THURSDAY, DECEMBER 27, 1894.

A STANDARD TREATISE ON CHEMISTRY.

A Treatise on Chemistry. By Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. Vol. I. "The Non-Metallic Elements." New edition, completely revised by Sir H. E. Roscoe, assisted by Drs. H. G. Colman and A. Harden. Pp. xi. 888. (London: Macmillan and Co., 1894).

TO write a satisfactory review of this book is no easy task. The word which shall express an appreciation and a criticism does not come readily to one's pen. Turning over the pages and reading the lucid descriptions of preparations and properties of element after element, and compound following compound, one is depressed, and borne down by the burden of many facts and much learning. But to this depression succeed the pleasure and the sense of power that belong to the gaining of knowledge, and the feeling of security that remains with the man who has got down to fundamental facts.

In the preface to the first edition of this "Treatise," the authors said:

"It has been the aim of the authors . . . to place before the reader a fairly complete, and yet a clear and succinct, statement of the facts of modern chemistry, whilst at the same time entering so far into a discussion of chemical theory as the size of the work and the present transition state of the science permit."

In his preface to the present edition, Sir Henry Roscoe says:

"In this new, completely revised and reprinted, edition I have endeavoured to carry out the aims which were put forward in the preceding preface seventeen years ago."

The aim and scope of the work are made evident by these extracts from the prefaces. There can be no doubt that the authors succeed in giving "a fairly complete, and yet a clear and succinct, statement of the facts of modern chemistry." The descriptions of the properties of elements and compounds are lucid, full, and accurate; where all the properties of a substance cannot be described, the selection made is satisfactory, sometimes, one may suppose a student to say, too satisfying. But this even flow of excellent description does not inspire with enthusiasm him who reads; it does not open up glimpses of the unexplored regions; it fails to stir the emotions. The book is wanting in the charm that accompanies the "twilight of dubiety."

It is difficult always to agree with the authors in their estimate of the relative importance of chemical facts. The most important fact of modern chemistry I take to be the statement that "the properties of the elements and compounds, and the compositions of compounds, vary periodically with the atomic weights of the elements." This fact ought, I think, to be made the basis of every treatise on descriptive chemistry; because only by doing this can the facts regarding individual substances appear in right perspective. The great fact which we owe to the genius of Mendeléeff will find expression in a later volume of this "Treatise" (see p. 53); but the student who uses the book will then probably have arranged the intellectual contents of his mind, so far as chemistry is concerned, in many little parcels, each tied up separately, and he will find much difficulty in untying the parcels, arranging their contents afresh, and getting them all within the compass of the one binding generalisation.

As regards the statements of the properties of the different non-metallic elements and their chief compounds, no detailed appreciation is called for. Where all is excellent, a general expression of praise is sufficient. The chapters wherein are described hydrogen, fluorine, oxygen, sulphur, nitrogen, and the other non-metals, and the principal compounds which these element form by combining with one another, contain all that " student of chemistry requires to know about these elements and compounds, except the comparison and contrast—that is to say, the classification—of the substances described The student has presented to him, in this volume of the "Treatise," the material that is needed for acquiring a real knowledge of the chemistry of the non-metallic elements.

Some of the expressions, and the ways of putting descriptive facts, might be improved, in my opinion.

"Hydrogen occurs almost solely in a state of combination in nature" (p. 129).

"In a state of combination hydrogen occurs in water" (p. 129).

"Bromine does not occur in the free state in nature" (p. 188).

Expressions like these seem to me to be survivals from the alchemical times, when, to take an example, nitric acid was looked on as water endowed with acidic qualities, which could be removed or restored at pleasure, and hence was called *aqua fortis*. Surely it is not hydrogen that occurs "in a state of combination," but compounds of hydrogen that occur in nature. Similarly if bromine occurs at all it must be "in the free state," else it would not be bromine but something else. Each compound of hydrogen, and each compound of bromine is just as definitely a chemical individual as hydrogen, or bromine, itself.

I do not think that the object of chemistry, namely the study of the connexions between changes of composition and changes of properties, is set forth with sufficient clearness. The statement on pp. 51, 52, for instance, that

"the science of chemistry has for its aim the experimental examination of the elements and their compounds, and the investigation of the laws of their combination one with another,"

cannot be regarded as satisfactory. On the other hand, the examples given of chemical action, in the pages preceding that where the sentence just quoted occurs, undoubtedly serve to keep before the student the fundamental fact that change is the essential note of all chemical occurrences.

The term *density* is sometimes applied to gases in a way that is confusing. For instance, on p. 160 the term is applied to the relative density of chlorine, referred to hydrogen as unity, and also referred to air as unity, without an indication that the unit has been changed.

A feature of the book which is much to be commended is the giving of the definite experimental data from which important conclusions are deduced. A good example of this is seen in the authors' treatment of the eudiometric synthesis of water (pp. 246–250). The actual details of an experiment are given, with the experimentally determined data, and the conclusion to be drawn is then stated. It is much to be regretted that the authors do not quote the results obtained by Scott regarding the volumes of hydrogen and oxygen which combine to form water, but content themselves with the less recent, and certainly less accurate, measurements made by Morley (p. 251).

The authors would have done well to have followed their own practice elsewhere, and to have given moderately complete details of the methods, and the data, whereby the atomic weight of each element has been determined. In describing the electrolysis of dilute sulphuric acid solution (pp. 45, 129, 251), the authors might have more clearly insisted on the fact that the electric current is employed to set free hydrogen and oxygen from an aqueous solution of sulphuric acid, and that they had not, following it is true almost every other text-book, spoken of the phenomena as the electrolysis of *water*.

Chemical equations convey, at the best, only a small portion of the information one wishes to have regarding chemical occurrences; but, by the simple devices of using three kinds of type, and adopting a symbol to represent an aqueous solution of a substance, these equations may be made to tell much more than is conveyed to the reader by the equations used in this "Treatise."

In the extract from their preface already quoted, the authors say that they enter into "a discussion of chemical theory so far as the size of the work and the transition state of the science permit." The subject-matter of chemistry is so large, and the difficulties of bringing the vast array of facts into a focus are so great, that the science is likely to continue for a long time in a transition state, and the principles of chemistry to continue to be, as they are at present, rather a number of somewhat loosely attached hypotheses than an harmonious and binding theory. Nevertheless, more unity might profitably have been given to the chapter on the "General Principles of the Science." Many portions of this chapter are admirable; the whole of it is characterised by lucidity. The portions dealing with the laws of combination and the Daltonian atomic theory are especially excellent. Brief but very clear accounts are given of the experimental methods for determining molecular weights, including the methods which are based on van't Hoff's extension to dilute solutions of the law of Avogadro. In connexion with the molecular condition of substances in solution, there is a deliciously airy note (p. 111): "For the literature of this subject the volumes of the *Zeitschrift für Physikalische Chemie* . . . may be consulted." The student who proceeds, with a light heart, to consult the journal in question will find he has his work cut out for him.

The book, taken as a whole, is admirable. The sure position that the earlier editions of the "Treatise" have taken in chemical literature has shown how much the work was wanted, and how cordially it has been welcomed by chemists. It is sufficient to say that this, the first volume of the revised edition, well maintains the reputation of the original "Roscoe and Schorlemmer."

M. M. Pattison Muir.

MAN—THE PRIMEVAL SAVAGE.

Man—the Primeval Savage. By Worthington G Smith. (London: Stanford, 1894.)

Mr. Worthington Smith has devoted himself for many years to a study of the localities near London where implements have been found, and has described the various palæolithic floors with great minuteness, and illustrated them with great artistic skill. In this book he brings all his previous discoveries together, and groups them round his last work at Caddington, near Dunstable, on the borders of Hertfordshire and Bedfordshire. He has presented to us a monograph on palæolithic camping-places, rather than a general treatise on Man, the Primeval Savage.

The palæolithic floor at Caddington was buried under a depth of clay, sand and gravel, amounting in some places to thirteen feet from the surface. The strata occur in the following order from the surface: (1) Contorted drift; (2) reddish-brown clay, with implements stained with red ochre; (3) subangular gravel with ochreous implements, slightly worn and battered; (4) white clay; (5) gravel, with white unworn implements; (6) reddish-brown clay with implements; (7) clayey gravel with implements; (8) clayey brick-earth; (9) palæolithic floor resting on a clayey brick-earth similar to that above it. All these deposits form a thickness about eight feet in this section, and belong to the complicated series of superficial sand clays and gravels grouped together by the Geological Survey as brick-earth, and clay-with-flints, and which are clearly proved to be later than the boulder-clay of the district. The interest chiefly centres in the palæolithic floor No. 9, resting upon a sun-cracked surface in some places, and in others supporting heaps of flints carefully selected, and evidently piled together for the purposes of implement-making. Around them lay worked flints by the thousand. It is obvious that here we are on the track of a palæolithic camping-ground, and that the deposits which now cover it up have been accumulated, the fine clays by heavy rains on the margin of a stream or on the borders of a lake, and the sands and gravels by the natural drift of the soil downwards from a higher level. The distribution also of the flint implements in the section, prove that man inhabited the district while the strata were being accumulated above the palæolithic floor up to No. 2 inclusive. As the mud accumulated on the old floor, the hunters, attracted probably by the water close at hand, visited the same spot from time to time, and left their implements in 7, 6, and 5 of the section. These, our author considers to be the same age as those of the palæolithic floor. The worn ochreous implements in Nos. 2 and 3, he relegates to a later time in the palæolithic age, and considers them to have drifted downwards from a higher level into their present position. "The water must have drained elevations which have now vanished, and the hill-tops of the Dunstable district of the present time must represent the valleys of the old time." Caddington is now on the water-parting between the sources of the Lea and the Ver, and under present conditions there is no higher ground from which these materials could have been derived.

The worked flints on the palæolithic floor represent every stage of manufacture, from the unshaped block to the finished *hâche*. There are the flint cores and the flakes lying beside them, there are flint hammer-stones and anvils, punches and scrapers, and other implements broken in various stages of manufacture. Very few of the latter are finished. They would, of course, be carried off for use, as was the case with those of the palæolithic floor discovered some ten years ago, in the brickpit at Crayford, by Mr. Flaxman Spurrell. Mr. Smith, we may remark, has followed the example of the latter, in the infinite pains he has taken to build up the original forms of the flint blocks from the broken implements and splinters. It is clear that in this place we have the workshop in the condition in which it was left by the palæolithic hunter. The fact that no bones and no charcoal have been discovered, shows that the palæolithic huts were some little distance away, and that this spot was selected solely for the purpose of implement-making.

The association of the ruder with the more finished of the palæolithic implements in this floor, as in the case of many of the palæolithic caverns, proves that an appeal to rudeness of form as a test of age, is a wrong principle. That man must have learnt first of all to make the simpler before he made the more complex implements, is so obvious, that it has never been disputed. The ruder, however, were used side by side with the more finished, and many of those forms which are taken to be of pre-palæolithic age in the gravels of the Kentish plateau are the necessary result of the working of the flint block into the palæolithic *hâche*. We may remark, further, that some of these are also found in the refuse-heaps round the old flint-mines of Cissbury, and have been made in the manufacture of neolithic implements.

In the introduction, Mr. Smith deals with the general question of the relation of palæolithic man to the glacial period, and concludes, rightly in our opinion, that man inhabited south-eastern England after the glacial period. We also agree with him in looking at the pre-glacial or post-glacial age of man as merely of local significance, because the glacial period is a purely local phenomenon not marked in the warmer southern lands, such as the Indian peninsula, which was inhabited by the palæolithic hunter. We know of him in India simply as living in the pleistocene age. He probably invaded Europe in the pre-glacial age, and lived in the south while Britain lay buried under a mass of glaciers, or was covered by a berg-laden sea. He is post-glacial in the valley of the Thames. He is not separated from our own times either by a wall of ice—one of the ice periods of Prof. James Geikie—or by the tumultuous waters of a vast deluge, such as that recently put before us by Sir Henry Howorth. He is separated by a geographical revolution during which the seaboard of north-western Europe, as we find it now, came into being, and Britain became an island—as well as by a change in our land from a continental to an insular climate.

The author also touches the difficult problem of the physique of primeval man, and he accepts the "type de Canstadt" as the earliest palæolithic race. This is, however, founded on a human skull which M. d'Acy has conclusively proved to have no claim to any definite age. According to the evidence of the catalogue, still preserved, of the pleistocene mammalia found at Canstadt in 1700, it was not found along with them. Dr. Reissel, who superintended the exploration for the Duke of Würtemberg, wrote in 1701 that no human remains were then found, "inter quæ tamen nulla humanis possunt comparari." Some fifty years later Dr. Albrecht Gessner, writing on the discovery, remarks that it is strange that no human remains had been met with. Both these were doctors to the Dukes of Würtemberg, and can only be supposed to know a human skull when they saw it. It was not until 1835 that the skull in question was found by Dr. Jaeger, in the Museum at Stuttgart, and assigned without proof of any kind to the find made 135 years before. It is very unfortunate that such faulty evidence as this should not only be accepted by the authors of "Crania Ethnica," but also used for the definition of the type "de la plus vieille des races humaines." In the present unsatisfactory state of the inquiry into the physique of palæolithic man, the only safe course is to subject all the facts which have been recorded to the most searching criticism, and to wait for the further light which will come sooner or later from new discoveries. In this small and well-illustrated monograph, Mr. Smith has made our knowledge of the palæolithic workshop more definite than it was before, and has collected together a mass of information which will be of great service to the archæologists of London.

W. BOYD DAWKINS.

THE SEQUENCE OF STUDIES.

Physiology for Beginners. By Professor M. Foster, M.A., M.D., F.R.S., and Lewis E. Shore, M.A., M.D. (London: Macmillan and Co., 1894.)

Outlines of Biology. By P. Chalmers Mitchell, M.A., F.Z.S. (London: Methuen and Co., 1894.)

Practical Methods in Microscopy. By C. H. Clark A.M. (Boston: Heath and Co., 1894.)

THE scientific precision and modernness of a book of elementary physiology, written by Dr. Shore, under the supervision of Prof. Foster, is scarcely to be called in question. This little volume is amply illustrated, and written with clearness as well as exactness. The authors are especially to be commended for laying stress in their preface upon the necessity of a preliminary acquaintance with Chemistry and Physics, and it is to be regretted that they had not the courage to insist upon this point. But here they are gravely open to criticism. "Knowing," they say, "how frequently a book on physiology is taken up without any such previous acquaintance, we have given a few chemical and physical facts as preliminaries in chapter i." A few, and quite too few, it is—six complete pages—expanding scarcely any of the principles which are involved in the simplest physiological explanation, giving, of course, no conceptions of the relations of chemical combination to energy, nor of osmose, diffusion, solution, isomerism, nor the action of ferments, all of which come to the front directly one approaches respiration or digestion. We cannot but think that this concession to a common educational error is greatly to be deplored. The authors occupy a position of authority, and it was their privilege—a privilege they have neglected—to demand here, by assuming a sound basis of chemical and physical knowledge, the proper sequence of studies. As it is they have produced a little primer that by virtue of its clearness and attractiveness and the prestige of their names, will serve to uphold for a few years longer a fundamentally faulty system of scientific education.

The evil of a neglect of the rational sequence of studies becomes particularly apparent in the chapters upon the eye and ear. In the former of these an attempt is made to convey all the optical principles involved, in seven lines—"convex lens" is not even defined—and in the latter comes a series of dogmatic statements about sounds and noises, without a particle of that progressive reasoning process which is the very essence of genuine scientific study. Once the initial concession was made, however, this kind of thing was an inevitable consequence. In order to explain the science in hand, three or four others have to be compressed to the limits of a paragraph.

The same unfortunate disposition to begin the wrong way about is apparent in the little book by Mr. Chalmers Mitchell. But in his case there is even less excuse. His book is designed to prepare students for the Conjoint Boards Examination, and therein he is an examiner. Since he calls the tune he might have danced as he liked, and he has, we conclude, preferred of his own free will to contravene the common-places of educational science. We find such a proposition as the following, printed in spaced type; so that the medical student, preparing for examination by Mr. Chalmers Mitchell, who fails to learn it by heart will have only himself to blame for his failure. The earthworm, we are told,

"has reached the second stage of cœlomate development in that it is very highly segmented, and there is little or no trace of the third stage, the stage of the condensation of segments." . . . "Vertebrates are highly segmented animals, in which condensation of segments has become an important factor, resulting notably in the formation of a complicated head, and of kidneys formed by the aggregation of many nephridia."

Now these propositions are illustrated rather than supported by a brief description of the anatomy of the earthworm, dogfish, and frog, and we find that even in the case of these types the metameric segmentation of the cranial nerves is scarcely alluded to, and the homology of the mandibular arch with the branchial bars is not presented as a probability, but stated as a fact. And, in brief, Mr. Chalmers Mitchell, who is not a crammer, but a teacher, gives the medical student the impression almost in so many words—"cut and dried" and ready to be cast into the oven—that the vertebrate type is merely a concentrated derivative (concertina fashion) of the chætopod type, advancing this pure, and as he gives it, baseless, speculation, in the face of the absence of any chætopod stage in the embryology or palæontology of the vertebrates, in the face of the lesser metamerism in the vertebral column of more primitive fishes, and in the face of the declared opinion of many prominent anatomists. But whether the view he gives is right or wrong is, from our point of view, the smaller issue; the great and grave objection is the unscientific spirit of the presentation, the narrowness of the base of anatomical fact upon which this far-reaching generalisation is raised. We find this disposition to what is really the old theological trick of dogmatism, again and again in his book, and it is the evident and necessary consequence of an attempt to touch the far-reaching theories of comparative anatomy without a sufficient preliminary study of individual types.

It is odd that we should find another aspect of the same mistake cropping up in one chapter of Mr. Clark's extremely useful and well-arranged handbook for the beginner in microscopy. It is in almost every way a well-arranged and well-written work, and will be particularly a boon to the amateur to whom experienced advice is inaccessible. But before proceeding to the petrographical instrument, Mr. Clark has attempted a "concise description" of polarised light, which begins—

"The elasticity of the ether in space is believed to be equal in all directions. The same is true of the ether in non-crystalline substances and in crystalline substances of the cubical system. The particles of ether are consequently free to vibrate equally in all directions. In other crystalline substances the elasticity of the ether is modified by the crystalline structure. In some crystals there is one axis or direction about which the molecules are arranged in a uniform manner; such crystals are said to be uniaxial. In other crystals there are two such axes."

Now we believe a student who will clearly understand this will be sufficiently advanced not to require it, and that to the raw beginner, this passage, and its context, will be incomprehensible. Were it not for the actual evidence of these books it would seem the most unnecessary thing in the world to assert that a clear working idea of the *theory* of polarised light, or the general ideas of chemistry and physics, or a cyclopædia of the anatomy of the metazoa, cannot be imparted in half a dozen pages or so of text. If it could, our text-books in these subjects would be unnecessary, for the ultimate aim of all intelligent research and teaching in pure science is broader and simpler general notions, and there can be no need for a volume if a handbill will suffice. Cannot the scientific writer insist upon the proper sequence of studies in his preface, and proceed on the assumption that his counsel will be observed? To positively encourage students to proceed to subjects for which they have not the necessary grounding, to proffer them snap-shot chapters upon these neglected preliminaries, is really, we are persuaded, to place a grave impediment in their way to genuine knowledge, all the graver because it seems a help, and to place one also in the way of our advance towards more efficient science teaching in the future.

