end of the volume, there is, for example, no mention of the work done with the Thompson Equatorial, with which more than 1000 parallax plates were taken, and almost as many measured. There is no question that the national observatory at Greenwich continues to maintain its high standard of duty and performance, though one may require to read between the lines to prove it.

The Cape portion of the Astrographic Chart and Catalogue extends from declination $-40°$ to $-52°$. The volume of the catalogue now issued [2] is the eighth of this series to appear. The previous members, numbers i.-vii., give the measures in the seven zones from $-41°$ to $-47°$, the first appearing in 1913, and the last in 1923.

The present volume was delayed in printing, apparently by the War, for the introduction was signed by the late Mr. S. S. Hough in 1916. It contains the measures of the rectangular co-ordinates of all the stars shown on the plates having their centre on the declination circle $-50°$.

The introduction gives a short description of the telescope, the method of measurement of the plates, the process followed to guard against errors in the measuring, and the further checking of the results by the intercomparison of overlapping plates. It is evident that great care has been taken to eliminate errors.

The plates were exposed during the years 1902–1908 and measured with two exceptions between November 1907 and September 1908. The catalogue gives the measures of the x and y co-ordinates to 0·001, an estimation of the diameter of the image, together with the identification by number and magnitude of the star in the Cape Photographic Durchmusterung, if it occurs in that catalogue. No attempt is here made to convert these estimates of stellar diameter into a standard scale of magnitude.

The catalogue proper consists of 465 pages, with a total of 79,105 stars, or a distribution for the first 12 hours of R.A. of 32,504 and for the last 12 hours of 46,601.

[2] Catalogue of Rectangular Co-ordinates and Diameters of Star Images derived from Photographs taken at the Royal Observatory, Cape of Good Hope. Zone $-50°$.

Annual Meeting of the Geographical Association.

THE annual meeting of the Geographical Association was held at the London School of Economics on the first three days of January. The gathering was remarkable for an extraordinarily valuable series of papers of high scientific worth, for a broadcast message by Prof. J. L. Myers, the president, and for a continuation of the plan of inviting a distinguished continental thinker, in this case Dr. A. Sommerfelt, of Oslo, to speak.

In his presidential address, Prof. Myers dealt with the historical method in ethnology and with cogent argument and gentle banter criticised the conclusions of some investigators, especially Mr. Perry, who, applying the hypothesis of long distance diffusion of culture, announced as discoveries speculations which recalled extreme conclusions of Max Müller on the Aryan Race or those of the Anglo-Israelites on the lost tribes.

Among the other papers two stood out as remarkable. One was entitled on the programme "Life among the Hill Tribes of Algeria." It cannot be described satisfactorily either as a description illustrated by films or as films with appropriate comments, for the first of the noteworthy points just was that it was a coherent whole in the sense that the words and music of a good song are coherent. The kinema was in fact seen at its best as an educational instrument of the highest class. It is not easy to make it so, but that it can be done is evident. The second point is that what was done was worth doing. The films were thoroughly scientific, extremely human, entirely free from anything meretricious and yet extraordinarily interesting. They are the property of Mr. J. A. Haeseler, who is collecting a scientific library of such films, and they had been taken with the help of Capt. Hilton-Simpson, who gave the address. They showed the actual life of the people: basket-making, shoemaking (with platted grass), making of wooden door-locks with an adze strikingly like a bronze age tool, quern-making and corn grinding, ploughing, skinning a goat and the processes by which the skin is turned into a water-bottle, and bread-making; but perhaps the most fascinating of the pictures were those showing the fundamental arts of textile and pottery making. Spinning was exhibited as carried on by two or three of the simplest possible methods, one even without the aid of a spindle, while two types of simple looms, and the entire operation of weaving from the making of the warp and setting up of the loom to the final process of inserting the woof, was shown with wonderful clearness. So also clay was obtained from a pit, freed from stones, and a pot gradually built up before one's eyes without—and this is the remarkable fact—the aid of the potter's wheel. The ornamenting of the pot, the firing, the fire being lit by a boy, and the glazing of the still hot vessels were equally clearly shown.

The performance—it is difficult to find a satisfactory word for the combined appeal to eye and ear—gave an insight not only into the geography of another land, but also into conditions of a long past time very thinly veiled by later growths and accretions. We have said that the kinema was seen at its best. This was because it was used for what it is best fitted. It was used to show things as they happened, dynamically, not statically. We might perhaps qualify our statement and say "almost at its best." It would have been better, though impracticable, to show several of the films again. Even when one knew what to look for, it was quite impossible to follow all the details of what was done. Further, it was obvious that while for exhibiting processes the film is the superior, yet for still life and for analysis the slide has still the advantage.

The other paper was by Mr. C. E. P. Brooks and was entitled "The Climatic History of the Fiord Countries." Climatology is still in its infancy, and this is one of the few papers which deal with climatic, as distinct from meteorological, problems without becoming unintelligible amid a maze of statistics. Mr. Brooks traced out the succession of climates during and since the Ice Age, if indeed we are out of the Ice Age. He suggested that the Daun stadium should be dated round about 1800 B.C. rather than 5000 B.C., and estimated the temperature of the Norwegian coast at 5000 B.C. to have been 4° higher than at present. His most striking suggestion related to the causes of these climatic changes. He associated them with variations in the ice conditions in the Arctic Ocean. Analysis of these conditions by Kerner Marilaun and himself has shown that the Arctic floating ice-cap must either have its present extent or the whole ocean must be free of ice, no intermediate stage being stable. He related the voyages of the Norsemen, who in their voyages to the south of Greenland mention storms but never ice, and the great Asiatic migrations, to an open period in which the Arctic was unglaciated, and concluded, "In the twelfth century the glacial stage recurred and has apparently persisted to the present day." So we are still in the Ice Age!

