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EPIGENESIS OR EVOLUTION.

Zeit- und Streitfragen der Biologie. Von Prof. Dr. Oscar Hertwig. Heft 1: Präformation oder Epigenese? Grundzüge einer Entwicklungs-theorie der Organismen. (Jena: Gustav Fischer, 1894.)

THE theory of preformation, or rather let us say predetermination, as revived at the close of the nineteenth century, is much more formidable than its prototype of the eighteenth century. Not only is it stripped of all its earlier crudities, such as the doctrine of *émboitement*, but it is supported by a mass of evidence accumulated by the researches of the last quarter of a century. Its fundamental assumption, that of the existence of minute, qualitatively unlike, ultimate particles of living matter, is strengthened, if not supported, by the analogy of the atomic theory, and the observed phenomena of mitosis accompanying the maturation of the ovum and spermatozoon, and the subsequent acts of impregnation and segmentation have been skilfully blended with the fundamental assumption in such a way that they are made to seem to be a proof of it. This strong position is now assailed by Dr. Oscar Hertwig, who is in many respects peculiarly well fitted to the task. He is the master of a simple, lucid and logical style, he has himself been, in conjunction with his brother, a pioneer in many of the discoveries on which the doctrine of predetermination is founded, and he has recently set himself to the task of verifying the experiments of Roux and others, and of examining the evidence which they afford for or against the doctrine which he attacks. His answer is unequivocal. The phenomena of development are to be explained on epigenetic, not on evolutionary grounds, and the latter hypothesis is contradicted by a number of well-ascertained facts.

In an introductory chapter Dr. Hertwig refers to and accepts Roux's definition of development, evolution, and epigenesis, which may be repeated here, as they give precision to terms which are often loosely used or little understood. By "development" is meant the origin of perceptible heterogeneity. Epigenesis means, not merely the formal increase of perceptible heterogeneity in a substance apparently similar but possibly extremely complex, but a real increase of pre-existing heterogeneity. Evolution means, the becoming perceptible of pre-existing latent imperceptible differences. Dr. Hertwig goes on to describe the positions taken up by Weismann and Roux, and deals particularly with the former, who, as he shows, regards the germ as a veritable microcosm, in which every separate variable part which appears in the course of the whole ontogeny is represented by a living particle; on the characters of these particles the characters of the parts of the adult organism, whether composed of one cell or many, depend. The sum of these particles forms the germ-plasm. Agreeing with Weismann that a theory of heredity must be founded on and brought into harmony with the cell theory (and therefore rejecting the opposite view of Nägeli), Dr. Hertwig proceeds to attack Weismann's fundamental assumptions. As he rightly says, the foundation and corner-stone of Weismann's theory is the assumption of differential or anisocleronomic[1] division of the cell nucleus. All-important as this assumption is, there is no foundation of fact to be found for it in all Weismann's work. Instead we find purely dialectical argument, and more than this, we find that Weismann has attributed the most opposite characters to his "idioplasm," declaring it, in one place, to be stable and unchangeable, in another to be labile and changeable. But, Hertwig points out, the facts are directly opposed to the assumption of anisocleronomic division of the germ substance. In the Protozoa the division is clearly isocleronomic, and we know of no instance among them in which the act of division, as such, is a means of producing new species. Moreover, the numerous cases (*e.g. Podophrya gemmipara*) of complicated life histories among Protoza show that the dissimilarity which may at first obtain between the two products of cell division is no indication of permanent and essential difference. The case of the lowest multicellular organisms is adduced as showing that in these also the cell division is isocleronomic, for each cell of which the soma of many of these organisms is composed, retains the power of giving rise to the whole organism. The phenomena of regeneration and heteromorphosis afford evidence that there are in the tissues of many highly differentiated organisms cells, or groups of cells, which retain, in a high degree, the power of giving rise to new and complicated structures, and this is particularly exemplified in cases of heteromorphosis, which is to be distinguished from regeneration by the fact that, in the former case, lost organs are replaced by organs which differ in form and function from those which were lost, or organs are, as a result of special conditions, produced in abnormal positions on the body. Under this head of phenomena of heteromorphosis, Dr. Hertwig groups the extraordinary phenomena which have been brought to light by his own researches and those of Driesch and E. B. Wilson on the segmenting ova of such different animals as frogs, echinids, and amphioxus, and these observations have been extended, since the publication of Dr. Hertwig's book, by the researches of Prof. Rafaello Zoja on the developing ova of cœlenterates. Briefly stated, the results of these experiments are as follows: In the case of echinids, amphioxus, and cœlenterates, the first two, four, or even eight blastomeres may, by suitable means, be isolated without impairing their vitality. Each blastomere, instead of giving rise to an incomplete embryo, or a portion of an embryo, begins the developmental course afresh, as it were, and produces an embryo perfect in all its parts, but one-half, one-fourth, or one-eighth the size, as the case may be, of the normal embryo. In the case of the frog it was not possible to isolate the blastomeres; but Hertwig was able, by pressure, to so alter the segmentation, that the normal relations of the blastomeres, one to another, were completely changed, and yet a perfectly normal embryo resulted. Unquestionably this proves that, in the first stages of segmentation, at any rate, the division of the germ substance is not differen-