H. G. Wells.

OUR BOOK SHELF.

Climbing and Exploration in the Karakoram-Himalayas. By William Martin Conway, M.A., F.S.A., &c. Containing Scientific Reports. (London: T. Fisher Unwin, 1894.)

This supplementary volume contains reports on the scientific results of Mr. Conway's adventurous journey, with his map of the mountain region between Rakipushi and Golden Throne, through which he travelled. The author supplies a list of measured altitudes and notes on the map, mentioning the differences from that of the Trigonometrical Survey of India. Lieut.-Colonel A. G. Durand describes the ethnology and later history of the

Eastern Hindu Kush, giving a brief sketch of the physiography of the region. Prof. T. G. Bonney and Miss C. A. Raisin furnish notes on the rocks collected by Mr. Conway, from which it appears that the majority much resemble those of the Alps. The most interesting specimens are a peculiar schist with secondary mica, a piedmontite-schist, and a fragment allied to pseudo-jade. Mr. W. F. Kirby identifies the butterflies, Dr. A. G. Butler the moths, and Mr. W. B. Hemsley the plants. Of the last about a dozen were obtained at or over 16,000 feet. The well-known *Saxifraga oppositifolia* was gathered at 17,000 feet, and another species (the highest habitat) at 17,320 feet. Mr. W. L. H. Duckworth writes on two skulls brought from Nagyr, and Prof. C. Roy discusses Mr. Conway's notes on mountain sickness, coming to the conclusion that the primary cause of it is asphyxia. Mr. Conway's observations agree with those of other experienced climbers, that a man in good condition begins to feel the effect of increased altitude at about 16,500 feet. The fact that he is sensible of more inconvenience when in a hollow among the peaks than when on an exposed ridge, Prof. Roy attributes to some loss of oxygen by the air when it has passed over a considerable tract of melting snow. Mr. Conway has made valuable additions to our knowledge of the geography and physical history of this remote mountain region, and the present volume supplements the more popular account of his travels, which appeared earlier in the year.

The Royal Natural History. Edited by Richard Lydekker, B.A., F.R.S. Vols. i. and ii. (London: Frederick Warne and Co., 1893-94.)

ABOUT twelve months ago (NATURE, vol. xlix. p. 220), in a short notice of the two first parts of this work, we heartily recommended it as worthy of the notice of our readers. On a careful perusal of the two volumes now before us, which equal one-third of the projected series, we still feel quite justified in our recommendation; the illustrations are for the most part extremely good, and the text is not only interesting, but it is also intelligently written.

The first of these volumes treats, in fifteen chapters, of the Primates, the Chiroptera, the Insectivora, and the Carnivora, as far as the dogs. We would especially notice the chapters on the cats and the dogs, as having information well up to date. Instead of the often-quoted old stories, it is refreshing to meet with accounts of the habits of these animals, taken from the writings of V. Ball, Blandford, Guillemard, Hudson, and Sterndale. Thus, in the account of the common Indian mungoose, we find mention of the results, to within the last year or two, of Mr. Espent's experiments of introducing this little carnivore to Jamaica. The sugar-planting industry in this island was threatened with destruction on account of the swarms of rats; within three or four years after the introduction of the mungoose the rat plague came to an end, and the beneficial results to the island exceeded £150,000 a year. Volume ii. commencing with the bears, finishes the Carnivora, and describes the hoofed mammals. The illustrations play so important a part in these volumes, that we would suggest that the comparative sizes of the figures should always be given, and when possible the reader should be told where the figures first appeared.

Kitchen Boiler Explosions. By R. D. Munro. Pp. 44. (London: Charles Griffin and Co., 1895.)

THE time having again arrived when domestic boilers will be a source of trouble to paterfamilias, Mr. Munro comes forward with an account of a series of experiments with red-hot kitchen boilers, apparently reprinted from the *Transactions* of some Society. Whether this be so or not we do not wish to inquire, but to us it seems that the diagrams of steam-pressure are little suited to the "intelligent householder" for whose edification they are intended. The chief conclusions drawn from the experiments are that (1) a dead-weight safety-valve should be fitted to every boiler; (2) water will flow into a red-hot boiler although there is no free outlet, and, also, that a steam-pressure can be attained in such circumstances sufficient to cause rupture of the strongest boilers in use; (3) whilst a very high steam-pressure may be generated in a red-hot boiler by the sudden injection of cold water, a disastrous explosion cannot thus be produced; (4) an explosion, in the true sense of the word, cannot occur unless the boiler contains water as well as steam. Probably the perusal of Mr. Munro's book will help to diminish the disasters from boiler explosions.

The Island of Madeira, for the Invalid and Naturalist. By Surgeon-General C. A. Gordon, M.D., C.B. Pp. 110. (London: Baillière, Tindall, and Cox, 1894.)

PERSONS who are fortunate enough to be able to leave England during the dreary months of winter, and who select to sojourn in Madeira—"The Flower of the Ocean" —should take this brochure with them. The characteristics of the people and the place are concisely stated, and there is more information on the geology, meteorology, zoology, and botany of the island than is usually given in guide-books of a similar kind. It is well known that Madeira has an extensive fauna and flora, and we agree with the author that it is a matter of regret that the island has no public museum where they could be collected and investigated. Prof. Smitz, however, is gradually forming such an institution at the college in Funchal.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"Acquired Characters."

Now that the correspondence on this subject, which you allowed me to start in your number of November 1, seems drawing towards a close, I ask leave through you to thank your correspondents for their courtesy in replying to my inquiries, and also to make a few observations by way, so far as I am concerned, of conclusion.

As none of your correspondents has found any fault with the conditions which I suggested as essential to a good definition, I conceive that I may assume them to be correct.

Furthermore, as none of these writers has adopted or defended any of the definitions which Weismann appeared to me to give or to suggest, or has said anything by way of criticism on my strictures on these definitions, I think that I may conclude that I was not far wrong in those strictures, and that Weismann's writings do not afford any good definition of the words to which he has given currency.

Mr. Poulton has suggested that a definition may be found in the statement that "whenever an organism reacts under an external force, that part of the reaction which is directly due to the force is an acquired character." But surely this is difficult of application: for in every case of a reaction on an external stimulus there are two elements—viz. first, the internal capacity to respond, and secondly, the external force or stimulus. Each of these is necessary to the result and to every part of the result, and neither is of itself sufficient to the result or to any part of the result. How then can we analyse or break up the result or the reaction into two parts, and say that the one is the direct result of the external force, and the other is either not its result at all, or its indirect result? Where there is the joint action of two causes, each necessary but neither sufficient of itself, I conceive that you cannot either logically or physically sever any part of the result from the action of both of the causes, and there is no ground for attributing directness to one part of the the effect, and indirectness to another. Mr. Poulton dwells truly on the reaction having two causes—the internal and the external; but this does not justify the analysis of their results into two parts.

Nor does the matter become plainer to me when I follow Mr. Poulton in his application of his definition to concrete cases. The first is the case of the so-called "Exercierknochen," a reaction on an external force resulting in a certain number of changes of tissue. How is it possible to classify those changes into two categories—the one including changes directly due to the external force, the other changes not directly due to it? If the stress is to be laid on the word "direct," then one must respectfully ask what is its meaning? how is it to be ascertained and verified? one must inquire whether what is said to be an indirect result, means anything but a result which is transmitted? in which case we should find ourselves in a vicious circle. I will not follow Mr. Poulton through the other instances, but I believe the reader will find, like me, that each raises similar difficulties, and that in no case does he analyse the actual result into two distinct and separable parts.

Mr. Galton proposes, and Prof. Ray Lankester adopts another definition, viz. "Characters are said to be acquired when they are regularly found in those individuals only who have been subjected to certain special and abnormal conditions." Now I suppose that characters can be found regularly only either (1) in individuals exposed to conditions which induce them, or (2) in individuals who have inherited them. To say, then, of a character that it appears only regularly in individuals exposed to certain conditions, is to say that it does not appear in individuals by inheritance, and the proposition that acquired characters are not transmissible is thus reduced to an identical one. The possibility of inheritance is excluded by the definition, and the inquiry whether acquired characters are inherited is impossible.

I do not propose to follow your learned correspondents into many of the subjects touched on by them; but the more their letters are read, the more apparent does it become that they are not at one, either with themselves or with Lamarck or Weismann, as to the use of the words "acquired characters;" and for myself, I repeat my regret that an inquiry of great moment should be obscured, as I venture to think, by a premature use of classificatory words before the real classification of nature herself has been ascertained. For the question, "Are acquired characters transmissible?" I hope to see substituted the inquiry "What characters are transmitted?" EDW. FRY.

The alleged Absoluteness of Motions of Rotation.

All motion is relative. This apparently universal statement is a particular statement about the meanings of words. The word "motion" means "relative motion," or, more precisely, "the motion of a body" means its motion relative to other bodies. We may go further and make the still more fundamental statement that *all position is relative*, or, in other words, the position of a body means its position relative to other bodies. It is easy for anyone who puts words together without reflecting sufficiently on their meaning to put together the noun "motion" or "position" and the adjective "absolute," but the expressions "absolute motion" and "absolute position" are nevertheless meaningless, just as much so as "white blackness," "retrograde progress," "the action between a rough body and a smooth body at their point of contact," or "the potential energy of non-conservative forces."

The above remarks contain the standpoint from which it is concluded that motions of rotation are no more "absolute" than motions of translation. Anyone who accepts them for translation, but rejects them for rotation, places himself in an illogical position. Unless I have misunderstood them, this is the position occupied by Maxwell and Prof. Greenhill. The standpoint of those who assert that position and motion are in all cases relative is directly opposed to that of Newton. He held that there is absolute position in immovable space, and relative position with respect to bodies, and correspondingly there are absolute and relative motions; that absolute motion of a body is its transference from one absolute place to another absolute place; that we can never determine the absolute place of a body, but only its relative place; that in cases of rotation we can distinguish absolute from relative motion by the effects of "centrifugal force." He gives no indication how to distinguish absolute and relative motions of translation. From the impossibility of determining the absolute position of a body it seems to follow that absolute motion of translation cannot be determined; this view is adopted by Maxwell.

Assuming that it is the object of the Science of Mechanics to give as simple a description as possible of the observed facts about the motions of bodies, the assumption at the outset of absolute motions or positions about which we can know nothing appears an unnecessary complication.

From a logical point of view the cardinal statement in the discussion is that all position is relative. It appears to be conceded by Newton, and has been insisted upon by Maxwell, that all knowable position is relative. To say that at any instant a body has an absolute position in space, but that we can never know where it is except by reference to other bodies, that is to say that every body has an absolute position, although we can only know its relative position, is to introduce an unnecessary complication, if it is not to talk nonsense.

What is done in practice is to determine the position of a point by reference to a Cartesian system of axes, or by an equivalent method. What is called "the velocity of a body" is its velocity relative to the axes; what is called "the acceleration of a body" is its acceleration relative to the axes; a body has a motion of rotation when the angles between lines of the body and any axis are changing with the time. It is part of the solution of a mechanical problem, as presented by any set of observed facts, to determine the system of axes with reference to which the description of the observed motion becomes as simple as possible, and there exists a calculus for transforming the expression of a motion from one system of axes to another when the relative motion of the two systems is known.

The question how the axes are to be determined has been much discussed, but no general answer appears possible. Particular answers apply in particular cases. In general any three points, not in a straight line, determine a set of coordinate axes. For one of the points may be chosen as origin, the line joining this point to another of the points may be chosen as one coordinate axis, and the plane of the three points may be chosen as one coordinate plane. For the three points any identifiable parts of bodies may be taken; but, in general, axes so chosen will be inconvenient, or, what comes to the same thing, the description of a motion by reference to them will not be simple.

The description of motion is generally made in terms of the concept *force*, that is to say, we state the acceleration relative to the axes which a free body placed in a given position relative to the axes, and moving with a given velocity relative to the axes would have, and the nature of the constraints which give rise to differences of acceleration in a constrained and a free body moving through the same position with the same velocity relative to the axes. In an actual problem the acceleration of a free body whose other circumstances (position and velocity relative to the axes) are known, must be found by experiment. It does not concern the matter now under discussion that among these circumstances position is generally predominant over velocity in determining accelerations. What is of more importance is the fact that *the force acting on a body depends on the system of axes chosen.* For the force is a vector quantity whose line of action coincides with the line of the acceleration relative to the axes, and whose magnitude is proportional to this acceleration. This point has been noted by Maxwell. The result that the field of force depends partly on the axes is frequently a guide to the choice of convenient axes of reference, namely, we choose axes with respect to which the expression of the field of force is simple, and it often happens that in this way all motional forces, other than frictional resistances, can be made to disappear. An easy and striking example of the differences introduced by changing the axes can be found by considering the motion of two particles which move in one of the planes chosen as coordinate planes with uniform velocities in different lines relative to the coordinate axes. If a new set of axes is constructed by taking one of the particles as origin, and the line joining the particles as one axis, the acceleration of the other particle relative to the new axes is directed towards the first particle, and varies inversely as the cube of the distance between the two particles. Another very interesting example is furnished by Foucault's pendulum, to be discussed presently.

These remarks will serve as a preparation for the way in which we interpret, in accordance with the principle of the relativity of motion, those experiences which have been held to favour the view that motions of rotation admit of absolute determination. The history of the discussion appears to show that little would have been heard of this doctrine apart from the desire to explain to a public unused to regard motion as relative the theory of the rotation of the earth. To one who has mastered the relativity of motion it is manifestly the same thing to say (1) that referred to axes fixed in the earth all the stars describe circles every day about the polar axis, or (2) that referred to axes fixed among the stars the earth rotates about its polar axis once a day. If any ground can be alleged for holding that one of these statements is the simpler, that is a ground for a certain choice of axes, not for saying that one motion is "real" or "absolute," and the other "relative" or "apparent."

All the so-called "proofs of the earth's rotation" are deductions from particular experiences which show that other motions besides the diurnal relative motions of the earth and stars are more simply expressed by referring to axes fixed among the stars than by referring to axes fixed in the earth. They all depend on the specification of "the acceleration due to gravity" near the earth's surface. The neighbourhood of the earth is a field of force, and the magnitude and direction of the force at any point depend on the axes of reference. The specification of the field of force is simplest when referred to the centre of the earth as origin, and to axes fixed in direction with reference to the stars. The field is then expressed by the law of gravitation.

It is worth while to elucidate this matter in greater detail by an examination of the most famous of these "proofs," that by means of Foucault's pendulum. What is observed is that if the pendulum is really free to swing about a point, and if the bob always passes above the same point of a horizontal table (fixed with reference to the earth) when at the lowest point of its swing, then the plane of vibration turns slowly round, so that the line of vibration is above now one, now another line drawn on the table, the oscillation in the line being practically simple harmonic. If this motion were referred to axes fixed with reference to the table, there would be a component acceleration from the bob of the pendulum towards the point of support (to be accounted for by the constraint), a component acceleration in the plane of vibration at right angles to the former (which we should recognise as a component of gravity), and a component acceleration perpendicular to the plane of vibration, and proportional at any instant to the velocity in the simple harmonic motion. If we had nothing else to guide us, no observation of the stars, no theory of gravitation, but knew only from less refined observations that free bodies fall downwards with constant acceleration, we should have to do two things: we should have to try to simplify the specification of the acceleration of the bob of the pendulum by referring to a new set of axes, and we should have to conclude that our previous observations of falling bodies had not disclosed all the facts about the field of force in the neighbourhood of the earth. We should simplify the specification of the observed accelerations by referring to axes which (relative to the earth) rotate with the plane of vibration of the pendulum, and we should conclude that such axes are required in order that the laws governing the motion of falling bodies may be correctly formulated. What the experiment with Foucault's pendulum really proves is not that the rotation of the earth relative to the stars is an "absolute motion," but that the system of axes, with reference to which the acceleration of a free body near the earth's surface is of constant amount and directed towards the earth's centre, is not fixed in the earth, but (relative to axes fixed in the earth) these axes rotate with the stars.

It will be found on examination that every other so-called "proof of the earth's rotation" is of the same character. By each it is shown that the earth rotates in the same time and in the same way relative to the axes required for the statement of the law of gravitation as relative to the stars. It is not legitimate to suppose that two relatives make one absolute.

It is true that the conclusion at which we have arrived takes longer to state, and appears at first sight less simple than the statement by way of "absolute motion," but it contains no undefined terms, and no reference to anything assumed to exist, but about which nothing can be known.

Objection has been taken to the attempt to express mechanical theory in terms of relative motion, on the ground that it will be perplexing to beginners, and difficult at any stage. In answer to this it may be urged that in teaching beginners there is no need to say anything about either relativity or absoluteness. The motions that interest them are motions relative to the earth; the motions of boats, trains, cricket-balls, billiard-balls, and machinery; things that can be sufficiently described by reference to lines fixed in the earth. It is only at a later stage when general mechanical theories have to be studied, and a foundation laid for physical astronomy and mathematical physics, that it is proper to insist on the relativity of motion; and at this stage it appears to me more important that our statements of principles should be free from metaphysical obscurity than that they should be verbally short.

A. E. H. LOVE.

The Antiquity of the "Finger-Print" Method.

SIR WILLIAM HERSCHEL, in his letter to NATURE (Nov. 22, p. 77), expresses his unbelief in the statement in the *Nineteenth Century* (No. 211, p. 365), which ascribes to the Chinese the original invention of the "finger-print" method of personal identification. While I do not know upon what Mr. Spearman has founded this statement, I have collected from a few sources some facts which seem to justify the claim made on behalf of the Chinese.

Although at present I have no record to refer to, it is a fact that every Japanese, old enough to have outlived the *ancien régime* that passed away in 1869, well remembers the then current usage of "stamping with the thumb" (*Bo-in*) on legal papers, popularly called "nail-stamp" (*Tsume-in*), on account of the common use of a thumb with the edge of its nail in ink; whereas on papers of solemn contract, accompanied by written oath, the "blood-stamp" (*Keppan*), or the stamp of the ring-finger in blood drawn therefrom, was demanded.[1]

Chûryô Katsurakawa, the Japanese antiquary (1754–1808), writes on the subject as follows: "According to the 'Domestic Law') (*Korei*), to divorce the wife the husband must give her a document stating which of the Seven Reasons[2] was assigned for the action. . . . All [letters] must be in the husband's handwriting, but in case he does not understand how to write, he should sign with a finger-print. An ancient commentary on this passage is: 'In case a husband cannot write, let him hire another man to write the document . . . and after the husband's name sign with his own index-finger.' Perhaps this is the first mention [in Japanese literature] of the 'finger-print' method" (1) This "Domestic Law" forms a part of the "Laws of Taihô" enacted in 702 A.D.; with some exceptions, the main points of these "Laws" were borrowed and transplanted from the Chinese "Laws of Yung-Hwui" (*circa* 650–55 A.D.) (2); so it appears that the Chinese of the 7th century A.D. had already acquired the "finger-print" method.