Periodicals in Canadian Libraries.[1]

THE need for co-operative library catalogues of scientific periodicals has long been felt in many countries, and various projects have been undertaken to supply the deficiency. The work before us is an attempt to provide for those specially interested in scientific periodicals, and consists of a list of such material available in Canadian libraries, together with bibliographical information. Journals are arranged under their latest form of title, and publications of academies under the name of the society or institution. In our opinion, this method of listing the publications of academies does not facilitate their ready identification. Under the words "Kaiserlich" and "Königlich" there are quite a number of entries, although events of recent years have caused these adjectives to be dropped or replaced. As an example, the Königlich-Preussische Akademie der Wissenschaften is now known as the Preussische Akademie, though this fact cannot be ascertained from the present work. Had publications of this character been listed under the first word of their title—in this case Sitzungsberichte—consultation would have been simplified.

It is to be regretted that a number of Canadian libraries, whilst those responsible realised the importance of the work, were unable to include their possessions in this list. The reasons given were that periodicals were not catalogued or that the staff at their disposal was inadequate. It is obvious that full advantage cannot be taken of the resources of Canadian libraries if a proportion of them are compelled to neglect the preservation and cataloguing of serials. Further, Canadian science will be under a distinct handicap until steps are taken to provide the libraries with adequate competent assistance.

Despite the difficulties confronting them, the compilers have produced a work which should prove of considerable aid to scientific workers in the Dominion, and they are to be congratulated upon the completion of a volume which bears evidence of much care and painstaking labour. Due credit should also be given to the Canadian Department of Scientific and Industrial Research, the material co-operation of which enabled the early publication of the volume.

F. W. CLIFFORD.

[1] A Catalogue of Scientific Periodicals in Canadian Libraries. Prepared by Dr. Gerhard R. Lomer and Margaret S. Mackay. Pp. xx+255. (Montreal: McGill University, 1924.) n.p.

University and Educational Intelligence.

ABERDEEN.—The Fullerton Research Scholarship in natural science has been awarded to Miss Elizabeth T. Geddes.

GLASGOW.—The King has been pleased to approve the appointment of Mr. James Montagu Frank Drummond to the regius chair of botany in the University, vacant by the retirement of Prof. F. O. Bower. Mr. Drummond took first-class honours in the Natural Sciences Tripos at Cambridge in 1904, and gained the Frank Smart Studentship for research in botany at Gonville and Caius College. He became lecturer in botany at Armstrong College, Newcastle-upon-Tyne, and in 1909–1921 was lecturer in plant physiology in the botany department of the University of Glasgow. Since 1921 he has been Director of the Scottish Plant Research at Corstorphine, Edinburgh. During the War he served in Palestine, Egypt, and France, acting as battalion intelligence officer, and afterwards as brigade education officer. His published works refer chiefly to plant physiology, but include a series of papers on the "Botany of the Palestine Campaign" communicated to the Linnean Society.

LONDON.—The lectures which were to have been given early this month by the late Prof. J. I. Hunter at University College on "The Anatomy and Physiology of the Sympathetic Innervation of the Striated Muscle" will be delivered by Prof. G. Elliot Smith on January 19, 26, and February 2 at 5 o'clock.

The degree of Ph.D. in Science has been conferred on Mr. W. Jevons (Imperial College—Royal College of Science) for a thesis entitled "Spectroscopic Investigations in connexion with the Active Modification of Nitrogen" and other papers.

NOTICE is given by the Imperial College of Science and Technology, South Kensington, of the alteration in the date of the Entrance Scholarship Examination, which in 1925 will begin on April 24. Eighteen Scholarships, value 62*l*. 10*s*. each, are offered, six being tenable at the Royal College of Science, six at the Royal School of Mines, and six at the City and Guilds (Engineering) College, for admission at the beginning of the session, namely, the first Tuesday in October. Prospectuses and full particulars may be obtained on application to the Registrar, Imperial College, South Kensington, S.W.7.

APPLICATIONS are invited by Yale University for two Theresa Seessel Research Fellowships for the promotion of original research in biological studies, each yielding 300*l*. Preference will be given to candidates who have already obtained their doctorate, and have demonstrated by their work fitness to carry on successfully original research of a high order. The holder must reside in New Haven during the college year, ranging from October to June. Applications should be made to the Dean of the Graduate School, New Haven, Conn., U.S.A., before May 1 next, and should be accompanied by reprints of scientific publications, letters of recommendation, and a statement of the particular problem which the candidate expects to investigate.

THE Royal Technical College, Glasgow, directs attention in its report for 1923–24 to the part taken by it for many years in the further education of adults. The evening classes of last session were attended by 2587 adult students of ages ranging from twenty-one to seventy-five and, in addition, 546 enrolments were received for the "Elder" lectures on astronomy by Prof. George Forbes on "The Old Astronomy and the New," and by the Rev. E. Bruce Kirk on "Stars in their Associations." The College maintained also its press campaign for informing the public of the important part taken by chemistry in the life and industry of the country, and numerous papers of a popular character were published by members of the staff on such subjects as the utilisation of waste, chemistry and wireless, etc.