[1] The German words "erbgleich" and "erbungleich" being untranslatable into English, I have coined the equivalents isocleronomic and anisocleronomic from the Greek Κληρόνομος an heir.—G. C. B.

tial but integral, not qualitative but quantitative, and this undermines the most important of Weismann's positions.

Very important are Dr. Hertwig's criticisms on the doctrine of determinants, so sharply and clearly defined by Weismann. This doctrine is inseparably connected with that of the anisocleronomic division of the germ-plasm, and it might appear that if the latter is disproved, it is unnecessary to enter into a detailed criticism of what depends upon it. But the criticism is not without its utility, since, Dr. Hertwig shows us, the conception of determinants is only an extreme instance of a false conception of causality common in current biological literature. Weismann has supposed that the ultimate vital particles, which he calls biophors, are grouped in the germ-plasm into "determinants," and that every smallest cell-group in the adult organism which has definite characteristics and a definite situation is represented in the germ-plasm, both of ovum and spermatozoon, by a definite determinant. These last are so arranged in the germ-plasm, and are endowed with such special forces, that they are able, in the course of ontogeny, to move at the right time into the right place—this movement being effected by the almost purposive anisocleronomic division of the germ-plasm. This very definite idea is founded on an erroneous conception of the relations between primordium (Anlage) and primordial product, which are supposed to stand in relation to one another as cause and effect. More or less unconsciously, the biologist commonly assumes that, because a given animal proceeds of necessity from a given egg, there is an identity between primordium and primordial product; so much so, that the developing organism is often spoken of as if it were a self-contained system of forces, a sort of organic perpetuum mobile. He overlooks the fact that for the fulfilment of the developmental processes many other conditions are necessary, without which the primordium could never arrive at the condition of its final product. Between the two there is clearly no identity, and it is false and mischievous to suppose, as the older evolutionists did, and the new evolutionists are again trying to make us believe, that the perceptible heterogeneity of the last stage of the developmental process is only the final expression of an invisible corresponding heterogeneity of the first stage. Throughout the whole of the ontogeny there is an exchange of material taking place, the adult has arrived not only to its bulk, but to its complexity as the result of metabolism; inorganic material is perpetually changed into organic, and serves for the growth and development of the primordia. It is true that the form changes are constant and invariable for the species, from a certain kind of germ a certain kind of animal is invariably produced; but is not this largely because, in the ordinary course of events, the ovicell is always subject to similar conditions of assimilation and excretion, and to similar conditions of gravity, light, temperature, &c.? Throughout the course of organic development things which were external are transformed into things internal, and the primordium grows and is changed at the expense of the environment. In thus recalling to our attention the fact that an organism is above all things metabolic, that its growth and changes are the result of its metabolic activity, and that its ultimate mass is the result of assimilation, of the taking up and making an integral part of itself of matter which was previously apart and different from itself, Dr. Hertwig does a real service to biology. He forces us back to the consideration that physiology and morphology are not two separate and independent lines of study, but that they are so closely interdependent that no generalisations can be made on the evidence of one kind of observation alone; they must be supported by equally cogent arguments from the other side. The theories of the evolutionists are essentially morphological, and in this they resemble the theories of the last century, that they take no account of one of the most wonderful of all vital phenomena, that of metabolism, but strive to find an explanation of the ultimate perceptible differences in form by asserting that the differences were always there, and have only expanded so as to become perceptible. Weismann's attempt to deal with this question by assigning the power of change as the result of metabolism to the biophors, does not really offer more than a purely formal explanation of the question, for what he predicates of the biophors may very well be predicated of the whole cell. Our ideas of increase of complexity as the result of metabolism are made none the clearer by shifting the responsibility of the change, if one may express oneself so, to subordinate parts.