After the above-quoted passage, Katsurakawa continues thus: "That the Chinese apply on divorce-papers the stamps of the ends of the thumb and four fingers, which they call 'Shau-mú-ying' (*i.e.* hand-pattern stamp) is mentioned in 'Shwui-hü-chuèn,' &c." (3). This "Shwui-hü-chuèn" is one of the most popular novels enjoyed by the modern Chinese—so popular that I have met with many Chinese labourers possessing it in the West Indies; its heroes flourished about 1160, and its author lived in the twelfth or thirteenth century A.D. (4). As is usual with many other examples, this novel gives us more accurate descriptions of minor institutional features that co-existed with either the heroes or the author, or both (5). After making careful search in this novel, I can now affirm that the Chinese in the twelfth or thirteenth century used the finger-prints, not only in divorce, but also in criminal cases. Thus the chapter narrating Lin Chung's divorce of his wife, has this passage: "Then Lin Chung, after his amanuensis had copied what he dictated, marked his sign-character, and stamped his 'hand-pattern'" (6). And in another place, giving details of Wu Sung's capture of the two women, the murderers of his brother, we read: "He called forth the two women; compelled them both to ink and stamp their fingers; then called forth the neighbours; made them write down the names and stamp [with fingers]" (7).

It has been lately suggested by my friend, Mr. Teitarô Nakamura, that possibly the "finger-stamp" was merely a simplified form of the "hand-stamp," which latter method had once been so current in Japan that it gave to the documents the common names "Tegata" (*i.e.* hand-pattern) and "Oshite" (*i.e.* impressed hand)[3] (8). This view applies equally well to

[1] The "thumb-stamp" was equally regarded with the formal engraved seal (*Jitsu-in*), but the "blood-stamp" had nothing to do for identification. For the formula of the latter mode of stamp, *vide* Ota, "Ichiwa Ichigen," new edition, Tokyo, 1882, vol. xiii. p. 39.

[2] The Seven Reasons for divorcing the wife are: (1) filial disobedience; (2) barrenness; (3) licentiousness; (4) jealousy; (5) leprosy; (6) loquacity; (7) larceny.

[3] It must not be presumed as a fact that after the "finger-stamp" was introduced, it soon supplanted the "hand-stamp"; for even in the seventeenth century the latter was sometimes used, as is instanced in the writing of Katô-Kiyomasa (1562–1611) preserved in a monastery near Tokyo. *Cf.* Kitamura, "Kiyû Shôran," new edition, 1882, vol. iv. p. 16.

the case of the Chinese, for they still use the name "hand-pattern" for the finger-print (see above). That this "hand-stamp" was in use in an ancient kingdom of Southern India, there is a proof in the Chinese records (9).

When we recognize that the hand-marks were early in use for identification by the three distinct nations, the Japanese, Chinese, and Indians, and when we consider that even the teeth-marks were so commonly used for authentication in India that the heir-apparent to As'ôka Râdja did not hesitate in plucking out his own eyes on recognizing the king's teeth-mark that accompanied the false epistle (10), it would seem quite true that among those ancient nations who were, with few exceptions, ignorant of the use of "written signature" method, it was but a natural process that the methods were invented to apply to identification some more or less unchanging members of human body.

Further, that the Chinese have paid minute attention to the finger furrows, is well attested by the classified illustrations given of them in the household "Tá-tsáh-tsú"—the "Great Miscellany" of magic and divination—with the end of foretelling the predestined and hence *unchanging* fortunes (11); and as the art of chiromancy is alluded to in a political essay written in the third century B.C. (12), we have reason to suppose that the Chinese in such early times had already *conceived*—if not perceived—the "for ever unchanging" furrows on the finger-tips.

Bibliography.—(1) "Keirin Manroku," 1800, new edition, 1891, p. 17. (2) Y. Hagino, "Nihon Rekishi Hyôrin," 1893, vol. vi. pp. 2, 24. (3) Same as (1). (4) Takizawa, "Gendô Hôgen," 1818, vol. ii. chap. xli. (5) *Cf.* Davis, "China," vol. ii. p. 162; Bazin, "Théâtre Chinois," Introduction, p. li. (6) Shi-nái-ngán (?), "Shwui-hü-chuèn," Kin's edition, Canton, 1883, tom. xii. p. 4. (7) *Ibid.*, tom. xxx. p. 18. (8) *Cf.* Terashima, "Wakan Sansai-dzue," 1713, tom. xv. art. "Tegata." (9) Twan Chìng-Shih, "Yú-yáng Tsáh-tsu," ninth century A.D. tom. xiv. (10) Hiuen-tsang, "Sí-yú-kí," sub. "Takchas'ila"; Hirata, "Indo-zôshi, MSS. vol. xxi. pp. 10–11, 26. (11) Terashima, *op. cit.* tom. vii. art. "Ninsômi." (12) "Kan-fei-tze," tom. xvii. sub. "Kwei-shi."

KUMAGUSU MINAKATA.

15 Blithfield Street, Kensington, W., December 18.

Peculiarities of Psychical Research.

MAY I enter an emphatic protest against the notion insinuated both by Mr. Wells and Prof. Karl Pearson, that "Psychical Researchers" are a sort of sect engaged in spiritualistic or other propaganda? Most people, I am afraid, fight shy of psychical research, either because they are afraid that *if* there is anything in it it is the devil, or because they have a scientific reputation which they are afraid of losing. I do not know to which category Mr. Wells belongs, but apparently he fails to understand that in order to make out a case against psychical research he has got to show, not that the existence of telepathy and clairvoyance has not been proved, but that there is not even a *prima facie* case worth investigating. When we remember that ten years ago "mesmerism 'was included along with telepathy and clairvoyance, we shall not attach much importance to such efforts to stifle inquiry. Even if the result should be to confirm Mr. Wells's anticipation, and show that all the coincidences that have been reported can be explained away as mistakes or misstatements, the inquiry will yet have been worth the labour bestowed on it, if only as affording a measure of the value of testimony to the miraculous. And if this comes to pass, the bigots of science will be ready enough to claim a share in the work, if only by saying, "I told you so!"

I do not know what Prof. Karl Pearson means by his quite gratuitous attack on "the scientific acumen of the psychical researchers." Surely he cannot imagine that they overlooked the point which he has unearthed? The instructions to the experimenters were, that "the agent should draw a card at random, and cut the pack between each draw" ("Phantasms of the Living," vol. i. p. 33, foot-note). Could an abnormal distribution of the cards affect the result if those precautions were taken, or has the Professor any reason to suppose the instructions were not carried out? EDWARD T. DIXON.

Cambridge, December 14.

THE following are a few of my grounds for questioning the scientific acumen of the psychical researchers:—(1) M. Richet's experiments are cited as if they were significant of telepathic action. On the contrary, they give odds of so little weight that they are significant of nothing but want of acumen. I have in card drawing, tossing and lottery experiments, all conducted with every precaution to secure a random distribution, obtained results against which the odds were more considerable. (2) Mr. Dixon is unable to see the importance of ascertaining whether there was an abnormal distribution in the cards cut or the cards guessed. His inability is a strong confirmation of my standpoint. (3) I have heard lectures, and read papers written by psychical researchers. Both alike seem to me akin to those products of circle squarers and paradoxers, with which, as a reviewer, I am painfully familiar. As a concrete example, I take my friend Dr. Oliver Lodge's psychical papers. They are typical, to my mind, of the manner in which the scientific acumen of even a professed and most highly competent man of science vanishes when he enters this field of "research."

I do not intend to take part in a controversy on the subject at the present time, but I do suggest that no better exercise could be found for a strictly logical mind with plenty of leisure than a criticism of the products of the chief psychical researchers. Such a criticism would be of much social value, in the light of recent attempts to popularise the "results" reached by these investigators. KARL PEARSON.

University College, London, W.C. December 19.

The Artificial Spectrum Top.

I HAVE read with interest Prof. Liveing's theory of my artificial spectrum top as summarised in NATURE of Dec. 13, p. 167, and am sorry I did not know of his conclusions before he made them public, because a very simple experiment would, I think, have convinced him of their inaccuracy. If Prof. Liveing, or any of your readers, will examine my top rotated in the light of a *bright* sodium flame, they will find that the colours are quite distinct. I know of no other way of seeing blue and red by the light of sodium, and the phenomenon, I think, shows decisively that the colours of the top are "artificial" sensations in the sense explained in my theory of the instrument.

December 16. CHARLES E. BENHAM.

I HAVE examined Mr. Benham's top by the light of a bright sodium flame, but have failed to see anything like the colours which I see by daylight or by the light of an incandescent electric lamp. By the sodium light the outmost three circles appear, when the rotation is one way, to be dark brown, the inmost three dark leaden grey, while the intermediate circles are paler brown. Reversing the direction of rotation interchanges the appearances of the outmost and inmost three circles. I cannot see any red or blue, or green, in any case. Other people here seem to see much the same as I do when the top is illuminated by the sodium flame only. With certain black and white figures of my own, I can get a pink appearance in the sodium light, but no green or blue. With spiral figures, which are worrying to look at, I find that some people can see a play of colour even with the sodium light, but I do not see it myself. Using a turn-table, by which the rate of rotation can be regulated at will, I have found that the speed, in white light, required to bring out the colours is decidedly different for different people. This fact convinced me that the explanation of these very curious appearances must be looked for in some physiological cause. It is perhaps worth remark that a sodium flame, when there is much sodium in it to make it bright, is by no means monochromatic, though sufficiently so to make the experiment with the top a very interesting one; and as Mr. Benham sees colours by this light which some others fail to see, it goes far to prove the phenomenon to be subjective.

Cambridge, December 19. G. D. LIVEING.

"Solute."

CORRESPONDING to the words "solvent" and "solution," some word is very badly wanted to express "the dissolved substance." The analogous word is evidently "solute," and it is as short and euphonious as the others. May I inquire why it is not in general use? Surely some one must have proposed it?

Leipzig. F. G. DONNAN.

"The Elements of Quaternions."

IN answer to my reviewer's question (*vide* p. 154), I must frankly admit that

(*a*) Eq. 8, p. 40, should have been a group of six equations, $i = \sqrt{-1}, j = \sqrt{-1}$, &c.; and that

(*b*) The inference should have been that i, j, &c., are (unequal) square roots of negative unity.

H. W. L. HIME.

THE LICK OBSERVATORY.

THE recent issue of volumes ii. and iii. of the "Publications" of the Lick Observatory serves to give some indication of the growing activity of this world-famed institution, and to foreshadow the great part which it is destined to play in the astronomy of the future. As in the case of so many other observatory publications, these volumes contain much with which the various astronomical journals have already made us familiar, and one of their chief objects appears to be to collect the observations into a convenient form for reference.

Volume ii. is entirely devoted to the magnificent micrometric work on stars and nebulæ performed by Mr. Burnham during his four years' connection with the Observatory, which, to the general regret, terminated in June 1892. It will be a matter of satisfaction to all interested in the progress of astronomy to learn that this keen-sighted astronomer has nothing but praise for the great telescope. He says: "It has more than satisfied the severest tests which could be applied, and the highest expectations concerning its performance have been realised. It is a monument of the genius and skill of the unrivalled opticians, Alvan Clark and Sons, to whom the progress of astronomical work all over the world is so largely indebted." The fact that powers up to 2600 have been successfully employed, further emphasises the excellence of the objective.

Mr. Burnham strongly insists upon the advantages to be gained by the use of a micrometer in which the wires are bright on a dark field. With this method of illumination, he tells us, "any object that can be seen under any circumstances, however faint, can be well and accurately measured. There is no such thing as a star too faint for measurement, if it can be seen at all."

Besides the immense number of numerical results, the volume gives a mass of most interesting information relating to the various objects observed. Some of this has already been published, but many new points have been added. Thus, it appears that the observations of θ Orionis show that "the six principal stars are absolutely fixed with reference to each other, so far as any change is concerned which could be detected by observations covering more than half a century." The fulness of the account of this remarkable group, and of the numerous supposed discoveries of stars within the trapezium, furnish an excellent example of the thoroughness which is so characteristic of Mr. Burnham's work. With reference to the very faint star discovered within the trapezium by Mr. Alvan E. Clark soon after the telescope was erected, he writes: "It is a difficult object with the 36-inch, and certainly has never been seen before, notwithstanding the numerous alleged discoveries with telescopes down to three or four inches aperture. Not less than a dozen of these imaginary stars have been distributed about the interior of the trapezium."

To the average astronomer, the star 95 Ceti would probably not be of absorbing interest, but to Mr. Burnham it is "the most mysterious and strange double star in the heavens." The companion was discovered by Clark with a 7½-inch, was subsequently measured by Dawes in 1854, and by Burnham with some difficulty in 1888, since when he has not been able to see it even with the 36-inch.

Mr. Burnham finds that "none of the stars which have been supposed from spectroscopic observations to be close doubles, have shown any evidence of the fact when examined with the large telescope under the most favourable conditions." He then goes on to say that "it is possible some other explanation will be found for the recurrent phenomenon first discovered by Miss Maury in the Harvard spectrum photographs. At all events, it is hardly worth while, until the method has been verified upon some of the numerous known pairs suitable for this purpose, to consume the valuable time of the great telescope in a further examination of objects of this class." One would almost imagine that Mr. Burnham had failed to grasp the fact that the separation of the component stars in such cases, by the spectroscopic method, is solely due to their relative velocity, which in ordinary pairs is relatively small. At any rate, it has been estimated that a telescope of sufficient dividing power to separate the components of β Aurigæ must have an aperture, not of three, but of eighty feet!

Limitations of space forbid further reference to the rich feast which Mr. Burnham has provided; the value of much of his work will probably be only fully realised by astronomers of another age, but at the same time a large proportion of his results are of the greatest immediate interest and value.

Vol. iii. of the "Publications" consists of Prof. Weinek's now well-known selenographical studies; a report on specimens of glass similar to those used in the construction of the great object-glass; an investigation of the glass scale of the measuring engine; and Prof. Keeler's observations of the spectra of nebulæ.

It comes as a surprise to us to learn from Prof. Holden's introduction to this volume, that the work of the Lick Observatory is not without danger of suffering for want of funds. Even so small a matter as a suitable instantaneous shutter "could not be constructed until the summer of 1893, for lack of funds and of skilled workmen." In the early stages of the lunar photographic studies, we are also informed that the work would have been seriously interrupted had not the Smithsonian Institution come to the rescue with "several small appropriations of money." The appearance of the present volume has been made possible by the generosity of Mr. Walter W. Law, of New York City, in providing funds to cover the whole cost of producing the fifteen magnificent plates of the moon which embellish its pages. They are modestly described as "a gift to science," and they afford another example of the practical sympathy with astronomical inquiries displayed by so many of our American cousins.

Few will be inclined to deny the great value of the lunar photographs which have been taken at the Lick Observatory, and it is a matter for congratulation that the astronomical world has so soon been made acquainted with the first-fruits of their investigation.

Prof. Holden tells us that it was quite impossible to undertake the investigation of the negatives at the Lick Observatory, owing to the limited staff, and they were therefore placed freely at the disposal of Prof. Weinek, "whose previous experience in lunar observations and in photography, as well as his very unusual artistic skill, made his advice and assistance of extreme value."

No pains have been spared to make the study of the objects selected as complete as possible. As an instance we may mention that Prof. Weinek's drawing of Copernicus, enlarged twenty times from the negative, represents the great labour of 224¾ hours, and is described by Prof. Holden as "a monument of skill and patience."

It is proposed that a complete map of the moon, on a scale of 3 feet to the diameter, shall eventually be made, though the practicability of making a map on four times the scale is demonstrated by an enlargement of Tycho. The photograph of the Lunar Apennines, on the 3-foot scale, reproduced in Fig. 1, is a magnificent example of a camera enlargement from one of the negatives.

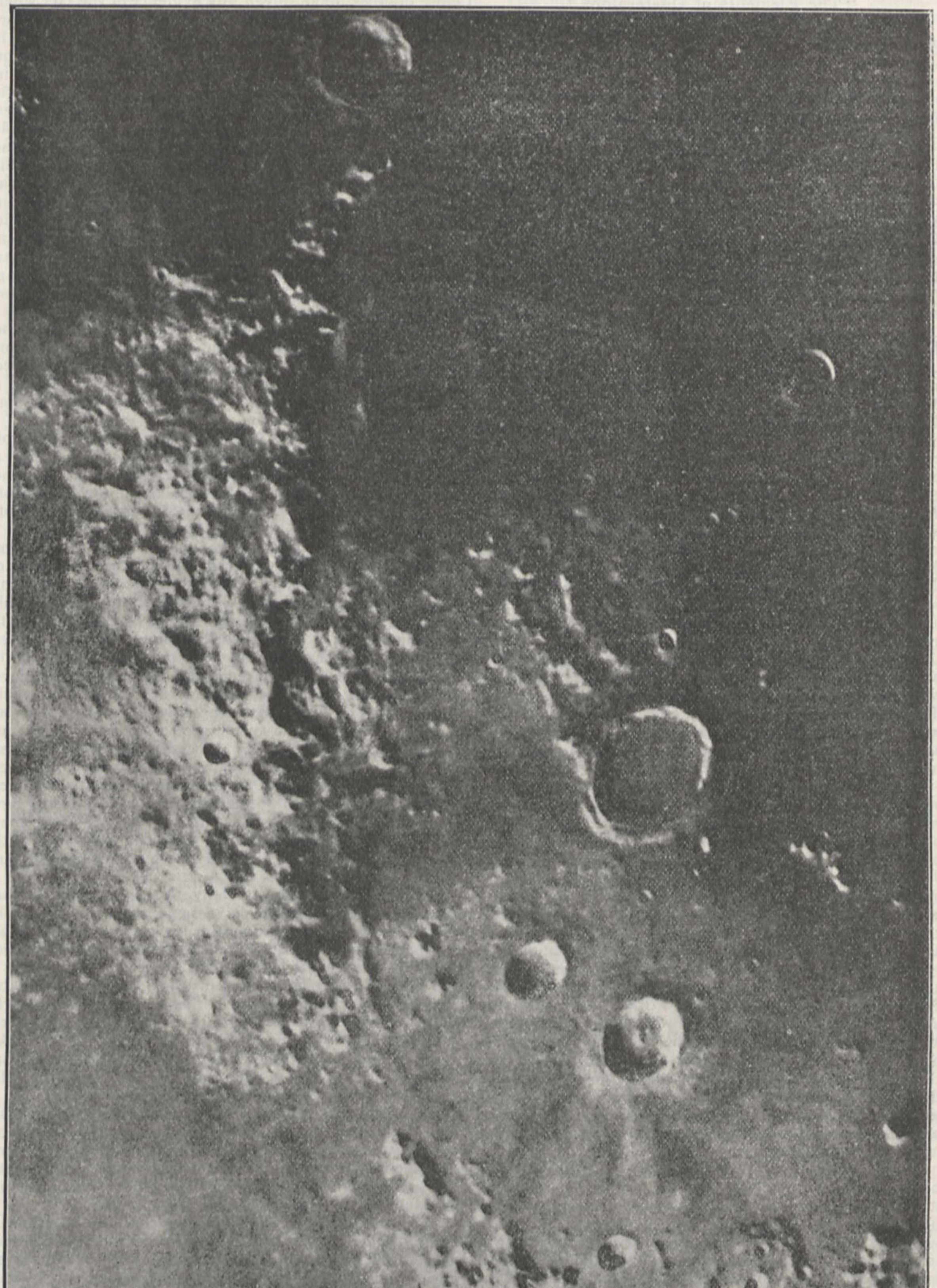

FIG. 1.—The Lunar Apennines, photographed at the Lick Observatory.