FROM the University of Leeds we have received a copy of the Council's annual report for 1923–24, which was issued on the eve of the Jubilee and "Coming of Age" celebrations of December 15-20. In it the Council announces, after summarising the serious and numerous present deficiencies in accommodation and equipment and in the provision for the social and athletic life of the University, that action is being taken to meet the most pressing requirements and to make the Jubilee year a starting-point for another period of progress. That they are able to do so is due in large measure to increased local aid. Grants from local authorities during the past year amounted to 30,000*l.*, not counting the fees, amounting to 4500*l.*, remitted to students as a condition of such grants. The other chief sources of revenue were: endowments, donations, and subscriptions, 21,000*l.*; parliamentary grants-in-aid, 58,000*l.*; and fees for tuition, examinations, graduation, Students' Union, etc., 60,000*l.*

THE December number of *The University Bulletin*, issued by the Association of University Teachers, contains an address by the president of the association, Prof. Alexander Mair, on the significance of this organisation as marking a distinct phase in the evolution of the university system in Great Britain. Among the many *éclaircissements* produced by the War were, Prof. Mair says, the revelation to English people that in the universities they had a *national* asset, and a clearer awareness on the part of the universities themselves of their function and destiny. A "get-together" spirit took the place of particularism, and one of the indications of this was the appearance of the Association of University Teachers. At present about 75 per cent. of all the teachers in the university institutions of England and Wales belong to its 25 branches, although "Oxford and Cambridge are still hesitant, and their absence makes the one (an important) gap in an otherwise continuous front." In referring to the spirit of co-operation as between universities and the recognition of their value to the nation as post-War phenomena, Prof. Mair seems to have overlooked the fact that the first Congress of Universities of the Empire took place, and the Universities Bureau of the British Empire was constituted in 1912.

Early Science at the Royal Society.

January 10, 1662/3. Mr Howard mentioned a way of roasting in a very short time, with basting the meat with flames of lard poured upon it: Dr. Wilkins, that of boiling and stewing meat with lamps: Mr. Hoskyns, that of roasting many pieces of meat with a fiery globe of plated iron standing in the middle: Sir Cyril Wyche, that of keeping water and other things warm in a double pot, separated by an interstice.

January 11, 1671/2. Mr. Isaac Newton was elected. [Newton had written earlier "I am very sensible of the honour done me by the Bishop of Sarum in proposing me candidate, and which I hope will be further conferred upon me by my election into the society, and, if so, I shall endeavour to testify my gratitude, by communicating what my poor and solitary endeavours can effect towards the promoting philosophical design."]

1664/5. Sir Robert Moray mentioned, that the King had made an experiment of cold, with three glasses filled with sweet water, used for washing, one glass bigger than the other, taken out of a trunk by the King's barber, and freezing, after they had a very little while been opened, first at the top, and then with shootings of ice to the bottom, and so congealing together.

January 13, 1663/4. The president acquainting the council, that Mr. Hooke had discovered to himself, Sir Robert Moray, and Dr. Wilkins, an invention, which might prove useful to England, and to the world, and that he had a good opinion thereof; but that it was necessary, that some experiments should be made for farther certainty, before it was made public which would require some charges not so fit to be put upon the inventor; it was ordered, that the President, Sir Robert Moray, and Dr. Wilkins have power to employ any sum under ten pounds of the society's money for the said purpose.

1669/70. Mr. Oldenburg produced a manuscript sent and addressed to the president by Mr. Flamstead of Derby, giving an account of some of the more notable celestial phenomena of the year 1670 to be conspicuous in the English horizon, among which was an eclipse of the sun visible in England, April 9, but omitted by all other astronomers. The society declared that this was a very useful labour for the improvements of astronomy; and that therefore the author should receive their thanks by the secretary.

January 15, 1661/2. Prince Rupert sent the society a description in High Dutch, of the method of making good gun-powder; which Mr. Oldenburg was desired to translate, and Sir Robert Moray to return their thanks to his highness.

1673/4. It being moved that Dr. Daniel Cox having made many observations and experiments concerning the nature and figures of all sorts of salts, might be desired to impart them to the society, he was desired accordingly, and promised, that he would do so, after he had viewed and examined such salts by such a microscope, as had been approved of for its goodness by the Society: and a microscope being brought by Mr. Cock to be examined, the trial of it was referred to a fitter time, it being then candle-light.

January 16, 1667/8. Mr. Oldenburg mentioned that he had received advice from Paris, that the person formerly said to have undertaken the translation of the "History of the Royal Society" into French, had not yet begun it, and was willing to forbear, upon notice sent him, that there was one in London, who would perform it. And Dr. de Molin being the person, who had undertaken that work in England, and now present, was desired by the Society to proceed in what he had begun with all possible care and diligence.

Societies and Academies.

LONDON.