It is indeed apparent, on reflection, that the characters of the perfected organism are not and cannot be the characters of a single cell—or even of a cell fusion such as the oosperm—and the converse of this is true that the characters of a cell cannot be the characters of the perfected organism. For what is a perfected metazoon or metaphyte but an aggregation of cells of most numerous and unlike characters? The characteristics of the perfected organism are the result of the correlation of all its parts, of the relations of cell to cell and of groups of cells to groups of cells; and are we to attribute to a single cell which has no relations characters which are essentially the results of the relations of innumerable cells one to another? Of the importance of the correlation of the parts of the perfected organism we can have no doubt, nor can we escape from the corollary that the characteristics of the organism reside not so much in the cells themselves as in the aggregation and interdependence of the cells, and if we may demur to the suggestion of the *colonial* character of the metazoa which is contained in the sentence, we must at least admit much of the truth of Hertwig's statement (p. 85):

> "That the ovum is an organism which multiplies itself by division into many organisms similar to itself, and that it is through the reciprocal action (Wechselwirkung) of all these many elementary organisms at each stage of the development that the organism as a whole is gradually and progressively established."

Dr. Hertwig institutes the comparison which was made long ago by Herbert Spencer, between an organism and a human society, and it is worth while mentioning his illustration as showing his conception of the relations of the cell to the organism. The organisation of a complex human society, he says, is something new, and not to be thought of as existing beforehand in the organisation of an individual man. It is nevertheless founded in human nature, but we cannot in gross, mechanical fashion seek for the organisation of a society in the primitive nature of man. In a like manner the character of the perfected organism is founded on the nature of the cells which compose it, but it contains in itself a new element, a heterogeneity due to correlation and reciprocity, which is limited by the specific nature of the cell substance, but is not to be sought for as a specific constituent of the substance of any individual cell. Starting from this point of view, we may consider Dr. Hertwig's own doctrines which he sets forth in the second part of his work entitled "Gedanken zu einer Entwicklungstheorie der Organismen." Whereas Weismann seeks the cause of the orderly development of the primordium in the primordial substance itself, Hertwig considers that the development of the primordium is dependent on conditions or causes which lie outside of the primordial substance of the ovicell, but are none the less produced in regular succession during the process of ontogeny. Such are, in the first instance, the reciprocal relations in which cells stand one to another in increasing degrees of complexity, whilst they increase in number by division; and in the second place, the action of the external environment on the organism. The argument in support of this proposition is given in so condensed a form that it is almost impossible to give any part or abstract of it without giving the whole. The following sentence is so important that it may be quoted at length, and the reader should refer to the work itself for the rest of the argument and the conclusion: —

"One of the most important and essential causes of the appearance of heterogeneity in the course of development is to be found in the specific power of the ovicell, to multiply itself by division. From this fact alone, that the nuclear substance is able, in the course of most manifold chemical processes, to assimilate, step by step, matter from the reserve material stored up in the egg, and oxygen from the surrounding atmosphere, it is able at the same time to evoke an ever-increasing heterogeneity. The increase of mass of the nuclear substance involves its progressive division into 2, 4, 8, 16 parts, and so forth. But the division is again the cause of a constantly changing spatial distribution of the substance. The 2, 4, 8, 16 and following nuclei which arise by division give way to one another in opposite directions, and attain to new positions at definite distances from one another within the limits of the egg. Whilst all the material particles of the ovum were at first arranged round the fertilised nucleus as a single centre of force, they are now grouped around as many individual centres as there are new nuclei, and they segregate themselves around these as cells. It is therefore clear that the ovum as an unicellular organism, when compared with the ovum as a multicellular organism, has altered its quality to an important extent, and that merely by the process of isocleronomic division."