Several points of great interest are touched upon by Prof. Holden, among which is a brief discussion of the dimensions of the smallest object on the moon which can be registered on the photographic plate by the 3-foot refractor. From this we learn that a crater on the moon which is less than one-tenth of a mile in diameter will form an image which is about the same size as the grains of silver in the photographic film, and cannot in general be distinguished. Craters not more than 0·3 and 0·15 English miles in diameter, however, have been detected already. Prof. Holden concludes that for further advances in lunar photography it will be necessary to employ plates of greater sensitiveness so as to shorten exposure, and also plates in which the grain is finer. Workers in all departments of celestial photography have felt the need of such improvements, and, as Prof. Holden remarks, "future improvements depend more upon the manufacturer of plates than upon the astronomer who uses them."

Prof. Weinek's concise descriptions of the lunar formations figured in the volume, and his account of the new features so far discovered, leave nothing to be desired. Observers of the lunar surface may take consolation in the fact that even yet they are not in danger of being entirely superseded by photographic methods, for, as Prof. Weinek points out, "both methods must be perfected, and each must support the other." It is worth remark here, however, that enlargements recently made of lunar photographs taken at the Paris Observatory seem to mark a clear step towards perfection. (See page 207.)

Prof. Keeler's work on the spectra of nebulæ during his connection with the Lick Observatory, may fairly be said to mark the commencement of a new era in the history of the spectroscope as an instrument of precision. The observations were undertaken in the first instance at the suggestion of Dr. Huggins, who appealed to the Lick astronomers in 1890 in connection with the discussion as to the origin of the chief nebular line. It was found possible to use the third and fourth order spectra of a grating spectroscope with advantage, and even then the spectra were "by no means extremely feeble." Former work left the wave-lengths of the nebular lines uncertain to at least two tenth-metres, but the uncertainties now amount to only a small fraction of a tenth-metre. Further, it is claimed that the observations of the nebulæ have shown the existence of errors in Ångström's scale and in the wave-lengths of the reference lines, so that the observations did not become consistent until more reliable reference wave-lengths were determined by Prof. Rowland. As an example of the accuracy attainable, the velocity of Venus in the line of sight was found to be 6·4 miles per second at a time when the computed velocity was 7·69 miles.

It is not a part of our present purpose to discuss the origin of the chief line in the spectrum of the nebulæ, but we may say that Prof. Keeler does not favour the suggestion that it is due to magnesium; but, on the other hand, his measures definitely decide against the nitrogen origin of the line.

After all corrections have been applied, the normal positions of the first and second lines in the nebular spectrum are stated to be $5007{\cdot}05 \pm 0{\cdot}03$ and $4959{\cdot}02 \pm 0{\cdot}04$ respectively, and neither of the lines is represented among the Fraunhofer lines which appear in Rowland's photographic map. Indeed, we are not aware that either of these lines has ever been recorded as an absorption line in the spectrum of any celestial body whatever.

The observations have not been entirely limited to the determination of the position of the chief line. It has been found, for instance, that "the nebulæ are moving in space with velocities of the same order as those of the stars. Of the nebulæ observed, that having the greatest motion of approach, 40·2 miles per second, is G.C. 4373; that having the greatest motion of recession, 30·1 miles per second, is N.G.C. 6790. Most of the nebulæ have considerably smaller velocities than these."

It might well be imagined by anyone who has seen a photograph of the Orion nebula that the different parts would have a relative movement with regard to each other. Such, however, does not appear to be the case, according to Mr. Keeler; or, at least, there is no relative movement greater than four or five miles per second. Attempts to measure the velocity of rotation of the large planetary nebula G.C. 2102 showed that there was no radial motion greater than eight miles per second.

A study of the spectra of the nuclei of the planetary nebulæ has led Prof. Keeler, as it has independently led Prof. Pickering, to the conclusion that they are very closely connected with the bright-line stars, and thus the latest and most precise work goes to confirm one of the fundamental points of Mr. Lockyer's meteoritic hypothesis.

With reference to the discordant accounts of the spectrum of G.C. 826, to which attention was drawn by myself in 1889 (NATURE, vol. xli. p. 163), it is stated that Dr. Huggins's observation of a continuous spectrum in 1864 "was evidently a mistake," the spectrum being of the usual bright-line type.

Apparently in order to reconcile the presence of a continuous spectrum in such a nebula as that of Orion with the idea that masses of rarefied gas were alone in question, it has been suggested that this continuous spectrum may really be a large number of adjacent bright lines. The enormous dispersion employed by Prof. Keeler, however, fails to resolve it into lines, and thus Prof. Tait's suggestion as to the meteoritic constitution of nebulæ still stands as the best explanation of the spectrum.

Many other points of interest are raised by Prof. Keeler's admirable work, but sufficient has been said to indicate the progress which has been made in this branch of celestial physics and chemistry. Although Prof. Keeler has now removed to the Allegheny Observatory, his successor at the Lick Observatory—Prof. Campbell—has already shown himself to be fully capable of maintaining the spectroscopic department of the Observatory at the same high standard of efficiency.

A. FOWLER.

STUDIES OF A GROWING ATOLL.

THE researches of the surveying ships of the British Navy have from time to time rendered services to science no less important than those which it is their function to perform for navigation. It has become an established practice to encourage the surgeons of these vessels to undertake scientific investigations in the leisure which their professional duties frequently afford, and facilities are sometimes given for a competent man to continue such work by allowing his transference to another vessel when his own has to leave the place where he has been working. For this the Admiralty deserves credit and the thanks of those who desire to see her Majesty's ships maintaining the position they took up in the days of Cook, and continued through the voyage of the *Beagle*, and the long line of expeditions which followed it, to the voyage of the *Challenger*. While it may not be too much to hope for a renewal of special marine research by the Royal Navy before private enterprise reaps the waiting scientific harvest of the unknown Antarctic, we feel that too much prominence cannot be given to the good work done incidentally in the course of routine surveys.

The hydrographer, Captain Wharton, in his preface to the reports of Mr. Bassett-Smith on the Macclesfield

Bank,[1] expresses the general result of this piece of research very clearly and concisely, showing that its value is fully recognised by the authorities at the Admiralty.

The Macclesfield Bank is a shallow patch, rising abruptly from deep water in the middle of the China Sea, crossed by the parallel of 16° N. and frequently passed by vessels. It is of an oval shape, about 80 miles long and 30 wide, with a general depth of about 40 fathoms. Reports having been made of very shallow water on the edge of this bank, forming a possible danger to shipping, a complete survey was resolved upon, and as preliminary soundings had shown indications of a raised rim, instructions were given to pay special attention to the animal life upon what might turn out to be an atoll entirely beneath the surface of the sea. Half of the reef was surveyed by Captain Moore in the *Penguin* in 1892, and collections made by means of dredges and divers, under the superintendence of Mr. Bassett-Smith, several tons of specimens being subsequently despatched to the Natural History Museum for full study. The remainder of the bank was examined in 1893 by Captain Field in the *Egeria*, to which ship Mr. Bassett-Smith had exchanged in order to continue his work, and the result is such an investigation into the biological conditions of a submerged coral reef in mid-ocean as has never been made before.

The whole circumference of the bank rises as a ring of coral to within from 9 to 15 fathoms of the surface, being broken here and there by wide gaps of greater depth, but never so deep as the central depression, which varied generally from 40 to 48 fathoms. The minimum depth on the rim was 6½ fathoms, and an isolated shoal rising from the centre of the inner depression reached to within 5 fathoms of the surface.

The uniformity of the depth appears to Captain Wharton to be a strong argument against any movement of the bottom since the period when the atoll form was assumed; and he shows that the simple growth of coral on the rim will in time suffice to produce a perfect ring-shaped coral island without the aid of subsidence or upheaval. It appears, in fact, that here is an atoll in course of formation on a foundation sufficiently near the surface to allow coral to grow. Such a foundation Darwin admitted might allow a coral island to form without subsidence, and the recently discovered abundance of similar elevations in the tropical oceans is one of the main arguments for Murray's general theory of coral growth.

Mr. Bassett-Smith's first day's dredging convinced him that the Macclesfield Bank was by no means a "drowned atoll," but on the contrary that it was very much alive. The basis of the bank appeared certainly to be dead coral-rock, or in many places a calcareous rock composed of the consolidated vegetable organisms which seemed most common between the depths of 20 and 50 fathoms. Upon this ground corals grew in great patches, and other forms of life were very abundant, especially echinoderms, molluscs, crustaceans, and annelids; many very striking cases of mimetic resemblances were observed amongst them. Altogether forty-one genera of corals were dredged, excluding alcyonarian and hydroid corals; twenty-nine genera occurred between 25 and 35 fathoms, and twenty-seven genera in deeper water. It appears that reef-building corals can thrive at depths as great as 50 fathoms in the conditions of the Macclesfield Bank, where the water is very clear and warm. Concerning the genera represented, Mr. Bassett-Smith says:—

[1] "China Sea. Report on the Results of Dredgings obtained on the Macclesfield Bank, in H.M.S. *Rambler*, Commander W. U. Moore, R.N., April 1888, and H.M.S. *Penguin*, Commander W. U. Moore, R.N., April 1892, and H.M.S. *Egeria*, Commander A. M. Field, R.N., April 1893." By P. W. Bassett-Smith, Esq., Surgeon R.N. (London: Printed for Her Majesty's Stationery Office, 1894.)

"The most universally distributed were *Seriatopora*, *Pavonia* (especially a variety of forms very nearly allied to *Mycedium elegans* of Milne Edwards), *Leptoseris Montipora*, and *Stylophora Güntheri*, at all depths, but the sections 'Madreporaria Fungida' and 'Perforata' are undoubtedly most frequently met with in depths over 20 fathoms, and continued down to between 40 and 50 fathoms; the corallum being almost always light and delicate. *Agavicia*, *Phyllastræa*, *Pachyseris*, *Turbinaria*, and *Leptoseris*, in cups of varying size, from two inches to twenty inches across; *Oxypora*, *Pavonia*, *Hydrophora*, *Scaphophyllia*, and *Montipora*, in leaf-like expansions; *Cyphastræa*, *Galaxea*, *Turbinaria* and *Montipora*, in encrusting growths; or in branching forms, as *Seriatopora*, *Mussa*, *Madrepora*, *Psammocora*, *Napopora*, *Anacropora*, *Alveopora*, and *Rhodaræa;* the most massive forms found in deep water being *Pocillopora*, *Stylophora* and *Mussa*. On the sandy bottom of the lagoon, and near the rim, the corals that seemed to thrive best were small branching forms of *Psammocora*, and *Anacropora;* delicate frond-bearing clumps of *Pavonia; Leptoseris* cups, thick but light spreading branches of *Alveopora*, *Montipora;* and many simple corals as *Cycloseris*, *Fungia*, &c. Small fragments of more massive *Astræa* were brought up three times from deep water, twice from 30 to 40 fathoms, and once from 40 to 50 fathoms."

There was a strong current over the rim, even in calm weather, and the surface water, the temperature of which was sometimes as high as 88° F., swarmed with Plankton.

In addition to the chart of the bank showing its unmistakable atoll form, the blue-book contains two sections of the outer slope on a natural scale. The angles varied somewhat on different sides. On the north the slope was gradual, the 100-fathom line being one mile distant from the 20-fathom line, while 200 fathoms was only found ten miles farther out, beyond which the slope became more rapid to 1100 fathoms six miles beyond. On the east there was a much steeper slope, the 100-fathom soundings being found half a mile from the 20-fathom and 300 at the distance of a mile, while fifteen miles away the depth was 2100 fathoms. The wall-like spring of the bank from the ocean floor is still more striking on the south, where depths of 150 fathoms occur half a mile from the edge of the bank, 300 fathoms at the distance of one mile, and the oceanic depth of 1100 fathoms, only 3½ miles away, giving the remarkably high average slope of 1 in 3. The shoal at the north end of the future island is attributed to the strong current from the south-west sweeping the débris over the edge of the oceanic hill into the deep beyond.

The observations fully confirm Dr. Murray's preference for the term "organic" rather than "coral" as applied to the origin of atolls, for a very large part of the growing rock was shown to be due to calcareous algæ, to corals other than reef builders, and to the accumulation of the calcareous remains of crustacea, mollusca and annelids. Mr. Bassett-Smith suggests that the crust of algæ prevents the dissolved carbonic acid of the sea-water from touching the dead coral rock below, while the action of the growing algæ might decompose the carbonic anhydride. This we are inclined to doubt, as the decaying organisms would seem likely to produce far more carbonic acid than could be disposed of by the very feeble daylight which reaches depths approaching 40 fathoms; and from the continual dredging of "rotten rock" in the central depression, we feel inclined to think that active life and rock-growth are taking place there only in restricted patches. The observations seem to leave no doubt that the atoll is growing towards maturity and the air, not declining from a past existence as an island.

HUGH ROBERT MILL.

NOTES.

PROF. GEORGE FORBES, F.R.S., who has for the last three years been engaged on the utilisation of Niagara Falls, has, we understand, just returned to this country from the United States, the construction stage of the work being now completed. The close of the three years of Prof. Forbes's connection with the great work at Niagara Falls, which marks the change from the period of design and construction to the period of commercial activity in the existence of the Niagara Falls Power Company, forms a fitting opportunity for expressing the sense of gratification that all Englishmen, and the scientific world in particular, must feel in having had one of their countrymen chosen to undertake the important and difficult duties of electrical consulting engineer to an undertaking of such magnitude. The manner in which those duties have been discharged, and the pioneer services which Prof. Forbes has rendered them in respect of the work, have, we are in a position to say, received most gratifying testimony and cordial acknowledgment from his Company, who recognise in Prof. Forbes the scientific attainment, combined with independence of thought and action, which have been invaluable throughout the stage of operations now completed. Few realise the many novel conditions that have had to be met at Niagara Falls. But by dint of years of study of the many problems presented, Prof. Forbes has evolved a system which for completeness, adaptability, and security against breakdowns, had not been dreamt of before. The adoption of the alternating current before its value was fully realised by others, the initiation of the world in the use of a lower frequency than any that has hitherto been employed, and the remarkable confirmation of the foresight as to the economy of large transformers at high electric pressure, even at the low frequency employed, that has been established, are matters for congratulation. Although for fuller particulars of the system and apparatus employed, we must refer our readers to a previous account (NATURE, vol. xlix. p. 482), we may draw attention to the method by which Prof. Forbes met one of the most troublesome questions in connection with the design of the 5000-horse power generators (Fig. 1), arising in consequence of some requirements of the turbine designers—viz. the securing of a certain necessary momentum of the revolving part of the dynamo, without increasing the weight to be supported by the hydraulic piston in the turbine above a certain limit. The difficulty was met by fixing the armature, and revolving the field-magnet, formed of a nickel-steel ring with the pole-pieces pointing radially inwards, outside, the ring, or yoke, and the pole-pieces being supported by a bell-shaped cover fixed rigidly to the top of the vertical shaft from the turbine, the shaft being supported by bearings in the interior of the fixed armature. The foreseen ability to convert the alternating current into continuous current, and the low frequency into high frequency, which is

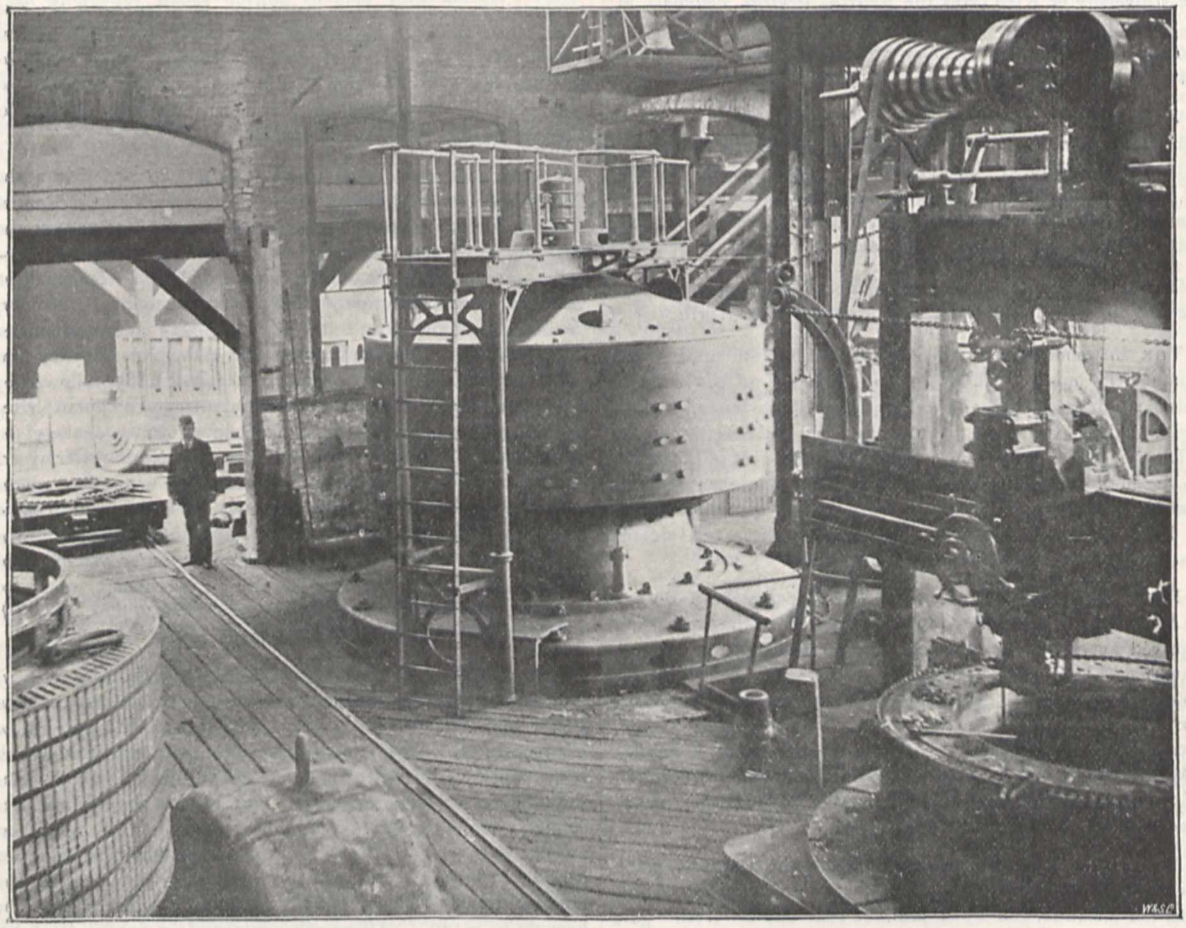

FIG. 1.

now realised in the invention of Messrs. Hutin and Leblanc, gives greater elasticity to the system; while the precautions taken to avoid sudden opening or closing of the circuits, gives a security against troubles experienced in the past by others such as has not obtained before. The success that has attended Prof. Forbes's efforts during the period of design and construction is of good augury for a successful issue to the commercial stage which the Niagara Falls Power Company now enters upon.