Aristotelian Society, December 15.—L. J. **Russell**: Science and philosophy. Scientific investigation makes use of hypotheses, but something more is needed in the search for truth. The inquirer goes to Nature with demands which his own nature enjoins on him. Demands like hypotheses are anticipations; they say more than Nature tells, but they go beyond hypotheses, for they challenge Nature. Some demands are logical, some æsthetic, some metaphysical. Only legitimate demands can be satisfied, and we can only know what demands are legitimate by explicating the universe in detail by their help. There cannot be a view which is true in philosophy and in the end unworkable in science, or a view which is true in science and untrue in philosophy.

CAMBRIDGE.

Philosophical Society, December 8.—C. T. R. **Wilson** and G. I. **Taylor**: The bursting of soap-bubbles in a uniform electric field. Measurements were made of the electric field necessary to burst a soap-bubble attached to a metal plate. The half-bubble correctly represents half of a complete bubble immersed in a uniform electric field. The instability is shown by photographs to take the form of a thin thread of fluid which is drawn off from the top of the bubble. The product (electric force) × (square root of radius) is constant at the bursting point. The maximum diameter of water-drop which can exist without bursting in a uniform electric field of 30,000 volts per cm. is 0·4 mm.—E. A. **Milne**: Dissociative equilibrium in an external field of force. The conditions of thermodynamic equilibrium in a gravitational field of heterogeneous substances capable of undergoing reversible chemical reactions, which were formulated by Willard Gibbs, are extended to include the case in which the products or reactants are associated with electric charges and may be subject to an external electric field. Application is made to the high-temperature ionisation of gases in stellar atmospheres.—T. M. **Cherry**: (*a*) Some examples of trajectories defined by differential equations of a generalised dynamical type; (*b*) Integrals developable about a singular point of a Hamiltonian system of differential equations. Part II.—J. **Brill**: Note on the Lorentz group.—E. H. **Neville**: Note on the harmonic conic.—R. **Hargreaves**: Thermodynamics and quantum theory.

DUBLIN.

Royal Dublin Society, December 16.—H. H. **Jeffcott**: The theory of variation of flow in pipe lines with surge chambers consequent on variation of load on hydraulic turbines operated therefrom. The surge chamber, used with long pipe lines, acts as a supplementary reservoir near the turbines, so that the demand for increased discharge consequent on increase of load can be promptly met. This avoids the delay occasioned by the inertia of the great mass of water in the pipe, which requires some time for its acceleration, and also serves to relieve the pipes from water hammer consequent on the partial or complete closing of the turbine gates on reduction of load. The surge oscillations set up in the pipe line and surge chamber must not be allowed to persist for so long as to disturb the operation of the turbines unduly, and to this end a damping arrangement is often introduced. Equations are obtained for the motion of the water in the general case corresponding to any given law of opening or closing of the turbine gates, and approximate solutions are given.—Rev. H. C. **Browne**: The influence of the Fitzgerald contraction upon distance measurements and clock times. A real, absolutely existing Fitzgerald contraction underlies the relativity contractions indicated by the Lorentz formulæ, and the entire symmetry and equivalence of all systems in uniform motion of translation depend upon this real contraction resulting from the absolute undiscoverable movement of each system through space. The constancy of the observed velocity of light for each system, and the establishment of a consistent "timing" of events, common to all observers throughout a system, also depend upon this real contraction. Since all experimentally determined velocities, including that of light, result from some expression of the form $c'=l'/t'$, where l' is a fixed measured length and t' an observed interval of time, the value of c' will vary inversely with the value of t', but the product $c't'$ will have the fixed value l' independently of the rate of time movement. The same is true for $v't'$, and the ratio v'/c' will also be independent of the rate of time movement, provided that this latter is constant. In this way all expressions containing a problematic absolute value for c' and v' can be eliminated, and the concrete observed values $c't'$, $v't'$ can be substituted; *i.e.* velocities and times can be written down in the specialised form in which they must enter into any equation containing space co-ordinates. The form of the Lorentz transformations changes, but the mathematical contents remain identical.

PARIS.