This is what Hertwig calls the function of growth as a form-producing principle. The other principles which he invokes are the relations of the cells to external conditions, or the function of their position, and finally the reciprocal influence of the parts of the whole on one another and on the whole, or the function of correlation. It is not to be denied that these are principles of considerable importance, and it may be said that their importance has never quite been lost sight of, but they do not bring us any nearer to the explanation of the totally different reaction of apparently similar substances to the same stimuli. What we want to know is why, when we place under a hen her own eggs and those of a duck, and so expose them to identical conditions, chickens and ducklings are hatched as unlike one another as may be. The answer given, "the difference can be due to nothing else than to the different nature (the different micellar structure) of the substance," is unsatisfactory, in that it is only a restatement of the fact, and is not an explanation at all. In attempting to give a more definite account of the different natures of the egg substance, Hertwig is obliged to take his stand upon much the same ground as the evolutionists. "In the hen's egg the species is present as fully as in the hen, and the hen's egg is as different from the frog's egg as the hen is from from the frog." He is compelled to agree with the evolutionists in assuming the existence of a specific, and even of a very highly organised primordial substance as a basis of developmental processes, but he claims that his concept of this substance is different from and better than theirs, in that he ascribes to the primordial substance or germ-plasm characters which are congruous with the concept and character of a cell, and does not ascribe to it the innumerable characteristics which are only evoked through the union of many cells and the concomitant action of external conditions. The distinction appears to be a slight one, yet on careful consideration it assumes more important dimensions. Hertwig, if I interpret him rightly, conceives of the germ-plasm as a substance of many and definite potentialities. He does not attribute this potentiality to one part, and that to another part of the germ-plasm, but argues that as a result of multiplication by division the relations of the cells with their contained germ-plasm are continually undergoing change, both with regard to one another and to the environment. In consequence of the different conditions thus induced, this potentiality is evoked in the germ-plasm of one cell, that in the germ-plasm of another, and so on in ever-increasing grades of complexity. The differentiation of any cell is the result of its position, which determines which of its many potentialities shall be called into action. It is the reaction of the living substance to the stimulus which evokes a particular potentiality, which brings about its form changes, and it may be supposed that a profound form change following on constant action in one direction incapacitates the cell in question for the performance of any of the many other duties for which it was primitively fitted. This conception is epigenetic in that it admits the coming into being of a new heterogeneity which was not pre-existent in the ovum as such; that which was present was a capacity for certain kinds of heterogeneity. Very different is the conception of the evolutionists, who define the exact potentialities of the germ-plasm, and assign each to a given material particle or group of particles. The heterogeneity is already present; thenceforward there is no room for the increase of complexity in response to external stimuli. Dr. Hertwig rightly says that Weismann's explanation amounts to nothing more than a renunciation of an explanation. His doctrine of determinants leads us into an invisible world in which there is no foothold for research. For this reason, if for no other, we should welcome Dr. Oscar Hertwig's invitation to return to the paths of epigenesis. A theory which has a formal answer for every question, which regards everything that we can see and lay hold of as predetermined and unalterable, which relegates the causes of phenomena to the unseen and unknowable—such a theory, if accepted as true, does not stimulate, but stifles inquiry. Fortunately it has had the opposite result. It has not been accepted, and it has developed an attack of a brilliant and overwhelming character. All the best arguments which Dr. Hertwig can bring against the theory of predetermination are derived, not from simple observation, but from experiment. A few simply conceived interferences with the normal course of the segmentation of the ovum have sufficed to strike down the doctrine of determinants. May we not hope that an extension of these methods may illuminate the regions which are still hidden from us? After all his attempts to supply an acceptable alternative to Weismann's scheme, Dr. O. Hertwig makes a partial confession of failure. To many his failure will seem complete, and it must be so since the evidence derived from experiment is as yet wholly inadequate. But his attempts indicate the paths along which research may be conducted, and he is very right when he claims, in conclusion, that it is the great merit of his conception of the developmental processes, that it opens the gates once more to research, with some brighter hope of results than the formal theory of predetermination afforded us.

G. C. Bourne.

COAL-DUST AND COLLIERY EXPLOSIONS.

Coal-Dust an Explosive Agent, as shown by an Examination of the Camerton Explosion. By Donald M. D. Stuart. (London: Office of the *Colliery Manager*, and E. and F. N. Spon.)

It is significant of the conservatism—not always wholly disinterested—that surrounds an old-established industry that, in spite of all that has been written and said on the action of coal-dust as an explosive agent during the last twenty years, we should still find people persistently clinging to the belief that the only possible cause of a colliery explosion *must* be fire-damp. It would be amusing, were the matter less serious, to note the extraordinary hypotheses and absurd surmises to which the believers in this time-honoured doctrine are occasionally driven in order to account for the existence of fire-damp in places where the common testimony of unbiassed people affords no proof of its presence. The hard logic of facts is, however, surely, even if slowly, undermining the mass of prejudice with which this question has been surrounded, and we may hope that before the close of the century the action of the Legislature will compel these people, whose obstinate unbelief jeopardises men's lives, to give practical heed, even more directly than at present, to the teachings of intelligent observation and inspection. The causes and conditions which lead to a colliery explosion are now so well understood that such a catastrophe ought to be no longer possible. If it does occur, we must lay the blame on the management and discipline of the mine, and it should not be difficult, under these circumstances, to fix the responsibility. The master of a vessel who carelessly navigates his ship, is liable to have his certificate dealt with in a very summary fashion. It does not avail him to plead that his crew are picked from a class that is proverbially reckless and foolhardy, and that his "look-out," therefore, was probably in fault. The Assessors take it for granted that he has a proper knowledge of his business, and they hold him responsible for the discipline on his vessel. Public opinion demands that a mine-manager should be treated in a similar manner. How difficult it is for the the law, in spite of the length of its arm, to get at a manager who has been guilty of culpable carelessness, has been shown in more than one inquiry that could be named.

As an illustration of the mental attitude to which we allude, we may refer to a recent report on the cause of the explosion at the Albion Colliery, South Wales, in June last. All the circumstances connected with that explosion seemed to indicate that it was due to the same cause as that which accounted for the explosions at the Park Slip, Apedale, and Malago Collieries, viz. the presence of coal-dust, and this conclusion was confirmed by the report of the inspectors appointed by the Home Office to inspect the colliery.