Reuter reports that a violent earthquake shock, lasting one minute, was felt at Oravicza, South Hungary, at 10.35 p.m. on December 19. Many houses fell in, while the walls of others were seriously cracked.

A Bacteriological Institute is about to be established in the University of Kieff (says the *British Medical Journal*), at an estimated cost of £10,000. A well-known druggist of Moscow has also given a house, valued at £3000, with £500 towards the fitting up of it as a bacteriological laboratory.

We regret to record the death, on the 19th inst., of Prof. Allen Harker, of the Royal Agricultural College, Cirencester, at the age of forty-six. Mr. Harker was a popular and successful teacher, and did good work, not only at the College, but in connection with County Council technical education schemes, both in Gloucestershire and Bedfordshire.

The Italian Botanical Society has decided to hold its annual meeting for 1895 in Palermo, in the latter part of April.

The influence of the Royal Gardens at Kew upon other Botanic Gardens is strikingly shown in a list of the staffs of botanical departments and establishments at home, and in India and the Colonies, given in the *Kew Bulletin* (Appendix III.). Disciples have gone from Kew to the ends of the world to become directors or curators of Botanic Gardens; indeed, almost every Garden seems to have on its staff someone trained at Kew, or recommended by the Director there. A clearer testimony of esteem could not be desired.

From Mr. Carruthers's Report of the Department of Botany in the British Museum for 1893, we learn that the herbarium received some valuable additions during that year by gift and purchase. The most important were Mr. Deby's great collection of diatoms, numbering nearly 30,000 named slides; the late Mr. Jenner's collection of over 6000 specimens of algæ, a collection of over 1000 of the lower cryptogams of Dominica and St. Vincent, presented by the committee for the exploration of the West Indies; and a large number of flowering plants from Malaya, presented by Mr. Ridley.

A rapid fall of the barometer on Friday, the 21st inst., made it clear that a serious disturbance was approaching our shores from the Atlantic, and by 6 p.m. of that day a "fresh" gale had already set in on the north-west coast of Ireland. During the night the centre rapidly crossed Scotland, and the whole of the United Kingdom experienced severe westerly gales, which occasioned great loss of life and property, and although the storm area subsequently crossed the North Sea, violent north-westerly winds continued during the whole of Saturday. The anemometer at Greenwich on Saturday morning showed a pressure of 29 pounds on the square foot, which is equivalent to a velocity of about 76 miles in the hour; but considering that the centre of the disturbance was at that time at least 400 miles to the north, there is no doubt that considerably higher velocities occurred in other parts of the country. In Scotland the barometer fell more than an inch and a half in 24 hours, and the subsequent rise was even more rapid.

We have received two numbers of *Biology Notes*, a monthly pamphlet published by the Technical Instruction Committee of the Essex County Council. Under the direction of Mr. Houston, the problem of reconciling the practical requirements of this district with a really scientific method of instruction in biology seems to be in rapid progress towards a satisfactory solution. Pioneer lectures are given, short courses for farmers and gardeners on such immediate topics as plant diseases, and systematic and largely practical courses of study in botanical science, at the Chelmsford Laboratory, and at various local centres. In addition, the Chelmsford Laboratory is rapidly becoming a centre for inquiry into, and the discussion of, agricultural problems. During the last three months the influence of ergot on gravid cattle, the toxic effect of bracken fronds, certain samples of foreign hay that had caused disease, and the distribution of potato disease, among other topics, have received attention. The combination of elementary instruction in science, on the one hand, with original inquiry on the other, and the practical simplicity of both departments of work, appear to us to be admirable features, and we would recommend it to the attention of those who are interested in Technical Education in similar districts in other parts of the country.

Field experiments of an instructive kind are carried on at the Royal Agricultural College, Cirencester, under the direction of Prof. E. Kinch. A pamphlet just received contains an account of experiments on oats grown this year on twenty-four plots differently treated. The general results were as follows:—The smallest yield of corn was from the plots receiving ammonium salts only, the unmanured plots, and the plots receiving cinereal manures only. The highest yield of corn was from the plots receiving fourteen tons of farmyard manure every year since 1885, followed by the plots receiving cinereals and mineral nitrogen, and phosphates and mineral nitrogen. The largest amount of straw was also given by the plots which had received the larger amount of farmyard manure annually, followed by those receiving mineral nitrogen with phosphates. The withholding of potash appeared to make little or no difference in the yield of straw, but the withholding of phosphates made a difference. The unmanured and cinereal manured plots gave the least straw. Experiments were also made to determine the crops of hay from twenty plots, to which different kinds and quantities and manures had been applied.

The last issue of the *Zeitschrift für Physikalische Chemie* (vol. xv. part 3, p. 386) contains a paper by Messrs. Raoul Pictet and Altschul on "Phosphorescence at very Low Temperatures," which is of special interest in connection with recent work on this phenomenon. Glass tubes containing the sulphides of calcium, strontium, and barium were exposed to sunlight, and the duration and the extent of the phosphorescence were noted. The various tubes, after having been again exposed to sunlight, were plunged into liquid nitrous oxide, the temperature of which, by rapid diminution of pressure, could be brought to $-140°$. After twelve minutes' immersion the tubes were brought into a dark room, and their behaviour carefully observed. At first no indication of phosphorescence could be seen. In a few moments the upper part of the tube, which had not been so strongly cooled as the rest, began to phosphoresce, and gradually the feeble light seemed to spread itself down the tube, the lower part of which, however, glowed much more feebly than the upper. After five minutes the tubes acquired their ordinary vivid colour, without subsequent exposure to sunlight or even to diffused daylight. All phosphorescent substances appeared to behave in this way. Further experiments were then made in order to determine the limits between which these phenomena occurred. For this purpose a quantity of alcohol was cooled to $-80°$, and in it a tube containing some phosphorescent substance, after insolation, was immersed. That portion of the tube which was surrounded by alcohol in the outset glowed feebly, but in proportion as it took up the temperature of the cooled liquid its phosphorescence diminished, until at -65° it entirely disappeared. The portion of the tube above the alcohol continued to phosphoresce strongly. After thirty minutes' immersion in the cooled alcohol, the tube was removed, and as it gradually acquired the temperature of the air, the lower portion began to glow. Before the glow—blue, green, or orange in colour, depending on the nature of the metallic sulphide—entirely disappeared, the colour became a faint yellow. It was found by comparative experiments that the alcohol exerted no specific influence on the results. These seemed to be entirely dependent upon the diminution or total cessation of molecular vibrations at the low temperature.

AN interesting paper on the Sicilian earthquakes of last August has recently been published by Dr. Mario Baratta (*Boll. della Soc. Geogr. Ital.*, Ott., 1894). The first shock of the series was felt on July 29 at Randazzo, and was succeeded by several other slight shocks, mostly in the Lipari Islands. Then came the severe earthquake of August 7 at 12h. 58m. p.m., and the still stronger one of August 8 at 5h. 16m. a.m. (Greenwich mean time). These affected chiefly the south-eastern slope of Etna, and were followed by more than twenty shocks in the same district, lasting until August 26. The meizoseismal area of the principal earthquake (August 8) is only about 7 km. long and 3 to 4 km. broad, and, as the intensity diminished rapidly outwards, it would seem that the focus cannot have been far from the surface. Moreover, the longer axis of this area runs north-west and south-east, and, when produced, passes through the central crater of Etna. It therefore probably coincides with a radial fissure of the cone, and indeed is not far, if at all, distant from that along which the eruption of 1329 took place. The pressure exerted by the column of lava in the central funnel, or by the forces which have raised it to its present height, may have caused such a fracture to be reopened. Thus, it is not impossible that the recent earthquakes indicate an unsuccessful attempt at a new lateral eruption.

FURTHER details relating to the same earthquakes are given in the *Bollettino Meteorico* (Suppl. 110) of the Geodynamic Office of Rome. The depth of the focus of the principal earthquake, according to Prof. Riccò, was about 4 km. The pulsations were recorded at Rome by the great seismograph, consisting of a pendulum 16 metres long, with a mass of 200 kg.; the first traces at 5h. 17m. 30s., and the principal maximum at 5h. 18m. 55s. The puteometer of the Observatory of Catania shows a trace about $21\frac{1}{2}$ mm. long, indicating a temporary lowering of the well-water, which, in returning, stopped about 4 mm. below its original level.

THE additions to the Zoological Society's Gardens during the past week include a Yellow Baboon (*Cynocephalus babouin*, ♀) from Fort Salisbury, South Africa, presented by General Owen Williams; two Grisons (*Galictis vittata*) from Brazil, presented by Mr. H. A. Catlett; a Song Thrush (*Turdus musicus*), a Goldfinch (*Carduelis elegans*), British, presented by Mr. B. M. Smith; a Grenadier Weaver Bird (*Euplectes oryx*, ♂) from West Africa, presented by Lady McKenna; a Wild Cat (*Felis catus*) from Scotland, deposited; five Shore Larks (*Otocorys alpestris*), British, purchased.

OUR ASTRONOMICAL COLUMN.

ADVANCES IN LUNAR PHOTOGRAPHY.—MM. Lœwy and Puiseux recently communicated to the Paris Academy a paper on photographs of the moon, taken at the Paris Observatory, by means of the great Condé equatorial. Some of the photographs have been enlarged by Dr. Weinek, and the enlargements seem to have excelled in beauty and detail previous lunar pictures of a similar kind. An examination of the photographs shows that not only can they be used to verify the general features of the moon's surface, as depicted upon the most recent and complete lunar maps, but they also show a number of details and small craters which so far have been omitted from such maps. There are, of course, a number of causes which prevent a single photograph from being an ideal representation of a celestial object, and enlargements are usually regarded with a certain amount of suspicion, for there is always a possibility that interesting formations will be unconsciously manufactured in the process. MM. Lœwy and Puiseux know this as well as anyone; nevertheless, they find that the enlargements undoubtedly reveal new features, and definitely determine the existence of several contested objects. They think an instrument of long focus is essential for the best results, and that the enlargements should not be carried beyond twenty or thirty diameters. One object upon which the photographs have thrown light is the small isolated crater Linné, situated in the middle of the Sea of Senenity. According to Shroeter, Beer, Maedler, Lohrmann, and other selenographers, this crater was distinctly visible up to 1866, when Schmidt announced its disappearance. It was afterwards discovered again, but was much smaller than when described and figured by Beer and Maedler. Dr. Weinek finds that the object appears upon a plate taken on March 14, but only one kilometre in diameter—that is, about one-tenth the value assigned to it by the earlier observers. The crater has also been found on other plates, and Sig. Schiaparelli has testified to its reality. Four new objects—three craters, and the fourth an isolated elevation of some kind—have been found in the plain which extends to the south of Ariadaeus, between the bright crater-plain Cayley and the Silberschlag crater. Ten new craters can be detected in the typical walled plain Albategnius. All the rills observed to the west of Triesnecker can be seen to extend beyond the limits previously assigned to them, and to connect Ariadaeus, Hyginus, and Triesnecker with interlacing clefts. Judging from these results, we cannot but conclude that the photographs represent real advances in lunar photography.

COMETARY EPHEMERIDES.—The following ephemeris for Encke's comet is in continuation of that given on November 22, and is due to Dr. O. Backlund. M. Schulhof's ephemeris, in the *Astronomische Nachrichten*, No. 3267, is used for Swift's comet:—

	ENCKE'S COMET. *Ephemeris for Berlin Midnight.*		SWIFT'S COMET. *Ephemeris for Paris Midnight.*	
1894.	R.A. (app.) h. m. s.	Decl. (app.) ° ′ ″	R.A. (app.) h. m. s.	Decl. (app.) ° ′ ″
Dec. 28 ...	22 14 57	... +3 35 18	0 3 23	... −0 20 22
30 ...	14 27	... 3 19 58	0 8 33	... 0 17 53
Jan. 1 ...	13 52	... 3 3 38	0 13 41	... +0 53 45
3 ...	13 9	... 2 45 51	18 46	... 1 33 10
5 ...	12 16	... 2 26 7	23 49	... 2 10 9
7 ...	11 8	... 2 3 47	28 51	... 2 46 41
9 ...	9 42	... 1 38 1	33 50	... 3 22 44
11 ...	7 52	... 1 7 53	38 48	... 3 58 19
13 ...	5 31	... +0 32 10	43 43	... 4 33 24
15 ...	2 32	... −0 10 34	48 37	... 5 8 0
17 ...	21 58 47	... 1 2 10	53 29	... 5 42 5
19 ...	54 3	... 2 4 42	58 19	... 6 15 42
21 ...	48 10	... 3 20 29	13 8	... 6 48 48
23 ...	40 57	... 4 51 53	7 55	... 7 21 24
25 ...	32 13	... 6 40 45	12 41	... 7 53 29
27 ...	21 57	... −8 47 28	17 26	... 8 25 4

It will be seen from these ephemerides that the two comets are in the same region of the sky, both being a few degrees south of Pegasus. Observations of the comets are greatly needed.

RUSSIAN ASTRONOMICAL OBSERVATIONS. — The latest *Bulletin* (vol. xxxv. No. 4) of the Imperial Academy of Sciences at St. Petersburg is almost entirely devoted to astronomical papers. E. Lindemann contributes a discussion of the visual and photographic magnitudes of Nova Aurigæ, and gives a light-curve extending from December 10, 1891, to April 13, 1892. N. Nyren discusses the observations made at Pulkova with the vertical circle, between 1882 and 1891, from the point of view of variations of latitude. The curves derived from the observations indicate that the interval between two maxima is 433 days, and between two minima, 434 days. As to the amplitude of the variation, though no definitive result is stated, the value of the radius of the circle described by the instantaneous pole appears to be 0″·145, and the direction of motion from west to east. Another paper on the same subject is contributed to the *Bulletin* by S. Kostinsky. In this case, the observations discussed were made with the great meridian instrument of the Pulkova Observatory, mounted in the prime vertical. The period obtained was 411 days, and the amplitude 0″·541. In addition to these papers, there is one on the orbits of Bielid meteors, deduced by M. Bredichin from observations made in 1892.

ON A REMARKABLE EARTHQUAKE DISTURBANCE OBSERVED AT STRASSBURG, NICOLAIEW, AND BIRMINGHAM, ON JUNE 3, 1893.

INTRODUCTORY NOTE.

THE Horizontal Pendulum.—The observations described in the subjoined article were made with the horizontal pendulum designed by Prof. Zöllner, and modified by Dr. von Rebeur-Paschwitz. This instrument consists of three thin brass tubes jointed together in the form of an isosceles triangle, the vertical angle of which is about 45°. The two equal sides are prolonged slightly beyond the base, and to the ends are attached two small spherical agate cups, the concavity of the lower one being directed from the centre of gravity of the pendulum, and that of the upper one towards it. When the pendulum is placed in position, these cups rest on two steel-points attached to the stand of the instrument and directed normally to the surfaces of the agate cups. One steel-point is almost exactly above the other, so that the axis of rotation is nearly, but not quite, vertical, its inclination to the vertical being still great compared with the movements of the ground we wish to investigate. The pendulum rests in the vertical plane passing through the axis of rotation, and on the side towards which it inclines. If this is towards the east, and if the axis is slightly tilted in the east and west plane, there will be no deflection of the pendulum; the only change will be in its sensitiveness. But if the axis is tilted in any other plane, it will no longer incline towards the east, and the pendulum will be deflected from its original position, in order to remain in the same vertical plane with the axis of rotation. It is evident that the smaller the original inclination of the axis to the vertical, the greater will be the deflection for a given tilt of the axis in the north and south plane; that is, the greater will be the sensitiveness of the pendulum.

From the middle of the nearly vertical tube of the pendulum, there projects outwards a small bar. Passing through an aperture in the frame to which the steel-points are attached, this bar carries a mirror, whose plane is at right angles to that of the pendulum. A ray of light, proceeding from a fixed source, is reflected by the mirror, and registers the movements of the pendulum on a strip of photographic paper wrapped round a revolving drum. The zero-line is traced by a ray of light reflected by a fixed mirror just below the other, and attached to the stand of the instrument.[1]

Observation of Earthquake Pulsations.—Nothing could show better than Dr. von Rebeur-Paschwitz's interesting paper how desirable it would be to have a few well-chosen stations in different parts of the world where these pulsations could be registered. They might then be traced as they spread out from the origin of a great earthquake, and might even be followed, as he suggests, in their course, completely round the world.

In several Italian observatories there are established instruments suitable for this purpose. Horizontal pendulums, with recording apparatus, are now at work at Charkow and Nicolaiew in the south of Russia; and two others will soon be ready at Strassburg and Merseburg in Germany. A bifilar pendulum[2] at Birmingham, belonging to the British Association, will shortly be furnished with a photographic recorder. Thus Europe is at present fairly well provided for.

A large number of stations in other parts of the world is by no means absolutely necessary. Results of great value would be derived if recording instruments were erected at places near the east and west coasts of North America, in South America, South Africa, India, Australia or New Zealand, and the Sandwich Islands. In Japan Prof. Milne's tromometer[1] leaves little to be desired.

The chief element to be determined is the exact epoch of the beginning, maximum amplitude, and end of the pulsations, or of each group of pulsations. The horizontal pendulum, Dr. von Rebeur-Paschwitz informs me, can be arranged so that its sensitiveness for slow tilts of the ground can be diminished without necessarily lessening its sensitiveness for earthquake shocks. The strip of photographic paper can thus be reduced in width without running any risk of the spot of light leaving the paper during its ordinary daily and other movements. Without increasing the expense, a more rapid movement of the paper could be permitted, and this would enable the determination of the time to be made with greater accuracy. Possibly, also, the construction of the instruments might be simplified if earthquake-pulsations are to be the principal subject of investigation. In the bifilar pendulum, for example, since the amplitude of the oscillations is a point of minor importance, the somewhat elaborate machinery for determining the angular value of the scale divisions might be dispensed with, and also the arrangements for readjusting the spot of light from a distance.

Hardly less important in these investigations is the determination of the exact time of occurrence of the earthquake at or near its centre of disturbance. But on this it is the less necessary to insist, for in so many of the more marked seismic districts there now exist organisations for the study of earthquakes. It may not be out of place, however, to suggest that in all seismic records, and in every part if periodically published, the standard time employed should be clearly stated. It is not universally known, for instance, that, in Japan, Tokio time was replaced on January 1, 1888, by the time of 135° E. long. In accounts from Beluchistan, again, we cannot be certain whether Madras time or railway time is meant, for both are used. The trouble of inserting this important detail is hardly to be compared with the confusion and error that may result from its omission.