Academy of Sciences, December 1.—Charles **Moureu**, Charles **Dufraisse**, and Marius **Badoche**: Auto-oxidation and antioxygen-action (XII.): Researches on the active auto-oxidisable form of acrolein.—Auguste **Bépal**: The fifth international conference of pure and applied chemistry, held at Copenhagen, June 16 to July 1, 1924.—Georges Claude was elected a member of the division of the applications of science to industry, in succession to the late H. de Chardonnet.—**Mordouhay-Boltovskoy**: The impossibility of an algebraical relation between π and e.—Bertrand **Gambier**: The polygons of Poncelet generalised.—Maurice **Gevrey**: The integration of the equation of dynamical tides.—E. **Huguenard**, A. **Magnan**, and A. **Planiol**: The measurement of the aerodynamical stresses supported by the wings of an aeroplane.—Jean **Boccardi**: Averaging by tenths of a year in variations of latitude. The usual method of taking the means of the values of the latitude, ϕ, by tenths of a year, has inconveniences and may lead to erroneous conclusions.—J. **Prédhumeau**: A new apparatus for the automatic construction of contoured maps by photographic restitution. An apparatus, named the "stereotopometer," is described, capable of giving maps with contour lines from photographs taken at the level of the ground. The method is purely optical, and this is claimed as better than the optico-mechanical methods previously described. The accuracy of the results is independent of distortion due to the photographic objectives.—Nicolas **Perrakis**: The thermodynamical interpretation of ionisation potential. The atom is transformed and equilibrium is maintained, due to the expulsion of an electron, the departure of which absorbs a quantity of entropy equal to C_iT, where C_i is the thermo-electronic constant, $4{\cdot}07\times10^{-27}$ cal./deg.—A. **Dufour**: The distortion of an electromagnetic perturbation propagated along an insulated conducting line.—G. **Bruhat** and M. **Pauthenier**: The theory of electrostriction in insulating liquids.—Michel **Doloukhanoff**: The automatic regulation of the power of an electrical installation. In an electrical installation composed of n machines of different powers, the number of combinations is $2^n - 1$, each working at or near its maximum capacity, and consequently with maximum efficiency. Thus four dynamos of 100, 200, 400, 800 kilowatts will permit of the range 100 to 1500 kilowatts, in 100 kilowatt steps and with a favourable power factor. The application of the same principle for other electrical machinery is outlined.—— **Rateau**: Remarks on the preceding communication. The use of approximate geometric series.—E. **Carrière** and **Arnaud**: Determination of the boiling-point curves and dew points of mixtures of hydrochloric acid and water under a pressure of 760 mm. The experimental results are given in tabular and graphical form.—J. **Heyrovsky**: Applications of the method of electrolysis with the mercury drop cathode.—M. N. **Goswami**: The direct hydrogenation and dehydrogenation of acenaphthene. Sabatier and Senderens showed that their method of hydrogenation with nickel at 210°-250° C. converted acenaphthene into tetrahydroacenaphthene, $C_{10}H_{10}\langle{}^{CH_2}_{CH_2}$ (with a bond between the two CH_2 groups). If the temperature of the nickel is lower, 150° C., a mixture of tetrahydro- and decahydroacenaphthene is obtained, separable by fractional distillation. If the vapours of acenaphthene, without hydrogen, are passed over nickel at 300° C., a good yield of acenaphthylene, $C_{10}H_6\langle{}^{CH}_{CH}$ (with a double bond between the two CH groups), is obtained.—P. **Lebeau**: The presence of ethane in fire-damp from the Mines de Gagnières. The presence of about 2 per cent. of ethane together with traces of unsaturated hydrocarbons (0·04 per cent.) in this fire-damp was proved. The method followed was liquefaction at −215° C., with subsequent fractional distillation.—Jacques de **Lapparent**: The phenomena of sedimentation in the Cretaceous and Eocene strata in the Western Pyrenees.—I. **Thoulet**: The liquid clouds of the ocean.—Ph. **Wehrle** and A. **Viaut**: The notion of interference in dynamic meteorology.—L. **Petitjean**: The displacement of fronts of discontinuity.—P. **Martens**: The cycle of the somatic chromosome in *Listera ovata*.—K. **Kvapil** and A. **Němec**: The relation between the "absolute air capacity" and the degree of acidity of forest soils. The absolute air capacity, defined as the volume of the pores of the soil which, after saturating the soil with water, remain filled with air, is a physical property of fundamental importance as regards fertility. The acidity, P_H, and the absolute air capacity have been measured for fifteen samples of forest soil, of which five were taken from forests containing conifers only, five from forests planted with deciduous trees only, and the remainder from regions planted with both.—E. **Rabaté**: The action of dilute sulphuric acid upon fields of cereals. Dilute sulphuric acid (4-10 per cent.), sprayed at a suitable stage of growth on growing cereals, has been proved to fertilise the soil, destroy weeds, check the action of certain parasites, and give an increased yield of 200 kgm. to 300 kgm. of seed per hectare at a cost of less than 100 francs.—A. **Policard**: The phenomena of fluorescence determined in the tissues by Wood's light (radiations of wave-length 3650 Å). Application to the histology of the human ovary. Sections which appear uniformly white under ordinary daylight illumination, show differences of colour under the Wood light, indicating differences of constitution. A detailed account is given of the application of this new histological method to the examination of the human ovary.—P. de **Beauchamp**: The transmission of variation in rotifers of the genus Brachionus.—Mlle. France **Gueylard**: The influence of life in salt water on the development of the spleen in fishes. In fishes capable of living in fresh and in salt water, a prolonged sojourn in the latter leads to a reduced development of the spleen.—Léon **Blum**, Maurice **Delaville**, and Van **Caulaert**: Modifications of the blood resulting from anaphylactic shock. The method of ultrafiltration has been applied to the blood. After anaphylactic shock, there is a marked increase in the proportion of calcium passing through the ultra-filter. This effect is constant, and indicates a change in the protein colloid.

CAPE TOWN.