In a report prepared for the colliery proprietors by six engineers, we find that these gentlemen are unanimously of opinion that the disaster was caused by an outburst of fire-damp, and they have great satisfaction in stating that no blame in the matter can be attributed to any of the officials or employés. There is no wonder, in view of this conflict of testimony, that the men of the Rhondda district should have demanded a fresh inquiry by the Government into the cause of the disaster.

The changes of opinion among "practical" men on this question of coal-dust are very suggestive, and strikingly exemplify the course through which a new truth has to run when it is in conflict with the settled conviction of interested persons. Like the course of true love, it does not run at all smooth under these circumstances. The idea that coal-dust could be the cause of a colliery explosion was, in the outset, scouted as absurd. Then, as facts multiplied, the dust was allowed to have a share in the catastrophe: it aggravated the violence of a fire-damp explosion. Next, the proportion of the fire-damp became smaller by degrees, until it reached the vanishing-point. Now we have reached the stage that all dusts are not explosive, and the colliery manager is satisfied that his dust is not as other men's dust. Even in the case of those who were more receptive of the teaching of experiment and of trained observation, the recognition of the real facts has had to run the conventional course. First they were not true, then they were not new, and we knew of them before; for did not Faraday and Lyell tell us all about the matter in the Haswell report? In a question of this kind, colliery management ought to be in advance of public opinion. That it has not always been so in the past, the history of coal-mining shows only too plainly. It was the shock to public sentiment, caused by a succession of disastrous explosions, which occurred in the early part of this century, that indirectly brought about a revolution in the art of coal-getting. People in the colliery districts, who were witnesses of the terrible loss of human life, and of the misery and destitution that followed it, asked their fellows if this ought to be part of the price to be paid for coal, and the answer was given in no uncertain tones. Government inspection was insisted upon, and, in spite of persistent opposition, eventually obtained. In the outset, the character of the inspection was not always what it should be; but, little by little, this has improved. We have a better trained class of men sent out by the Home Office now than formerly, and their hands have been gradually strengthened by the Legislature, although, perhaps, not to the extent that is desirable.

Statistics show that this intelligent inspection is gradually making its influence felt. Tested by the ratio of fatalities to number of men employed, and to amount of material raised, there is a slow but decided improvement. Of course, even under the most ideal system of inspection, coal-mining will continue to be a hazardous occupation; but this at least we may hope, that the steady sacrifice of 1000 victims a year, which that ruthless potentate, Old King Coal, seems to demand, shall not continue to be augmented by catastrophes that ought to be considered as preventable.

Mr. Stuart's book is to be welcomed, therefore, as adding one more link to the chain of evidence which establishes the fact that coal-dust may be the most important and, at times, even the only agent in bringing about a colliery explosion. About a year ago an explosion occurred at the Camerton Collieries in Somersetshire, the significant feature of which was that it took place in a mine wholly free from fire-damp. All the circumstances connected with this explosion were brought to the knowledge of Mr. Chamberlain's Commission by Mr. Garthwaite, the general manager, and were fully inquired into by H.M.'s inspectors. Mr. Stuart's examination was made independently of the official investigation, and it is satisfactory to note that as regards the main conclusion there is absolute unanimity on all sides. There can be no possible doubt, therefore, that the explosion of November 14, 1893, at the New Collieries, Camerton Court, was due to coal-dust, and to coal-dust alone, initiated by a gunpowder shot, and most probably by what is technically known as a "blown-out shot." Mr. Stuart's examination showed that there were no "extraordinary circumstances" present; the shot-firing was an ordinary operation, the presence of coal-dust was a normal circumstance, and the work was being done by a competent man. "The angle and declivity of the hole were such, that if the gunpowder were expelled, it would directly strike the dust; but it was so placed in the judgment of an experienced miner. . . . In the presence of this explosion, therefore, conjecture upon the supposed innocuousness of a gunpowder shot in a dry and dusty non-gaseous mine is at an end."

Whilst we wholly agree with Mr. Stuart in this conclusion, we are not altogether at one with him in regard to his explanation of the chemical process of a coal-dust explosion. Mr. Stuart appears to be of opinion that a coal-dust explosion is in reality only another form of a gas explosion. The action of the heated products of the exploded gunpowder is, he assumes, to cause the dust to experience a kind of destructive distillation whereby hydrogen and gaseous hydrocarbons are formed, which at the high temperature combine explosively with the oxygen of the air of the mine. Whether there is any necessity to invoke this distillatory process as an explanation of the phenomena, is extremely doubtful. Without expressing any final opinion on this point, it may be pointed out that no definite relation between the bituminous character of the dust and its "sensitiveness" as an explosive agent has been established. The dry mines of South Wales, where some of the most formidable explosions of recent times have occurred, yield a dust which is relatively rich in carbon, and which affords no very large quantity of gas on distillation.