C. DAVISON.

IN the last report of the Earth Tremor Committee of the British Association, reference is made to an observation of earth-pulsations by Mr. C. Davison on the evening of June 3, 1893, at Birmingham, which was obtained by the aid of Mr. H. Darwin's bifilar pendulum. I take the following details from the report:—At 5.43 p.m. (G.M.T.) the image was found to be perfectly steady, but at 6.29, when the observer returned to the cellar, it was moving slowly and steadily from side to side of the field of view, thus indicating the passage of a system of earth-waves. At 6.42 the image had come to rest, but at 6.46 the oscillations commenced again, and continued to be visible with varying amplitude until 8.13. After 8.13, though the observer watched for two hours and a half, no further motion was noticed. The period of the waves was found by a number of observations to be between fifteen and twenty seconds, and the range of motion at its maximum one-eighth of a second.

Mr. Davison's observation is especially interesting, because it corresponds exactly with a *very extraordinary disturbance* which was registered by the horizontal pendulums at Strassburg and Nicolaiew. Amongst the considerable number of disturbances common to both these places, that of June 3 is certainly the most prominent during the interval from January 1 to September 4, 1893. In the accompanying illustration (Fig. 1) the two curves, obtained by photography, are shown side by side; in correspondence with the difference of longitude between the two places, the lower curve was moved 17·5 mm. to the left. The pendulum in both cases was placed in the east-west plane. In the following notes the time is Greenwich Mean Solar Time, and is given in decimal parts of the hour.

(*a*) *Strassburg.*—The disturbance begins suddenly and small at 4·42, the curve having been perfectly sharp and steady before. The range of motion increases to 4 mm. at 4·52 and decreases at 4·69. It then again increases so as to make the curve disappear entirely between 4·77 and 5·05. During the interval the light-point was displaced by $3\frac{1}{2}$ mm. to the north, which corresponds with a deflection of the pendulum towards the south. At 4·82, the person who keeps control over the instrument entered the cellar, to look after it and to determine the time correction, which is done by shutting off the light during

[1] For a fuller account of the horizontal pendulum, see Dr. von Rebeur-Paschwitz's great memoir, "Das Horizontalpendel" (*Nova Acta der kais. Leop. Carol. Deutschen Akademie der Naturforscher*, Bd. lx. 1892, pp. 1-216); also Brit. Assoc. Rep., 1893, pp. 303-308.

[2] NATURE, (July 12, 1894), vol. 50, pp. 246-249; Brit. Assoc. Rep., 1893, pp. 291-303.

[1] Brit. Assoc. Rep., 1892, pp. 107-109.

a known interval of five minutes. He then locked the cellar, and when he returned at 8·45 he was obliged to make a correction,[1] because the light-point had left the paper. Unfortunately, he forgot to note down its exact place, but from the inspection of the curve it is evident that at 5·61, after a short interval of steadiness between 5·25 and 5·61, the pendulum received a sudden shock, which caused it to oscillate, and at the same time produced a deflection, by which the light-point was probably brought off the lower edge of the paper, from which it was distant 48 mm. at the time of the shock. There can be no doubt that such was the cause of the disappearance of the curve, for the base-line runs on perfectly undisturbed, which is a sign that the instrument continued to be in good working order, as usually. From 8·45 to 9·65 the motion is small; and from 9·65 till 11·16 the curve is nearly perfectly steady.

At 11·16 a new disturbance begins; the range of motion is very small at first, but increases to 5 mm. at 11·45, and to 10 mm. at 11·60; at 12·10 the disturbance, which is much like the first one, comes to an end, and is again followed by a steady part of the curve.

At 12·26 commences the last disturbance, which at 12·47 increases to 6 mm. Between 12·73 and 13·03 no traces of the curve are visible, and during this interval a displacement of 10½ mm. has occurred, which indicates a deflection of the pendulum towards the north. The motion continues to be visible until 14·45; the curve then resumes its nearly steady appearance, which is once again interrupted by small motion at 14·95.

(*b*) *Nicolaiew.*—The following details were communicated to me by Prof. Kortazzi, who informed me that on this day he went down into the cellar one half-hour later than usually, at 6·54, when he found that the light-point had passed from the paper on to the brass rod, which serves to clamp the paper, and was swinging considerably. From this reason the light-point could leave no traces on the paper between 5·95 and 6·62. The disturbance is very large and of long duration. It commences at 4·32 and reaches its first maximum at 4·80, when the range is >60 mm. Strong motion continues until 8·4. From the copy of the disturbance, which Prof. Kortazzi kindly sent me, and which is represented in the above figure, it appears that at about 5·77, or 11m. *before* the light-point was prevented to trace a curve, by passing on to the brass rod, the curve was suddenly interrupted, which shows that the pendulum was performing large oscillations. Between 9·72 and 11·05 the motion is small.

At 11·05 a new disturbance begins, which increases suddenly at 11·36, diminishes a little at 12·3, and increases again at 12·47 From 12·7 to 13·22 no traces of the curve are visible. At 13·9. the motion decreases considerably, and after another small increase at 14·87 reaches its end at 15·17.

The figure shows that the motion at Nicolaiew is much more considerable than at Strassburg. Whilst at the latter place the whole disturbance is divided into four distinct parts, which are separated by moments of nearly perfect steadiness, at Nicolaiew the first and second, as well as the third and fourth part, each form a continuous disturbance.

If we denote by V the relative strength of a shock in a direction normal to that of the pendulum, by a the range of motion, measured on the curve, by d and T the distance between the photographic drum and the pendulum mirror, and the period of oscillation of the pendulum, then we have the following relation between the observations in two different places:—

$$\frac{V_1}{V_2} = \frac{a_1}{a_2} \cdot \frac{d_2 T_2}{d_1 T_1}.$$

In the present case

$$d_1 = 1·8\text{m.},\ d_2 = 4·6\text{m.},\ T_1 = 17·0\text{s.},\ T_2 = 10·2\text{s.},$$

thus $\frac{d_2 T_2}{d_1 T_1} = \frac{47}{31}$. A shock of the same strength therefore produces at Nicolaiew a disturbance 1½ times as large as at Strassburg.

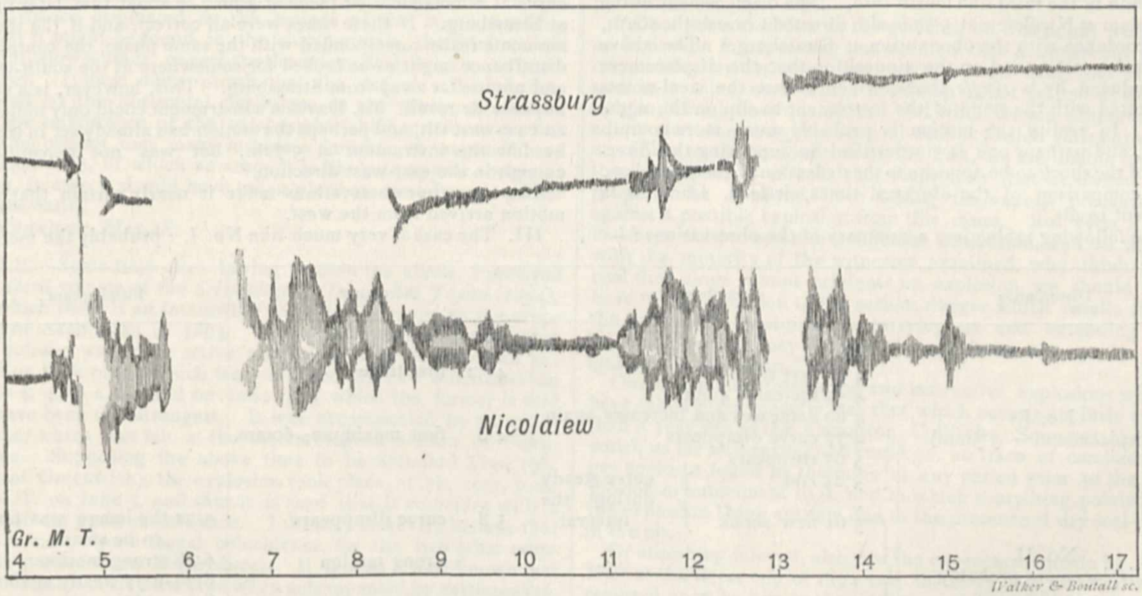

FIG. 1.—Earthquake Disturbance observed at Strassburg and at Nicolaiew on June 3, 1893.

In comparing the two curves, it is evident that the different intensity of motion at the two places is not due to the difference in the values of the instrumental constants. The reason why the motion of the pendulum is so much stronger at Nicolaiew is this, that the soil consists down to a great depth of sand, which is particularly favourable for the development of strong motion. In this respect Nicolaiew resembles the two former stations, Potsdam and Wilhelmshaven. Many facts tend to show that the soil at Strassburg, though often disturbed by small earthquakes of distant origin, never oscillates as much as at the forenamed places. It would not be right, therefore, from the mere look of the curves, to draw the conclusion that the earthquake—if such was the cause of the disturbance—must have originated at a place considerably nearer to Nicolaiew than to Strassburg.

Until now I have not been able to find a record of a phenomenon which might possibly be connected with this disturbance. From its size and duration, one ought to think that it must have been caused by a strong catastrophe, surpassing anything that has been reported during the last year from all parts of the world. But it is strange that the magnetic recording instruments at Potsdam have shown no trace of motion, and that nothing is reported from the delicate seismological instruments which are at work in Italy.

[1] In the original photograph the second part of the curve is much displaced, in the same way as the third part after the interruption. This was altered in the figure to economise space.

The case is remarkable in more than one respect. Displacements of the light-point, which, though the oscillations of the pendulum were much larger generally, were scarcely noticeable during the former observations with this instrument at other places, often occur at Strassburg. I am inclined to think that they are due to a vibratory motion of the ground, which scarcely affects the motion of the pendulum, but may cause a change in its position with regard to the steel pivots. These vibrations appear to be more easily propagated by the soil at Strassburg than at Nicolaiew, for though small displacements occasionally occur at the latter place, they are considerably smaller. This is particularly evident in the present case, where the only displacement worth mentioning is connected with the shock at 5·77. On the other side, the displacement at Strassburg, which produced the long break in the curve, is far the largest that occurred during one and a half years' observation. It is much larger than that which took place when an iron hook was driven into the pillar on the side opposite to the pendulum.

Our figure shows that the displacements of the pendulum were comparatively larger during the first and second than during the third and fourth disturbance. The change during No. III. is about 1 mm. Another fact worth noting is that in the two first cases the pendulum is deflected towards the south, and in the two last towards the north. This seems to indicate, if one considers the special arrangement of the instrument, that the motion arrived from the north in the first and second, and from the south in the third and fourth case. The displacement of the pendulum at Nicolaiew at 5·77 is also directed towards the south, in accordance with the observation at Strassburg.[1] The above conclusion is founded on the supposition that the displacement is produced by a *single* shock, which causes the steel-points connected with the stand of the instrument to slip on the agate cups. In reality, the motion is probably much more complicated, and perhaps one is not justified in supposing the direction of the shock to be opposite to the deflection of the pendulum. The comparison of the observed times, indeed, leads to a different result.

The following table gives a summary of the observations :—

Disturbance	Strassburg.	Nicolaiew	Birmingham
	h.	h.	
	4·42 first trace	4·32 first trace	
	4·52 increases 4mm.		
No. I.	4·69 decreases and increases again		
(displacement − 3·5mm)	4·77 curve disappears	4·8 first maximum>60mm.	
	5·05 reappears		
	5·25 end } curve steady during the interval		
			h.
	5·61 new shock	5·8 curve disappears	5·72 the image was found to be steady
No. II.		strong motion	6·48 strong motion
(displacement probably >−48mm.)			6·70–6·77 steady again
	8·45 } light point corrected	8·4	8·22 end
	9·65 } motion small } nearly steady	9·72 } small motion	
	11·16	11·05	
No. III.	11·45 first increase 5mm.	11·36 sudden increase	
(displacement + 1mm.)	11·60 second increase 10 mm.		
	12·10 end } curve steady	12·3 diminishes	
	12·26 first small motion		
	12·47 increase 6mm.		
No. IV.	12·73 curve disappears	12·7 curve disappears	
(displacement + 10.3mm.	13·03 reappears } motion small	13·22 ,, reappears	
	14·45	13·97 decrease of motion	
	14·95 new small increase	14·87 new increase	
		15·17 end	

When looking over these figures, one is inclined to think that the remarkable correspondence between the several phases of the disturbance cannot be due to chance. If we take as the beginning of a disturbance the moment when its first traces are visible, we have the following differences at Strassburg :— II.-I. = 1·19h. and IV.-III. = 1·10h., III.-I. = 6·74h., IV.-II. = 6·65h. At Nicolaiew, where I. and II., III., and IV. appear as a single disturbance each, we have III.-I. = 6·73h. Again the times of disappearance of the curve or of maximum motion are separated by nearly the same interval, viz., at Strassburg III.-I. = 6·83h., IV.-II. = 7·12h.,[1] and at Nicolaiew IV.-II. = 6·9h. The duration at Strassburg of No. I. is 0·83h., and of No. II. 0·94h. ; the duration of No. III., if we omit the last part, in which the motion was very small, is 2·84h., of No. IV. 2·69h. At Nicolaiew, during the first half of the disturbance, the strong motion ends 4·08h. after the beginning, and the second part lasts 4·12h. The intensity of I. and II. is evidently larger than that of III. and IV.

We will now see if the direction of motion can be determined by the observations.

I. Though the first trace of motion is 0·10h. earlier at Nicolaiew than at Strassburg, yet it is probable that the corresponding moments are those of the disappearance of the curve at Strassburg and of maximum oscillation at Nicolaiew, or 4·77h. and 4·8h. To judge from the copy, which Prof. Kortazzi sent me, the latter value is only approximate. The difference in time is certainly small, and the direction of the motion remains rather uncertain ; the general aspect of the figure, however, makes it more probable that it came from the east.

II. The time of disappearance of the curve at Strassburg, 5·61h., is probably correct within 0·02h. or 0·03h. Mr. Davison's observation shows that the motion, which in this case appears to have commenced suddenly, had not reached Birmingham[2] at 5·72h.; on the other hand, the disappearance of the curve at Nicolaiew took place at 5·8h., or about 12m. later than at Strassburg. If these times were all correct, and if the three moments really corresponded with the same phase, the centre of disturbance ought to be looked for somewhere at the south-west and not too far away from Strassburg. This, however, is a very improbable result. Mr. Davison's instrument could only indicate an east-west tilt, and perhaps the motion had already set in when he left the instrument at 5·72h., but was not perceptible enough in the east-west direction.[3]

The two other observations make it nearly certain that the motion arrived from the west.

III. The case is very much like No. I. : probably the motion arrived at Nicolaiew first, but its direction cannot be determined with certainty.

[1] In the figure the curve is displaced in an opposite direction, but this is the case because the drum stands west of the pendulum at Nicolaiew, and east of it at Strassburg.

[1] The beginning of II., though sudden and sharp, need not necessarily coincide with the movement of greatest motion ; in this case the difference IV.-II. would have a smaller value.

[2] The distance between Strassburg and Birmingham is about 800 kilometres.

[3] [Much weight cannot be attached to the absence of observed motion at Birmingham at 5·72h. The image of the wire was adjusted on the cross-wire of the telescope without difficulty, and must have remained practically in contact for a few seconds. A small movement, with a period so long as twenty seconds, might easily at this time have escaped notice.—C. D.]

IV. The curves again disappear at about the same time; but to judge from the time of greatest steadiness before the disturbance commenced at Nicolaiew, it appears to have reached Strassburg first. The last small increase at 14·87h. and 14·95h. is, on the contrary, earlier at Nicolaiew than at Strassburg, but this might be an independent disturbance. After the strongest motion, the light-point resumes its steadiness much sooner at Strassburg than at Nicolaiew.

It is evident that the case is, on the whole, not favourable to an hypothesis which first occurred to me, that all four disturbances might have been caused by four successive waves emanating from a single centre and a single shock, and circulating round the earth. The fact that II. and IV. are more considerable than I. and III. does not appear of much importance, for it is proved by many examples that the intensity of a disturbance is not alone dependent from the distance from the centre; but, if the hypothesis were right, disturbances III. and IV. ought probably to be much smaller. Besides, the velocity of about 100 km. per minute would be a very small value compared to those determined on other occasions.

I reject this hypothesis, but I do not think it improbable that I. and II., III. and IV. may be connected in the way just mentioned, and that both disturbances came from the same part of the world. It is the principal object of this communication to induce persons interested in the subject to study carefully the records of all self-registering instruments. If the disturbance originated at the bottom of the sea, something about it might be found in the ship journals, the tidal records might show a trace, or perhaps the magnetical records at distant places. I have many proofs that the size of a disturbance, traced by the horizontal pendulum, is not always a measure for the importance of the catastrophe which produced it; but in the present case many instances indicate an extraordinary phenomenon, of which an account is likely to appear sooner or later, in case it should have taken place at some remote corner of the earth.

Merseburg, May 18.

P.S.—Some time after having written the above, I received the third volume of the *Seismological Journal of Japan* (1894), in which there is an interesting paper by F. Omori on the eruption of Azuma-san in 1893. From this paper it appears that the volcano was in an active state since May 19, when an explosion took place, which was followed by two other ones on June 4, 4.10 a.m., and on June 7, of which the former is said to have been the strongest. It was accompanied by an earthquake, which was felt at the meteorological station of Fukushima. Supposing the above time to be Standard Time (9h. east of Greenwich), the explosion took place at 7h. 10m. p.m. G.M.T. on June 3, and thus it is seen that it coincides with a part of our great disturbance. I do not, however, believe that this is more than a casual coincidence, for the two other eruptions produced no disturbances. It is also a well-known fact that volcanic eruptions, even when accompanied by earthquakes, are generally not felt to any great distance, unless they bear a very violent character, like the eruption of Krakatoa; but from Mr. Omori's description it appears that the eruption of Azuma-san was nothing very extraordinary. I therefore believe that we must wait to find another explanation for our disturbance.

E. VON REBEUR-PASCHWITZ.

EXPLOSIONS IN MINES.

IN a lecture on some modern developments in explosives, given at the Society of Arts on December 17, Prof. Vivian B. Lewes threw out a suggestion as to the cause of explosions in dusty mines free from fire-damp, which explains the anomalies which have presented themselves in several recent explosions.