Royal Society of South Africa, October 15.—J. S. **Thomas** and W. F. **Barker**: The partial pressures of water and sulphuric acid vapours over concentrated solutions of sulphuric acid at high temperatures (Preliminary note). The method adopted was the dynamical one of bubbling a carefully measured volume of air through acid at the required temperature and accurately determining the total quantity of vapour removed and also the amount of sulphuric acid contained in this vapour. Only one concentration, 95 per cent., has been completely worked out. Extrapolation of the total pressure curve to 760 mm. gave the value 304° for the boiling-point of the acid; this compares favourably with the experimentally determined value 303°-307°.—E. **Newbery**: On the application of the cathode ray oscillograph to the study of electrode phenomena (Preliminary note). The cathode ray oscillograph, supplemented with a thermionic valve, has been applied to the study of the single potential curves of a series of metallic cathodes in dilute sulphuric acid during make and break of the current through the cell. Evidence of the existence of transfer resistance was obtained. The direct method of measuring overvoltage cannot be relied upon in any circumstances; the commutator method gives true values only when extrapolation to infinite speed is carried out. All the phenomena observed are readily explainable on the hydride theory of overvoltage.—D. F. **Morrison**: The pharmacological action of *Acokanthera spectabilis*. The main action is on the heart, and is similar to that of digitalis. The hour dose for frogs, and the minimal lethal doses for cats, rabbits, guinea-pigs, and rats show that the plant, which is indigenous to South Africa, possesses a high degree of toxicity. There is a relative immunity on the part of the rabbit. The plant contains glucosides.—M. R. **Levyns**: Note on some Peninsula plants. New or interesting plants from the Cape Peninsula.

Official Publications Received.

Proceedings of the Royal Irish Academy. Vol. 36, Section A, No. 8: Corresponding Points on the Curve of Intersection of Two Quadrics. By Dr. A. C. O'Sullivan. Pp. 131-154. 2s. Vol. 37, Section B, Nos. 1, 2, 3: The Colorimetric Estimation of Thiocyanates and Cyanates, by Dr. Kenneth Claude Bailey and Mrs. Dorothy F. H. Bailey; The Reaction between Ferric Chloride and Potassium Thiocyanate, by Dr. Kenneth Claude Bailey; Freezing-points of Solutions containing Ferric Chloride and Potassium Thiocyanate, by Dr. Kenneth Claude Bailey and J. D. Kidd. Pp. 18. 1s. (Dublin: Hodges, Figgis and Co.; London: Williams and Norgate, Ltd.)

Memoirs of the Department of Agriculture in India. Vol. 13, No. 4, Botanical Series: The 'Mahali' Disease of Coconuts in Malabar. By S. Sundararaman and T. S. Ramakrishnan. Pp. 87-97. (Calcutta: Thacker, Spink and Co.: London: W. Thacker and Co.) 12 annas; 1s.

Transactions and Proceedings of the New Zealand Institute. Vol. 55. Pp. xviii+884+71 plates. (Wellington, N.Z.: W. A. G. Skinner; London: Wheldon and Wesley, Ltd.)

Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-verbaux des Réunions. Vol. 34, Procès-verbaux (Septembre 1924). Pp. 60. (Copenhague: Andr. Fred. Høst et fils.)

Memoirs of the Indian Museum. Vol. 5: Fauna of the Chilka Lake. No. 13: On a species of Sub-fossil Solitary Coral from the Chilka Lake. By Prof. George Matthai. Pp. 897-903. (Calcutta: Zoological Survey of India.) 1 rupee.

Proceedings of the Geologists' Association. Edited by A. K. Wells. Vol. 35, Part 4, December. Pp. 265-430. (London: Edward Stanford, Ltd.) 5s.

Union of South Africa: Department of Agriculture. (Division of Chemistry Series No. 31) Science Bulletin No. 30: An Investigation into some Physical and Chemical Changes occurring in Grapes during Ripening. By P. R. v. d. R. Copeman. Pp. 38. (Division of Chemistry Series No. 33) Science Bulletin No. 32: The Composition of ripe Wine Grapes from the Government Viticultural Station, Paarl. By G. Frater. Pp. 30. (Division of Chemistry Series No. 34): Some Methods of detecting Irregularities in the Composition of South African Wines. By F. Fevrier. Pp. 17. (Pretoria: Government Printing and Stationery Office.) 3d. each.

Department of the Interior: United States Geological Survey. Water-Supply Paper 514: Surface Water Supply of the United States, 1919-1920. Part 12: North Pacific Slope Drainage Basins. C: Lower Columbia River Basin and Pacific Slope Drainage Basins in Oregon. Pp. v+204+2 plates. 25 cents. Bulletin 756: Oil and Gas Fields of the Lost Soldier-Ferris District, Wyoming. By A. E. Fath and G. F. Moulton. Pp. iv+57+8 plates. 20 cents. Bulletin 750-C: Observations on the rich Silver Ores of Aspen, Colorado. By Edson S. Bastin. Pp. ii+41-62. n.p. Professional Paper 92: The Middle and Upper Eocene Floras of South-eastern North America. By Edward Wilber Berry. Pp. v+206+65 plates. 1 dollar. (Washington: Government Printing Office.)

Department of Scientific and Industrial Research. Report of the Food Investigation Board for the Year 1923. Pp. iv+77+4 plates+14 charts. (London: H.M. Stationery Office.) 3s. net.

Proceedings of the Yorkshire Geological Society. New Series, Vol. 20, Part 1, December. Edited by H. C. Versey and Herbert E. Wroot. Pp. 154+v+12 plates. (Leeds.)

Committee of the Privy Council for Medical Research. Report of the Medical Research Council for the Year 1923-1924. Pp. 142. (London: H.M. Stationery Office.) 3s. 6d. net.

Diary of Societies.

SATURDAY, JANUARY 10.

BRITISH ECOLOGICAL SOCIETY (Annual Meeting) (at University College), at 10 A.M.—Prof. F. E. Weiss: Plant Structure and Environment (Presidential Address).—Prof. Oliver: Blakeney "Far Point."—Prof. Fritch: The Algæ of Swiftly Flowing Streams.—Prof. Yapp: Deposition of Frost on Leaves.