We would commend this book to the thoughtful attention of every colliery manager, with, however, the reservation that Mr. Stuart's theories are not to be accepted as of the same value as his facts. So long as he confines himself to the orderly arrangement and analysis of these facts he is on perfectly safe ground. The weakest portion of the book is that in which the author seeks to elucidate the chemical and physical phenomena of a coal-dust explosion, by the application of imperfect thermal data and of irrelevant chemical observations.

THE MODE OF LIFE OF MARINE ANIMALS.

Die Lebensweise der Meeresthiere. Beobachtungen über das Leben der geologischwichtigen Thiere. Von Johannes Walther. Zweiter Theil einer Einleitung in die Geologie als historische Wissenschaft. (Jena: Gustav Fischer, 1893.)

THIS is the second part of Prof. Walther's projected extensive geological treatise, the first part of which —on the Bionomy of the Sea—appeared some time ago. Of the three titles given, the second, or subsidiary one, seems best to describe the scope of the present book. It is not, as might be supposed from the primary title, a treatise on the physiology of marine animals—would that it were! that is still a great desideratum in biology —but is rather some observations on certain points in the life-relations, or mode of occurrence, of certain marine animals, viz. those which are of importance to the geologist [and no less to the biologist] as being the present-day representatives of former animals now preserved as fossils. Walther's idea is that we must study the relations of organisms to their environment at the present day, before drawing deductions from fossil remains as to the physical conditions of past geological periods. His object is to lay a sound foundation of fact, as to the mode of occurrence of particular sets of animals, upon which to base an account of the history or development of the events chronicled in the rocks. The idea is a sound enough one, if not very original—it must surely have been present, consciously or not, in the minds of various geological and biological writers—and the conclusions arrived at, if really based upon a sufficiently large accumulation of statistics, will no doubt be a valuable guide to the geologist in forming his opinions. The book, if very complete in its series of facts, would also be a useful reference work to the zoologist; but it may be doubted, on an examination of the lists given by

Walther, whether they are a sufficiently exhaustive compilation to inspire thorough confidence.

For example, the information as to the geographical distribution of species is rather unequal, being detailed in some cases, and decidedly meagre in others, as when for *Lagena sulcata* is given only "im Mittelmeer," and when for *Crania anomala* the only north-west European locality is the Clyde! While, on the other hand, such minute local detail is given as that *Globigerina bulloides* is not uncommon in the brackish water of the Dee from Chester to Hilbre Island. A number of detailed criticisms of this kind might be made, such as the extraordinary entry "*Lafoëa*, 450 faths." when several species of the genus are found in quite shallow water. But probably enough has been said to show that the lists are by no means complete.

The plan of the book is, briefly, as follows: first, the gaps in the palæontological record, and their causes, are discussed; then the following groups are treated in succession: Foraminifera, Radiolaria, Spongia, Anthozoa, Crinoidea, Asteroidea, Echinoidea, Holothuroidea, Bryozoa, Brachiopoda, Lamellibranchiata, Gastropoda, Cephalopoda, and Crustacea. A few general questions are discussed. The author alludes to the well-known fact that some of the most abundant groups in the sea are almost unrepresented in the fossil series, and that even amongst animals with hard parts the fossils of a particular bed might inadequately represent what had been the living assemblage at that spot. He quotes Edward Forbes' account of the natural history of a shell-bed off the north-west of the Isle of Man, and his later observations in the Ægean Sea, to show that even the fresh dead remains of organisms on the sea-floor do not always correctly show the relative abundance of the living species.

In each group, after a short account of the characters, mode of occurrence, &c., there follows a list of genera and species, with an indication of the distribution and range in depth, compiled from *Challenger* reports, monographs, and other sources; but there is a want of correlation and digestion of the facts, the nomenclature is not up to date, and the same species sometimes occurs several times under different names; *e.g.* on p. 308, *Ophiothrix fragilis* appears three times under the names *Ophiocoma rosula*, *Ophiothrix fragilis*, and *Ophiothrix rosula*, with a different range in depth each time. Occasionally an animal is found in the wrong group altogether, as, a Holothurian amongst the Asterids, and an Ascidian in the Gastropods. However, Prof. Walther has brought together a considerable amount of material which those who are interested in the distribution of animals in the sea, and the association of species to form "faunas" characteristic of particular regions, will have to utilise. For this the marine zoologists and the geologists will no doubt be grateful, and will, with profit, consult the lists; but I fear they will also sometimes regret that the author had not taken more pains to digest his facts and to correct his proofs. Many odd pieces of interesting information are given; but there is still room in some book on marine faunas for a detailed account of characteristic assemblages of animals with as full a description as can be given of their physical surroundings and their variations.