It was pointed out that until quite recently explosions in mines were always attributed to the accidental ignition of mixtures of air and methane, to which the name of "fire-damp" is given, and undoubtedly this cause is the prime factor in this class of disaster, and the introduction of such precautions as safety-lamps at once brought about a considerable reduction in the number of explosions taking place. Many disasters, however, still continued to occur under apparently mysterious circumstances, the conditions being such that any large proportion of methane in the air of the mine appeared practically impossible, but investigations of such explosions showed that coal-dust in a dry and finely powdered condition had generally been present in the mine at the time of the explosion, and the coked residue of this dust was found afterwards on the surface exposed to the explosive wave, and years of experimental investigation by scientific men of the greatest ability proved the fact that air containing so small a proportion of methane as to be itself perfectly non-explosive, becomes a good explosive again when holding dry and finely divided coal-dust in suspension, and within the last few years explosions having taken place in mines, which have always been celebrated for their freedom from any trace of methane. Further experiments have been made by Mr. H. Hall and Mr. W. Galloway, who have shown that the violent ignition of dust-laden air is possible by a blown-out shot, even if free from any trace of marsh gas, and there is evidence to show that the explosion is developed in throbs or waves.

It is therefore found that the explosions in mines may be brought about, first, by the ignition of a mixture of methane and air, in which the former rises above a certain percentage; secondly, by mixtures of air, coal-dust, and methane, in which the amount of the latter may be excessively small; lastly, by mixtures of coal-dust and air. With regard to these explosions caused by coal-dust and air alone, the Royal Commission on Explosions from Coal-Dust in Mines, in their second report, published this year, say:—

"On a general review of the evidence on this point, we have no hesitation in expressing our opinion that a blown-out shot may, under certain conditions, set up a most dangerous explosion in a mine, even where fire-damp is not present at all, or only in infinitesimal quantities; and while we are prepared to admit that the danger of a coal-dust explosion varies greatly according to the composition of the dust, we are unable to say that any mine is safe in this respect, or that its owners can properly be absolved from taking reasonable precautions against a possible explosion from this cause. But even if we had been able to come to a different conclusion, and to agree with the minority of the witnesses examined, who think that coal-dust alone cannot originate an explosion, we should still have to call attention to the serious danger which results from the action of coal-dust in carrying on and extending an explosion which may originally have been set up by the ignition of fire-damp."

One of the most interesting and instructive explosions which have taken place recently was that which occurred a little more than a year ago at the Camerton Collieries, Somersetshire, in which as far as investigation could go, no trace of combustible gas could be found in the mine at any period prior to the explosion or subsequent to it, and in which everything pointed to the explosion being entirely due to the presence of dry coal-dust in the air.

Of absorbing interest, also, are the experiments made by Mr. Hall at the latter end of 1892 and the early part of 1893, and reported upon by him to the Secretary of State on January 23, 1893, in which he shows by conclusive experiments that dry coal-dust under conditions frequently present in coal mines and in the entire absence of fire-damp, may be inflamed by a blow-out gunpowder shot, and cause a disastrous colliery explosion.

The evidence which can be collected from the investigation in the Camerton disaster, and from Mr. Hall's experiments, point to a cause for such explosions, which has apparently been overlooked, and which Prof. Lewes thought worthy of the gravest attention. Both at the Camerton Colliery and in Mr. Hall's experiments, powder was the blasting agent used, and such powder as is employed for this purpose, gives amongst the products of combustion nearly half the volume of permanent gases in the condition of carbon monoxide, methane, and hydrogen.

In the Camerton explosion, it seems probable that about 1¼ lbs. of such powder were used in the shot which caused the disaster, and this quantity of powder would give, roughly, a little over three feet of inflammable gas, which when mixed with pure air would give over 10 cubic feet of an explosive or, at any rate, rapidly burning mixture, and experiments which have been made upon the effect of fire-damp and dust combined in causing colliery explosions show conclusively that even when the fire-damp is present in such minute quantities as to form a mixture very far removed from the point of explosion, it still makes the mixture of coal-dust and air highly explosive; and from experiments which Prof. Lewes has made, it is clear that traces of carbon monoxide will do exactly the same thing when the air is laden with coal-dust, whilst the temperature of ignition is slightly lower than with methane, so that in the case of the Camerton Colliery, it being perfectly well ascertained that the air was charged with coal-dust, the probabilities are that not 10 feet, but a far larger volume of explosive mixture was formed by the rapid escape of the products of combustion into the coal-laden air; and this being ignited, either by the flame or red-hot solid products driven out into it by the blown-out shot, would initiate a considerable area of explosion.

The classical researches of Prof. H. Dixon have shown that hydrocarbons and, probably, carbon burn in air to carbon monoxide, and that this carbon monoxide will not form explosive mixtures with air, or even with oxygen, if they are absolutely dry; but if water vapour is present, they explode owing to the oxidation of the carbon monoxide to dioxide, causing the propagation of an explosive wave, which reaches its maximum velocity when the percentage of water vapour, between 5 and 6 per cent., and inasmuch as the air of the mines would always contain some moisture, and as the products of combustion also would give a large volume of water vapour, these requirements would be amply fulfilled.

Still more conclusive on this point were Mr. Hall's experiments. In these a charge of blasting powder was fired from a cannon suspended in a shaft, the air of which was proved by careful chemical analysis to be absolutely free from any trace of combustible gas.

In order to get some idea of the condition of the air inside the pit during the explosion, samples of air were taken and were analysed. Two brass tubes were fastened to the rope that was used to lower the cannon, one twenty yards from the bottom, the other forty yards from the bottom.

These tubes were so arranged and constructed that the explosion, as it passed the tubes, unsealed the outlet pipe, and the escaping water sucked in a sample of air which was trapped by a special arrangement, and kept in the tube until the rope could be wound up. By this method it was intended that the sample of gas taken should represent that state of the air whilst the flame was passing, or directly afterwards.

The tube nearest the bottom, as the analysis will show, did partly collect the gas in the above condition. The tube at the top, however, commenced to act prematurely, and was probably started by the sound wave which preceded the explosion. This tube simply contained ordinary air.

The following is an analysis of the gases found in the lowest tube:—

	Per cent.
Oxygen	3 9
Nitrogen	75·9
Carbon dioxide	12·1
Carbon monoxide	8·1
	100·0

This ingenious arrangement was due to Mr. W. J. Orsman, and it is probably the first successful attempt which has been made to get a sample of gas during the progress of explosion, and there is not the slightest doubt that the presence of such an amount of carbon monoxide converts mixtures of coal-dust and air into a highly explosive body.

As the explosion takes place, and as the carbon monoxide ready produced is oxidised to carbon dioxide by the action upon it of water vapour present, and also by its direct combustion with oxygen, the hydrogen of the water vapour is set free, whilst the heated coal-dust also yields certain inflammable products of distillation to the air, and partial combustion also of the coal-dust gives a considerable proportion of carbon monoxide once more, and these driven rapidly ahead of the explosion form with more coal-dust and air a new explosive zone, and so by waves and throbs the explosion is carried through the dust-laden galleries of the mine.

The experiments made by Mr. Hall, and investigations in various colliery explosions, make it abundantly manifest that no explosive should be licensed for use in mines unless it can be absolutely proved that it gives off no imflammable products of combustion. The following table will show the results given by some of the explosives most largely used, which point very clearly to the fact that, with the exception of the Sprengel explosives, such as roburite and nitroglycerine, none of the bodies in use conform to this important requirement.

Products of Combustion of Blasting Explosives.

		Combustibles.	
Powder.	Carbon dioxide.	Carbon monoxide.	Hydrogen and marsh gas.
Gunpowder ...	50·6 ...	10·5 ...	3·1
Blasting power ...	32·1 ...	33·7 ...	7·9
Sprengel explosives—			
Roburite	32·0 ...	nil ...	nil
Ammonite ...	33·0 ...	nil ...	nil
Nitroglycerine explosives—			
Nitroglycerine ...	63·0 ...	nil ...	nil
Gelignite	25·0 ...	7·0 ...	nil
Carbonite	19·0 ...	15·0 ...	26·0
Blasting gelatine ...	36·5 ...	32·3 ...	8·6

Whilst not only these considerations, but Mr. Hall's experiments, point to the absolute necessity of legislative enactments at once forbidding the use of blasting powder in any coal mines, no matter how free they may appear to be from fire-damp or from dust, if the returns made as to deaths caused by gunpowder and other explosives in mines for the year 1893 are examined, it will be clearly seen that the exclusion of gunpowder, in handling alone, would do away with 80 per cent. of the accidents. So that if explosives of the Sprengel class were employed, accidents due to the explosives used would be practically eliminated from the mining death roll; and it is only a question of time as to when England will foll w the action of France and Germany in altogether prohibiting the use of blasting power in dusty mines.

THE POSSIBILITIES OF LONG-RANGE WEATHER FORECASTS.[1]

IF we had perfect command of this subject, we should be able to trace the motion of a particle of aqueous vapour from point to point over the whole earth, and could predict whether at any time in the future it will fall as rain, or rise and fly away as an invisible gas. In the absence of this higher knowledge the only long-range forecasts that we are at present able to make are based upon empirical and very imperfect rules deduced from our study of the accumulated climatological statistics. Of course, such predictions do not imply any special knowledge of meteorology. Among the methods adopted in long-range forecasts are the following:

(*a*) The average rainfall, temperature, &c., for any period, such as a month, and deduced from many years of observation, is called the normal. The excess or deficiency of this month in any given year is called the departure for that year. A general prediction may be made to the effect that the rainfall for a given month and place may be expected to lie within the range of the values indicated by these known departures.

(*b*) The series of annual or monthly values just mentioned gives us the means of finding out whether there is any simple sequence or connection between them and the apparently unconnected values that occur from year to year. Thus, it sometimes happens that rainy seasons come after two or three dry seasons, or that after the same month has been dry in three successive years, one is then justified in predicting a wet month. Thus, Governor Rawson elaborated a system for the prediction of rain and the sugar crop in Barbados.

(*c*) Slight but appreciable widespread, rather regular fluctuations of temperature, pressure, and rain have been revealed in the climate of Europe by Dr. Brückner, who finds that a deficient temperature and an excess of rain have alternated with excess of temperature and deficiency of rain in periods of thirty-six or thirty-seven years during the past two or three centuries; the glaciers increase and diminish in volume, or advance and retreat, in correspondingly regular but somewhat retarded intervals. Predictions may be based on these well-established periods.

(*d*) Droughts are sometimes due to what happens in distant regions: thus, if there is a heavy snow on the Himalayas during the winter, there is a special liability to drought in lower India in the following summer, so that the prediction of a drought may be based upon the reports of snow fall in a distant region several months before the drought occurs; but other droughts may occur without this preliminary snow-fall. This connection

[1] Reprint of an article contributed by Prof. Cleveland Abbe to the U.S. *Monthly Weather Review.*

is, so far as at present known, a local, arbitrary, or accidental one, and has not yet been found to recur in any other portion of the globe.

(*e*) Droughts or floods may occur every year in some portion of an extensive region, so that it may become possible to predict the occurrence in a special section one year because one has occurred in another section a previous year. Thus, a serious drought in the lower Indian peninsula has, on five occasions, been followed by one in northern India the next year.

(*f*) If we had maps of the weather of the whole globe for every month for a long series of years we should, undoubtedly, be able to find many similar coincidences, so that a drought for a given section might be predicted from the rain-fall, the snow-fall, the temperature, the pressure, or other conditions in a distant part of the globe. As a rule, important climatic crises are the results of changes that have been going on slowly for a long time in distant parts of the earth. The general circulation of the air constitutes a complex system in which the areas of high pressure and dry clear air are the results of slowly descending winds moving toward the equator; the general rains are formed wherever a descending current of air, a mountain range, or other obstacle has an opportunity to push up the moister air of the earth's surface. From this point of view rainy and dry and cold and hot seasons depend largely upon the varying relations of the upper and lower currents to the continents and even to each other. The long-range prediction of the climate of any season must depend upon the prediction of the general character of the horizontal and vertical movement of the air. In our present geological epoch the continents are permanent features, and we consider only the changes that take place in the atmosphere, but in studying the climatic changes of earlier geological epochs we have to consider the changes in elevation of the continents themselves.

(*g*) Such apparent connections as that between snow-fall on the Himalayas and the subsequent drought in northern India are not to be thought of as cause and effect respectively. It might be argued that the layer of snow must be evaporated, or melted, thereby absorbing more heat than would have been required if it had fallen as rain and rapidly drained away; but this cooling influence is distributed over many weeks, and through the immense quantity of air that has passed over the snow-fields during the winter and the spring, and is thereby rendered too slight to have any great local influence in India. A broader view of the subject shows us that the winter snow-fall and the summer drought are simply two features of an extensive system of changes in which the whole atmosphere of the earth takes part. The whole globe may be divided into regions where the lower stratum is moving either horizontally or upward or downward, and where the upper stratum has similar diversities of movement. These systems of motion determine whether we shall have fair weather or rain, hot weather or cold, from day to day and accumulatively from month to month. Now these three movements are related to each other in such a way that the sum total of the energy involved throughout the atmosphere is sensibly constant, while the localities at which the upward and downward motions are taking place are undergoing perpetual changes.

The centres of high pressure over the oceans and continents slowly sway east and west or north or south; the paths of the storm-centres vary in a similar manner to suit the changes of these larger areas, and the centres themselves move rapidly or slowly in response to these same changes. The air that ascends between the northern and southern tropical regions of high pressure descends sometimes in high latitudes, giving them cold weather with rain or snow; at other times in low latitudes, giving them warm weather with droughts. It matters not whether the droughts in southern regions chronologically follow or precede the snows of the northern regions; in neither case can either one be spoken of as the cause of the other, but each is in its turn the result of changes in the so-called general circulation of the atmosphere.

This general circulation, with all its variations, diurnal, annual, and secular, is dependent upon the intrinsic density of each portion of the atmosphere and on numerous forces, such as the heat received from the sun, the attraction of the sun, moon, and earth, the resistance offered by the irregular surface or the earth, and the interaction of slow and rapidly moving masses of air. The proper study of this subject constitutes the application of hydrodynamics to meteorology.

The meteorological problem has some analogy to that offered by the hydraulics of the Mississippi River, where cut-offs, cave-ins, mud-banks, and crevasses are continually forming and re-forming. We do not expect to be able to foretell when and where these will occur many years in advance, but we do keep a watch on the condition of the river; and when conditions are favourable for the formation of any important change, we watch the process until the catastrophe becomes more or less imminent, and then begin to make estimates, that may be called predictions, as to the exact time and place of the event.

In meteorology the best we can do at present in long-range predictions is to chart and study the occurrence of abnormal weather conditions over the whole globe; these phenomena must be interpreted in the light of all the knowledge we have of the mechanics of the atmosphere, for they are the results of purely mechanical operations covering the whole range of the mechanics of heat, gases, and vapours.

SCIENTIFIC SERIAL.

The Quarterly Journal of Microscopical Science, November.—On Julinia, a new genus of compound ascidians from the Antarctic Ocean, by W. T. Calman (plates 1–3). The colony is described as irregularly cylindrical in shape, measuring 78·5 cm. in length, and from 1·5 to 2·5 cm. in diameter; it was found floating on the surface of the sea in the north of Erebus and Terror Gulf; a considerable quantity was seen; no attaching fibres were found, but it was probably an attached form. The species is described as *Julinia australis*, and it is provisionally placed in the Distomidæ.—Hermaphroditism in mollusca, by Dr. Paul Pelseneer (Ghent) (plates 4–6). Hermaphroditism is found in the Amphineura, the Gastropoda, and the Lamellibranchia. It is not self-sufficient, is sometimes protandric; it would seem to the author to be not a primitive arrangement, but to be derived from the unisexual state, and to have been established upon the female organism.—Description of the cerebral convolutions of the chimpanzee known as "Sally," with notes on the convolutions of the brains of other chimpanzees and of two orangs, by W. Blaxland Benham (plates 7–11).—On the inadequacy of the cellular theory of development and on the early development of nerves, particularly of the third nerve and of the sympathetic, in Elasmobranchii, by Adam Sedgwick, F.R.S. More than ten years ago the author called attention to the inadequacy of the cellular theory of development: "Embryonic development can no longer be looked upon as being essentially the formation by fission of a number of units from a single primitive unit, and the coordination and modification of these units into a harmonious whole. But it must rather be regarded as a multiplication of nuclei and a specialisation of tracts and vacuoles in a continuous mass of vacuolated protoplasm." And "although opinions have changed on this important subject, and although there are some who think that they have escaped from the domination of this fetish of their predecessors, yet as a matter of fact the cellular theory of development is still rampant, still blinds men's eyes to the most patent facts, and still obstructs the way of real progress in the knowledge of structure." When a student begins his zoology he is told that "the various structures present in a protozoon are all parts of one cell, whereas in a metazoon the various parts are composed of groups of cells which differ from one another in structure." When in a later period of his studies he begins embryology, "the importance and distinctness of the cell meets him at every step, from the complete cleavage which he is led to believe is primitive, to the development of nerves according to the views of His." If we take the so-called mesenchyme tissue of elasmobranch embryos, it is described as consisting of "branched cells lying between the ecto- and the endo-derm," while, as a matter of fact, "the separate cells have no existence," but "there is a reticulum of a pale non-staining substance holding nuclei at its nodes. And far from the development of nerves being an outgrowth of cell-processes from certain central cells, it is simply a differentiation of a substance which was already in position." This important memoir is so condensed as to make it extremely difficult to condense it further, but enough has been given to indicate its nature.—On *Benhamia caecifera*, n. sp., from the Gold Coast, by W. B. Benham (plate 12). This large species (20 inches) was found at Axim in the Fantee country, on the west coast of Africa.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 22.—"A Determination of the Specific Heat of Water in terms of the International Electric Units." By Prof. Arthur Schuster, F.R.S., and William Gannon, Exhibition (1851) Scholar, Queen's College, Galway.

This research was originally undertaken by Prof. Schuster and Mr. H. Hadley, before the authors were aware that Mr. E. H. Griffiths was engaged on a similar investigation. After a number of preliminary experiments, and just as the final arrangements for the conduct of the measurements were being definitely made, Mr. Hadley, on his appointment to the headmastership of the School of Science and Art, Kidderminster, had to leave Manchester.

On Mr. Hadley's departure, Mr. W. Gannon took his place. From the former gentleman we received a good deal of help in the devising and construction of some important parts of the apparatus.

The principle of the method we have used is extremely simple. The electrical work done in a conductor being measured by $\int ECdt$, where E is the difference of potential at the ends of the conductor, C the current, and t the time. We keep the electromotive force constant, and measure $\int Cdt$ directly by a silver voltameter. We do not, therefore, require to know the resistance of the wire, and we thus avoid the difficulty of having to estimate the excess of temperature of the wire over that of the water in which it is placed. We also gain the advantage of not having to measure time, and therefore to be able to complete the experiments more quickly than we could have safely done if the length of time the current passed had to be measured with great accuracy.

Our final value is

$J = 4{\cdot}1804$ Joules on the mercury scale of hard French glass,
$4{\cdot}1905$ on the nitrogen scale,
$4{\cdot}1917$ on the hydrogen scale,
at a temperature of $19^{\circ}{\cdot}1$.