INSTITUTE OF METALS (London Section) (at Institute of Marine Engineers), at 7.30.—Dr. R. H. Greaves: Extensometers.

MONDAY, JANUARY 12.

ROYAL IRISH ACADEMY, at 4.15.

ROYAL SOCIETY OF EDINBURGH, at 4.30.—E. L. Gill: The Permian Fish Dorypterus.—E. A. Baker: The Blackening of the Photographic Plate at Low Densities.—Dr. E. L. Ince: The Modes of Vibration of a Stretched Membrane with a particular Law of Density.

INSTITUTE OF TRANSPORT (Yorkshire Local Section) (at Town Hall, Leeds), at 5.30.—J. B. Hamilton: Inaugural Address.

INSTITUTION OF ELECTRICAL ENGINEERS (Western Centre) (at Merchant Venturers' Technical College), at 6.—W. B. Woodhouse: Presidential Address.

INSTITUTION OF AUTOMOBILE ENGINEERS (at Chamber of Commerce, Birmingham), at 7.—Dr. S. S. Pickles: The General Manufacture of Rubber (Lecture).

INSTITUTION OF ELECTRICAL ENGINEERS (Informal Meeting), at 7.—W. Day and others: Discussion on Telephonic Development in Great Britain and in the United States.

INSTITUTION OF ELECTRICAL ENGINEERS (North-Eastern Centre) (at Armstrong College, Newcastle-on-Tyne), at 7.15.

INSTITUTE OF METALS (Scottish Section) (at 39 Elmbank Crescent, Glasgow), at 7.30.—Open Discussion.

SURVEYORS' INSTITUTION, at 8.

ROYAL GEOGRAPHICAL SOCIETY (at Æolian Hall), at 8.30.—Major R. W. G. Hingston: Animal Life at High Altitudes.

INSTITUTE OF BREWING (at Engineers' Club).—J. Stewart: The Season's Barleys.

TUESDAY, JANUARY 13.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—Prof. A. Fowler: The Analysis of Spectra (1).

INSTITUTION OF MECHANICAL ENGINEERS (South Wales Branch) (at Swansea), at 6.—A. A. Fordham: Steel Construction as applied to Steel Works and Mill Buildings.

INSTITUTION OF CIVIL ENGINEERS, at 6.—Prof. A. H. Gibson: The Investigation of the Surge-Tank Problem by Model Experiments.—F. Heywood: The Flow of Water in Pipes and Channels.

INSTITUTION OF AUTOMOBILE ENGINEERS (at Royal Society of Arts), at 7.—Dr. S. S. Pickles: The General Manufacture of Rubber (Lecture).

INSTITUTION OF ELECTRICAL ENGINEERS (North Midland Centre) (at Hotel Metropole, Leeds), at 7.—W. B. Woodhouse: Presidential Address.

MANCHESTER METALLURGICAL SOCIETY (at College of Technology, Manchester), at 7.—F. C. H. Lantsberry: Tool Steels.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN, at 7.—A. S. Watson: Portraiture of Men.

INSTITUTION OF AUTOMOBILE ENGINEERS (Coventry Graduates Meeting) (at Broadgate Café, Coventry), at 7.15.—A. E. Barrett: Piston Rings.

SOCIETY OF CHEMICAL INDUSTRY (Birmingham Section) (at University), at 7.15.—H. L. Heathcote: The Testing of Resistance to Tearing.—D. F. Twiss and F. Thomas: A Comparative Study of some Vulcanisation Accelerators.

INSTITUTION OF ENGINEERS AND SHIPBUILDERS IN SCOTLAND (at 39 Elmbank Crescent, Glasgow), at 7.30.

INSTITUTE OF METALS (North-East Coast Section) (at Armstrong College, Newcastle-on-Tyne), at 7.30.—A. R. Page: Brazing.

QUEKETT MICROSCOPICAL CLUB, at 7.30.—S. R. Wycherley: The Mounting of Chemicals for the Polariscope.

ROYAL SOCIETY OF MEDICINE (Psychiatry, Neurology, Disease in Children, and Epidemiology Sections), at 8.30.—Dr. P. C. P. Cloake and Dr. F. C. Shrubsall (Psychiatry), Prof. E. Bramwell (Neurology), Prof. A. Hall (Disease in Children), Dr. McNalty and Dr. Parsons (Epidemiology), and others: Discussion on The Mental Sequelæ of Encephalitis Lethargica.

INSTITUTION OF ELECTRICAL ENGINEERS (Scottish Section).

WEDNESDAY, JANUARY 14.

ROYAL SOCIETY OF ARTS, at 3.—Lt.-Col. G. M. Richardson: Dogs in Peace (Dr. Mann Juvenile Lectures).

ROYAL SOCIETY OF MEDICINE (Balneology and Climatology Section), at 5.30.—Dr. V. Coates: Clinical Types of so-called Infective Arthritis.

RADIO SOCIETY OF GREAT BRITAIN (Informal Meeting) (at Institution of Electrical Engineers), at 6.—S. Ward: Some Notes on Short Wave Reception.