W. A. H.

OUR BOOK SHELF.

Elementary Qualitative Chemical Analysis. By Prof. Frank Clowes, D.Sc., and J. B. Coleman. Pp. 180. (London: J. and A. Churchill, 1894.)

Tables and Directions for the Qualitative Chemical Analysis of Moderately Complex Mixtures of Salts. By M. M. Pattison Muir, M.A. Pp. 44. (London: Longmans, Green, and Co., 1895.)

Laboratory Exercise Book for Chemical Students. By E. Francis, F.C.S. (London: Blackie and Son.)

THE first of these books is an abridgement of Prof. Clowes' text-book on qualitative analysis, adapted for use in the laboratories of schools and colleges. For the most part, the book is like a host of others of the same kind. It differs from many of them, however, in the fact that the first fifty pages is devoted to instructions on the preparation of apparatus, to experiments illustrating the preparation and properties of certain gases and liquids, to descriptions of analytical operations, and directions for the performance of ordinary processes of chemical manipulation. Work of this character forms by far the best introduction to a course of practical chemistry, and it has an educational value, which is more than can be said for mere test-tubing. On account of this and one or two other notable features, the book will probably take a permanent place among laboratory guides.

"These tables and directions" (writes Mr. Pattison Muir) "are intended for the guidance of students who are acquainted with the principles of qualitative analysis, and who are able to make a qualitative analysis of a simple salt, and of a mixture of salts containing not more than a single metal in any one group, and three or four of the common acids." The student who has passed through an elementary course of practical chemistry is frequently puzzled how to conduct an analysis of moderately complex mixtures of salts and the commoner metals and acids, or an analysis of metals and alloys. Mr. Muir's book tells exactly what to do in such cases. By following the directions given, it would hardly be possible for the young analyst to go wrong. The processes described are easily carried out, and are concisely stated. A point worth noting is that the formulæ of solids are printed in heavy type; of liquids or substances in solution, in ordinary type; and of gases, in italics. This method of indicating the physical states of substances certainly possesses advantages. Altogether the book is a handy and trustworthy manual for analytical chemists.

The exercise book arranged by Mr. Francis has apparently been designed to take the place of the laboratory note-book. It opens with a few exercises in practical chemistry, the experiments being briefly—sometimes too briefly—described; and blank spaces are left for the entry of results. Then come a set of analytical tables, and a number of blank forms in which all the steps in the analysis of a mixture of two simple salts are indicated, spaces being left for the student to fill up with his inferences. The average student of practical chemistry works like a machine now, and we have no doubt that these tables will be after his own heart, for they only leave him to fill in his observations as if he were answering the questions in a census paper. The book may serve to drill the student into carrying out his tests in the proper order, but it will not benefit him mentally.

Elements of Astronomy. By G. W. Parker, M.A. (London: Longmans, Green, and Co., 1894.)

THIS is one of the books in which astronomy seems to be regarded as a subject which is to be studied much in the same way as one would take up an additional book of Euclid. It abounds in definitions, propositions, and corollaries; the diagrams of instruments scarcely give any ideas of what they are intended to represent; and the descriptive part of the subject might have been omitted without much sacrifice. The ground covered is that which is ordinarily understood by an elementary treatment of mathematical astronomy, dealing chiefly with the considerations relating to the positions, movements, dimensions, and distances of the various heavenly bodies, but includes also some very scanty references to their telescopic appearances. On the whole, the various points are clearly, though shortly, explained, but there is much to suggest that the author would be all the better for some little observatory practice; for example, his method of determining the angular value of a micrometer by means of the sun (p. 48) is scarcely practicable, and a sun-spot 13,000 miles long is by no means to be classed as one of the largest spots (p. 68). It may be pointed out, also, that a single observer, by observing at intervals of twelve hours, gets better results for the parallax of Mars than two working in the way indicated on p. 115. A ship's mean time at sea, too, is usually determined by one observation near the prime-vertical, and not by the method of equal altitudes.

In less than a dozen pages the author attempts to give an idea of the classification of the stars, and of "the principal discoveries which have been made in modern times, chiefly by means of spectroscopic analysis, with their nature and physical condition" (p. 203). The omission of the solar prominences in an account of the phenomena of a solar eclipse, is a good indication of the very feeble character of this chapter.

The book is intended specially for students preparing for University examinations, and by such it may be found useful.

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.*]

Exploration at Ruwenzori.