In comparing our results with that of other observers, we have in the first place to consider the value which Mr. Griffiths has obtained in his very excellent series of measurements. His final result (*Roy. Soc. Proc.* vol. lv. p. 26; *Phil. Trans.* clxxxiv. A (1893) is

$$J = 4{\cdot}1982(1 - 0{\cdot}00266\,\overline{\theta - 15}) \times 10.^7$$

This refers to the nitrogen thermometer. At a temperature of $19^{\circ}{\cdot}1$, the value would be reduced to $4{\cdot}1936$, which corresponds to our $4{\cdot}1905$ at the same temperature. Griffiths' value is to be increased slightly, owing to the fact that he really measures the difference between the specific heat of water and of air. This would increase the value of J by $\cdot 0011$ about, so that the value of J at $19^{\circ}{\cdot}1$ would be raised to $4{\cdot}1947 \times 10^7$, which is exactly one part in a thousand larger than ours. The difference is small, but must be due to some systematic error, as both Griffiths' value and our own agree so well with each other, that ordinary observational errors and accidental disturbances could not have produced so large a difference in our results. The least satisfactory part of a calometric measurement must always be the application of the cooling correction, and we have considered it of great importance to reduce that correction as much as possible. The uncertainty of the cooling correction does not necessarily depend on its value; thus we can much diminish it by starting, as we have done in the third series, with the initial temperature of the calorimeter about as much below that of the water jacket as the final temperature is above it; yet the *uncertainty* of the correction does not seem to us to be diminished by that process. We may reasonably estimate the uncertainty due to the cooling correction, by calculating what the error in the observed rate of cooling, either at the beginning or the end of the experiment, must have been in order to produce a difference of one part in a thousand in the final result. We find in our own experiments that the error must have amounted to more than 15 per cent. We consider it unlikely that so large an error occurred always in the same direction. Apart from the cooling correction, however, it is difficult to see how a difference one-tenth per cent. in our result can be produced unless by the accumulation of a number of small errors.

The difference between our value of the equivalent and that of Mr. Griffiths are, however, of smaller importance than the difference which exists between them and the equivalent as determined directly by Joule, Rowland, and Miculescu. Joule's latest value, which is the only one which needs consideration, is 772 65 foot-pounds, at 61·7 F. The number refers to the degree as measured by Joule's mercury thermometer. Rowland adds to this a correction to the air thermometer of about 3, and another small correction for a change in the heat capacity of the apparatus, which brings the value up to about 776. The correction to the air thermometer has been obtained by means of a comparison made by Joule himself with one of Rowland's thermometers. Joule's original thermometers have been temporarily placed by Mr. B. A. Joule in the hands of Prof. Schuster, in order that an accurate comparison may be instituted between them and modern thermometers. A full description of the comparisons made will be given on another occasion. The result arrived at shows that the correction is less than that assumed by Rowland, and would bring his value up only to 775 at the temperature indicated.

Great weight must be attached to Rowland's determination, which at the temperature to which Joule's number applies is 777·6, and at $19^{\circ}{\cdot}1$, 776·1, corresponding to our 778·5.

Equivalent in foot-pounds at Greenwich at $19^{\circ}{\cdot}1$ *referred to the "Paris" Nitrogen Thermometer.*

Joule.	Rowland.	Griffiths.	Schuster and Gannon.
774	776·1	779·1	778·5

We now turn to an investigation of Miculescu (*Annales de Chimie et de Physique*, vol. 27, 1892), in which the mechanical equivalent of heat is measured directly by what seems a very excellently devised series of experiments. Its result is $4{\cdot}1857 \times 10^7$.

In order to compare Miculescu's value with that of others, we must apply a temperature correction which is somewhat doubtful; but taking the mean of Rowland's and Griffiths' values as the most probable at present, we obtain at 15° the following table:—

Equivalent in foot-pounds at Greenwich at 15° *referred to the "Paris" Nitrogen Thermometer.*

Joule.	Rowland.	Miculescu.	Griffiths.	Schuster and Gannon.
775	778·3	776·6	780·2	779·7

If we remember that Rowland's number referred to the "Paris" nitrogen thermometer would probably be smaller by one unit, we are struck with the fair agreement there is, on the one hand, between the results of Joule, Rowland, and Miculescu, and on the other hand between Griffiths and ourselves.

As far as we can draw any conclusions from the comparison, it seems to point to a difference in the value obtained by the electrical and direct methods. Whether this difference is due to some remaining error in the electrical units, or to some undiscovered flaw in the method adopted by Mr. Griffiths and ourselves, remains to be decided by further investigation.

Linnean Society, December 6.—Mr. C. B. Clarke, F.R.S., President, in the chair.—Mr. E. M. Holmes exhibited and made remarks upon a small collection of Japanese marine algæ, some of which were of considerable rarity in European collections.—Prof. D. Campbell brought forward some illustrations of the relations of vascular cryptogams, as deduced from their development. His remarks, which were listened to with great attention, gave rise to an interesting discussion, in which Prof. Bower, Dr. D. H. Scott, Mr. Carruthers, and Prof. Marshall Ward took part.—"A new revision of the *Dipterocarpeæ*," was the title of a paper by Sir Dietrich Brandis, K.C.I.E., who gave an excellent account of this order of forest trees, their structure and mode of growth, together with a survey of the literature relating to them, and a clear exposition of his views concerning classification. He pointed out that the order *Dipterocarpeæ* consists almost entirely of large trees which do not flower until they have attained a great size, with a spreading crown on a branchless stem often more than 100 feet high. Hence it is difficult to obtain complete specimens in flower and fruit; and this explains why a large proportion of the genera and species have only of late years become accurately known. Korthals in 1840 knew 34 species; A. de Candolle in 1868 described 126; Mr. Thiselton Dyer in 1874 estimated the order at 170. Sir D. Brandis now considers that there are 320 well-ascertained species, belonging to sixteen genera, omitting the genera *Ancistrocladus* and *Lophira*, which he regards as justly excluded from the order. Notable species are the Sâl tree of India (*Shorea robusta*), great forests of which extend along the foot of the Himalayas and in Central India, the Eng tree (*Dipterocarpus tuberculatus*) of similar growth in Burma, and others found in Cochin China and Borneo. In the discussion which followed, an extended criticism was offered by Mr. Thiselton Dyer, who had paid special attention to this order of trees, and who, admitting the soundness of the author's views, considered his exposition of them most valuable. The paper was illustrated by lantern-slides showing the chief peculiarities of structure in the flowers and fruit.

Royal Meteorological Society, December 19.—Mr. R. Inwards, President, in the chair.—Mr. H. Southall read a paper on floods in the West Midlands, in which he gave an interesting account of the great floods which have occurred in the rivers Severn, Wye, Usk, and Avon. He has collected a valuable record of the floods on the Wye at Ross, which he arranges in three classes, viz. (1) primary or highest of all, those of 14 feet 6 inches and above; (2) secondary, those with a height of 12 to 14½ feet; and (3) tertiary, those with a height of 10 to 12 feet. The dates of the floods above 14 feet 6 inches are as follows: 1770, November 16 and 18; 1795, February 11 and 12; 1809, January 27; 1824, November 24; 1831, February 10; 1852, February 8 and November 12. The height of the recent flood on November 15, 1894, was 14 feet 3 inches, which was higher than any flood since November 1852. The flood on the Avon at Bath on November 15, 1894, is believed to have been the highest on record.—Mr. R. H. Scott, F.R.S., gave an account of the proceedings of the International Meteorological Committee at Upsala in August last, with special reference to their recommendations on the classification of clouds and the issue of a cloud atlas (see NATURE, December 20).—A paper by Mr. S. C. Knott was also read, giving the results of meteorological observations made at Mojanga, Madagascar, during 1892 to 1894.

EDINBURGH.

Royal Society, November 27.—Prof. Copeland, Astronomer-Royal for Scotland, Vice-President, in the chair.—Prof. M'Kendrick read a paper on observations with the phonograph, with experimental illustrations. He has devoted great attention to the development of the instrument. He uses very large conical metallic resonators, and has succeeded largely in getting rid of the nasal sound of the instrument, so that part-songs and concerted instrumental pieces can be reproduced with considerable accuracy, and can be made audible throughout a very large room. He exhibited, by means of a lantern, a large number of photographs of the surface of the wax drum, pointing out the peculiarities of the record corresponding to various qualities of instrumental or vocal notes and chords.

December 3.—Prof. Geikie, Vice-President, in the chair.—Dr. John Smith communicated notes on a peculiarity in the form of the mammalian tooth. Roughly speaking, the general appearance of the mammalian tooth is that of a cone, flattened to some extent, and twisted about its axis to a greater or less degree, and then bent so as to form a portion of a circle. If this bending takes place to a large extent, it is not easy to recognise the axial twist. The author showed that the characteristic is always present, being easily seen in the strong spiral of the narwhal's tusk, or the remarkably twisted teeth of the *Mesoplodon* described by Sir William Turner in the Reports of the *Challenger* expedition, and being almost unrecognisable in the human tooth. The axis of the twist is directed backwards and inwards from the face of the tooth, and it is this characteristic which enables dentists to distinguish teeth from each side of the mouth.—Mr. Gregg Wilson read a paper on the development of the Müllerian duct of amphibians. He contends that this duct does not arise from splitting of the segmental duct, but is developed in the same way as the Müllerian duct of the higher mammals.—Dr. George Hay, Pittsburg, submitted an account of a new method of correcting courses at sea. His apparatus consists of two superposed compass cards, whose north points are set at an angular distance apart which is equal to the magnetic variation. The true course being read off on one, the corresponding point of the other gives the compass course. Simple as this arrangement is, Dr. Hay asserts that he has never known it to be employed at sea.—Prof. Tait read a note on the constitution of volatile liquids. His equation, deduced from the graph of the *Challenger* results, applies with great accuracy to non-volatile liquids, such as water, at ordinary temperatures and at pressures up to 3000 atmospheres. It does not apply with quite so great accuracy at the lower pressures to such liquids at or near their boiling points, and it is still less accurate in this respect when applied to volatile liquids. Prof. Tait suggests that this may be due to the existence, in the liquid, of dissolved gases or of vapour.—Prof. Tait also read a note on the isothermals of ethylene." His equation enables one to calculate, with great accuracy, the pressure, at a given temperature and volume, in the neighbourhood of the critical point, from Amagat's observations; but the volume, at a temperatu e and pressure in the neighbourhood of the critical point, given by Amagat's observations, cannot be calculated, with any approach to accuracy, from the equation. This is due to the excessive rapidity with which the difference of the volumes in the liquid and vapourous states diminishes with increase of temperature as the critical point is approximated to.

PARIS.

Academy of Sciences, December 17.—Annual public meeting.—M. Maurice Lœwy in the chair.—The proceedings were commenced by an address, delivered by the President. The past year was referred to as a period of slow growth and consolidation of knowledge rather than as being characterised by any very brilliant discoveries. The members and associates deceased during the year—MM. Edmond Fremy, Brown-Séquard, Mallard, Duchartre, Ferdinand de Lesseps, General Favé, MM. Hermann von Helmholtz and P. Tchébichef—were referred to appreciatively, and their influence on the progress of science pointed out. The system of prizes given by the Academy was referred to at the conclusion of the address, which was followed by the reading of the awards by M. Berthelot. In Geometry the grand prize for the mathematical sciences was awarded to Dr. Julius Weingarten; honourable mention was accorded to M. C. Guichard. The Bordin prize was adjudged to M. Paul Painlevé (Analytical Mechanics), MM. Liouville and Elliot receiving honourable mention. The Francœur prize was obtained by M. J. Collet; the Poncelet prize by M. H. Laurent, for his mathematical works. In Mechanics the extraordinary prize of 6000 francs was awarded to (1) M. Lebbond (2000 fr.), for his works on electricity; (2) Commandant Gossot (2000 fr.), for the determination of the velocity of projectiles by means of sound phenomena; (3) Commandant Jacob (1500 fr.), for his study of the ballistic effects of the new powders; (4) M. Souillagouët (500 fr.), for his "Recueil de Tables du point auxiliaire." The Montyon prize fell to M. Bertrand de Fontvioland, for his works on the resistance of materials. The Plumey prize was equally divided between M. André Le Chatelier and M. J. Auscher. M. Autonne received the Dalmont prize (3000 fr. triennially) for his works on anal sis. In connection with the same prize, M. Maurice d'Ocagne was awarded a supplementary prize, M. Pochet exceptionally honourable mention, and M. Willotte very honourable mention. In Astronomy the Lalande prize was adjudged to M. Javelle for his researches on nebulæ. The Damoisean prize, for perfecting methods of calculation of perturbations of minor planets, went to M. Brendel. The Valz prize was awarded to M. Coniel for work on small planets, and the Janssen prize to Prof. George Hale (solar photographic observations). In Statistics the Montyon prize was adjudged to M. Boutin, a supplementary prize to Dr. Faidherbe, and honourable mention to Dr. A. Cartier and Dr. Tastière. In Chemistry the Jecker prize was divided between MM. Barbier, Chabrié, P. Adam, and Meslans. In Mineralogy and Geology the Vaillant prize was not awarded, as no memoir had been presented. In Botany the judges for the Desmazières prize awarded an "encouragement," to M. Sappin-Trouffy. The Montagne prize was accorded to M. Husnot for his publication on Mosses; Brother Joseph Héribaud received a second prize for his "Diatomaceæ of Auvergne." In Anatomy and Zoology the Thore prize to M. Cuénot for work on the physiology of insects. The Savigny prize to M. Mayer-Eymar for researches in conchology. The Da Gama Machado prize was reserved, although the Commission gave high praise to work submitted by Dr. L. Phisalix and M. L. Joubin. In Medicine and Surgery the Montyon prize to (1) M. Félizet for a treatise on "inguinal hernia of infancy, (2) M. Laborde for his work on "the physiological treatment of the dead body," (3) M. Panas for his treatise on "affections of the eyes." Mentions and minor awards went to MM. Legendre, Broca, Vacquez, Vaudremer, Marcel Baudouin, Ferreira, Ernest Martin, Pietra Santa, Voisin, and Petit. The Barbier prize was awarded to Prof. Henri Leloir for his work on scrofulo-tuberculosis, Drs. Artault and Tscherning receiving honourable mention. The Bréant prize was adjudged to M. Arloing for his work on the bacillus of peripneumonia in cattle; the Godard prize was accorded to MM. Melville-Wassermann and Noël Hallé; the Parkin prize to MM. Behal and Choay; the Bellion prize between Dr. Lardier and MM. Beni-Barde and Materne, Dr. Renon receiving honourable mention; the Mége prize to M. Faure; the Lallemand prize to M. Gley, honourable mention to MM. Nabias and P. Janet.—In Physiology, the Montyon was divided between MM. Phisalix and Bertrand and M. Raphaël Dubois, honourable mention being given to MM. Morot, Blanc, and Philippon; the Pourat prize fell to M. Haufmann, a mention being accorded to M. Thiroloix. In Physical Geography, the Gay prize was awarded to M. Martel. General prizes—The Montyon prize (unhealthy industries) was divided between MM. Balland and Lavet; the Cuvier prize was awarded to Mr. John Murray of the *Challenger* expedition; the Trémont prize was accorded to M. Émile Rivière; the Gegner prize to M. Paul Serret; the Delalande-Guérineau prize to the Marquis de Folin; the Jérome Ponti prize to Commandant Defforges; the Tchihatchef prize to M. Pavie; the Houllevigue prize to M. Bigourdan; the Cahours prize (1) to M. Varet and (2) M. Freundler; the Saintour prize to MM. L Deburaux and M. Dibos; the Laplace prize to M. Édouard Glasser; and the Rivot prize to MM. Glasser, Leprince-Ringuet, Henri Parent, and Le Gavrian. The programme of prizes for 1895, 1896, 1897, and 1898 is given in detail so far as yet decided.

BERLIN.

Physiological Society, November 23.—Prof. du Bois Reymond, President, in the chair.—Prof. Zuntz gave an account of his researches on the measurement of the amount of blood in circulation and the work done by the heart. For the horse he found 71 to 72 c.c. of blood per kilo body-weight per second; for the dog, as based on the consumption of oxygen, 78 c.c. These values do not correspond to the marked difference in size of the animals, but may be explained as due to the fact that the dog was experimented upon while fasting and at rest, whereas the horse was not. For a horse in complete rest the value obtained was 50 c.c. For man he estimated the value at 60 c.c. Blood-pressure falls but slightly along the arterial system, and was found to be nearly the same in the carotid and in a small branch of the facial artery. The work done by the human heart he calculated as amounting to about 20,000 kilogram-metres in the twenty-four hours. When the body is working the work done by the heart increases also, so that in the case of the horse the blood pumped out now amounted to 600 c.c. per kilo per second, or twelve times as much as during rest. The frequency of the pulse could by work be increased four-fold, and the work done by the heart to thrice its normal amount.—Dr. Cohnstein had carried out further experiments on the transudation of solutions of salts into distilled water, and using mixtures of salts as well as mixtures of colloids and crystalloids, he had observed that an increased transudation of the solids follows upon an increase of external pressure. He applied these results to explain the mode of formation of lymph, which he attributed to transudation as well as to filtration, thus opposing Heidenhain's view that it is due to a distinct secretion. He explained the action of lymphagogues, on the basis of his own experiments, as due to the power these substances possess, when mixed with an albuminous fluid, of confining the diffusion of the external fluid entirely towards the interior of the tube which contains them in solution.

AMSTERDAM.

Academy of Sciences, November 24.—Prof. Van de Sande Bakhuyzen in the chair.—Prof. J. A. C. Oudemans communicated the results obtained in solving two problems, an astronomical and a geodetical one, namely:—(1) In how long a period do stars, the velocities of which in the line of vision are known, lose or gain 0·1 magnitude? (See "Our Astronomical Column," December 13, p. 160).—Dr. Van Romburgh (Buitenzorg) has examined the essential oils of *Polygala variabilis*, H. B. K., *B. albiflora*, *Polygala oleifera*, Heckel, and *Polygala Javana*, and found them to be nearly all methylsalicylate.—Mr. Jan de Vries: on a group of plane curves. This paper contains some theorems on plane curves ϕ of the $(n + m)^{th}$ order, with m^2 double points, (Δ), forming the base of a pencil of curves of the m^t degree.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, DECEMBER 27.

ROYAL INSTITUTION, at 3.—The Manufacture of an Electric Current: Prof. J. A. Fleming, F.R.S.

FRIDAY, DECEMBER 28.

ROYAL GEOGRAPHICAL SOCIETY, at 4.—Holiday Geography: Dr. H. R. Mill.

SATURDAY, DECEMBER 29.

ROYAL INSTITUTION, at 3.—The Current Working of a Chemist: Prof. J. A. Fleming, F.R.S.

SUNDAY, DECEMBER 30.

SUNDAY LECTURE SOCIETY, at 4.—The Action of Light on Bacteria and Fungi: Prof. Marshall Ward, F.R.S.

TUESDAY, JANUARY 1, 1895.

ROYAL INSTITUTION, at 3.—The Working of an Electric Current: Prof. J. A. Fleming, F.R.S.

THURSDAY, JANUARY 3.

ROYAL INSTITUTION, at 3.—The Working of an Electric Current: Prof. J. A. Fleming, F.R.S.

SATURDAY, JANUARY 5.

ROYAL INSTITUTION, at 3.—The Working of an Electric Current: Prof. J. A. Fleming, F.R.S.

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