INSTITUTION OF CIVIL ENGINEERS (Informal Meeting), at 7.—H. J. Deane and others: Discussion on The Use and Abuse of Reinforced Concrete.

INSTITUTION OF ELECTRICAL ENGINEERS (South Midland Centre) (at Birmingham University), at 7.—H. W. Taylor: Three-Wire Direct-Current Distribution Networks: Some Comparisons in Cost and Operation.

NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS (Graduate Section) (at Bolbec Hall, Newcastle-on-Tyne), at 7.—J. Calderwood: Propeller Design.

ENTOMOLOGICAL SOCIETY OF LONDON, at 8.—Annual Meeting.

THURSDAY, JANUARY 15.

ROYAL SOCIETY, at 4.30.—Sir Charles Sherrington and E. G. T. Liddell: Further Observations on Myotatic Reflexes.—Prof. A. V. Hill, C. N. H. Long, and H. Lupton: Muscular Exercise, Lactic Acid, and the Supply and Utilisation of Oxygen. Parts I., II., and III.—Prof. A. P. Chattock: The Physics of Incubation.—H. M. Carleton: Growth, Phagocytosis, and other Phenomena in Tissue Cultures of Fœtal and Adult Lung.—J. F. Fulton: (a) The Influence of Tension upon the Electrical Responses of Muscle to Repetitive Stimuli; (b) Some Observations upon the Electrical Responses and Shape of the Isometric Twitch of Skeletal Muscle; (c) The Relation between the Durations of the Isometric Twitch and of the After-action of Tetanus.

ROYAL INSTITUTION OF GREAT BRITAIN, at 5.15.—J. S. Huxley: The Courtship of Animals and its Biological Bearings (1).

INSTITUTION OF MINING AND METALLURGY (at Geological Society of London), at 5.30.

INSTITUTION OF AUTOMOBILE ENGINEERS (London Graduates Meeting) (at Watergate House, Adelphi), at 7.30.—A. E. L. Collins: Steam Vehicles for Road Transport.

OPTICAL SOCIETY (at Imperial College of Science and Technology), at 7.30.—A. Steavenson: A Peep into Sir William Herschel's Workshop.

CHEMICAL SOCIETY, at 8.—E. E. Turner and A. B. Sheppard: 6-Chlorophenoxarsine.—G. A. R. Kon and R. P. Linstead: The Chemistry of the Three-Carbon System. Part III. $\alpha\beta$-$\beta\gamma$ Change in Unsaturated Acids; Part IV. A Case of Retarded Mobility.

ROYAL SOCIETY OF TROPICAL MEDICINE AND HYGIENE (at 11 Chandos Street, W.), at 8.15.—Lt.-Col. A. T. Gage and Lt.-Col. Clayton Lane: The Alkaloids of Cinchona and Malaria.

INSTITUTION OF RUBBER INDUSTRY (at 16 St. Mary's Parsonage, Manchester).—J. Adamson: History of the Rubber Industry in Manchester.

FRIDAY, JANUARY 16.

ROYAL SANITARY INSTITUTE (at Town Hall, Newcastle-on-Tyne), at 4.—Dr. A. F. G. Spinks, H. H. Evers, and others: Discussion on Maternity and Child Welfare Practice.

ROYAL DUBLIN SOCIETY, at 4.30.

ROYAL PHOTOGRAPHIC SOCIETY OF GREAT BRITAIN (Pictorial Group), at 7.

INSTITUTE OF METALS (Swansea Section) (at Swansea University College), at 7.15.—Prof. C. A. Edwards: Alloys and their Properties.

JUNIOR INSTITUTION OF ENGINEERS, at 7.30.—Major A. M. Taylor: The Hexaphase System and the Compensated 3-Phase System for 150,000 Volts Transmission with Records of Test.

SOCIETY OF CHEMICAL INDUSTRY (South Wales Section) (at Technical College, Cathays Park, Cardiff), at 7.30.—T. Lewis: Chemical Constituents of Ductless Glands.

SOCIETY OF CHEMICAL INDUSTRY (Chemical Engineering Group jointly with the London Section) (at Royal Society of Arts), at 8.—T. W. Stainer Hutchins, Dr. C. H. Lander, and others: Discussion on The Low Temperature Treatment of Bituminous Materials.

SOCIETY OF CHEMICAL INDUSTRY (Liverpool Section) (at Liverpool University), at 8.—Sir Max Muspratt, Bart.: Chemistry and Civilisation (Hurter Memorial Lecture).

ROYAL INSTITUTION OF GREAT BRITAIN, at 9.—Sir William Bragg: The Investigation of the Properties of Thin Films by means of X-Rays.

PUBLIC LECTURES.

WEDNESDAY, JANUARY 14.

LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE, at 5.—B. J. Fletcher: The Principles of Design (Introductory Lecture).

ST. BARTHOLOMEW'S HOSPITAL MEDICAL COLLEGE (Physiology Department, 6 Giltspur Street, E.C.1), at 5.—Prof. C. Lovatt Evans: The Physiology of Plain Muscle. (Succeeding Lectures on January 21, 28, February 4.)

KING'S COLLEGE, at 5.30.—Prof. A. P. Newton: The World of the Middle Age.

THURSDAY, JANUARY 15.

UNIVERSITY COLLEGE, at 2.30.—Miss Margaret A. Murray: Egyptian History (Introductory Lecture).