PERHAPS it may interest your readers to give a short account of Ruwenzori, where I have now spent four months. The mountain is a very difficult one to study, on account of the difficulty of reaching the most interesting part. Taking a sort of botanical section from the shore of the Albert Edward Nyanza, one finds first a series of grassy plains covered with Andropogous some two feet high, and in certain months supporting large herds of elephants, Kudu, and Lurwali antelopes. This is in part the old level of the lake, and in part gravel and sand brought by the numerous rivers; in places it is dotted by Acacia and the tree Euphorbia, which has something of the appearance of an enormous chandelier. After leaving this plain, one comes to a series of small hills from 4000 to 5000 feet in height, which have been apparently cut out of the mountain by the numerous rivers and streams. Some of these are covered with patches of cultivation, banana plantations, &c.; usually these are hidden from the main road. When one reaches the mountain proper, one finds up to 7000 feet a steep ascent covered with grass and small shrubs, usually three to four feet high. The valleys in this part are usually very steep V-shaped trenches, and cultivation is abundant everywhere, sometimes over 7000 feet, and in the Wakondja country the edible Arum is grown up to 7400 feet or more. This height, 7000 to 8000 feet, marks the beginning of the forest. It is composed of deciduous trees, sometimes with a very thick undergrowth; sometimes it is pretty open, with a profusion of fern and moss on the old trunks, and creepers in some places. I have found tree ferns and Begonia, but usually the flowers are rather pale in colour, or quite inconspicuous. At 8600 feet another distinct change takes place, and a wilderness of decaying young and mature bamboos replaces the trees. Here and there these are hung with creepers, but the predominant feature is the wetness of everything. Moss covers almost every trunk below, and amongst the roots are only very watery plants, such as Urticaceæ. At 9600 feet another change takes place; bamboos disappear completely, and tree heather takes its place. In a dry part of the mountain one finds a charming little violet, a Cardamine, Galiums, Epilobium, Rubus, &c. In wetter places one finds a regular peat-moss with Sphagnum, beautiful orchids, and short heather; in another place one will find enormous trees of heather, usually gnarled and twisted in growth—tree Senecios, tree Hypericums, &c. This region seems to extend to the snow (which, I am sorry to say, I have not been able to reach). On my highest attempt, I could see the heather trees apparently higher than the snow. On another attempt to get to the summit, I found what seems to me *Alchemilla alpina!* One feature of the mountain is the extreme scarcity of animals and birds. In the lower forest there are bushbuck, baboons, and two other sorts of monkeys; one the magnificent black and white-furred kind from which grenadiers' shakos were made, and another with short slatey fur, which is new to me.

Perhaps the commonest birds are the sunbirds; one green, yellow, and crimson, I have seen above 10,000 feet, and I have also seen (though I am almost afraid to say it) a robin, and a goldfinch.

As to the geology of the mountain, I do not care to risk an opinion at present, but I have taken many specimens which I hope may solve some of the questions. I think glaciers must have extended seven to eight miles down two of the valleys, but there was no evidence to my mind of any extensive glaciation. I think I am right in saying that the Salt Lake is nothing but an extinct volcanic crater, and in several places along the east side of the mountain there are others, or a small chain of volcanoes; usually the chain radiates from the centre of the mountain.

I hope to start to-morrow for Ujiji, my object being to see whether a practicable route exists from Tanganyika to the Albert Edward Nyanza. I only hope I shall be able to bring my collections safely home. G. F. SCOTT ELLIOT.

Salt Lake, Ruwenzori, August 2, 1894.

The Alleged Absoluteness of Motions of Rotation.

I MUST confess that discussions upon mathematical metaphysics appear to me to be somewhat unpractical. They are suggestive of the case upon which Serjeant Snubbin was engaged, which related to a right of way "leading from some place which nobody ever came from, to some other place which nobody ever went to." Nevertheless, I propose to offer some remarks upon this subject.

That *absolute* motions of translation and rotation exist appears to me too clear for argument; but whether our senses are capable of taking cognisance of them and reducing them to exact measurement, is quite another matter. The view advocated by Prof. Greenhill appears to be that motions of rotation are determinate—that is to say, they are capable of exact measurement within the limits of experimental error; whereas the contrary is the case with motions of translation. Mr. Love, on the other hand, holds that neither kind of motion is determinate in the above sense. Now a knowledge of the absolute value of the velocity of translation of any object involves a knowledge of the magnitude and direction of the sun's velocity in space; and until the latter has been determined, which has not yet been done, the former is necessarily unknown. It therefore follows that all motions of translation of which our senses are capable of taking cognisance are *relative*.

On the other hand, the motion of rotation of any object is independent of the motion of translation of the sun or any other body. If, therefore, it were in our power to construct a system of axes which either move parallel to themselves or whose angular motion is known, it would be possible to determine the absolute value of the angular velocity of any object. But even if it were possible to devise an experiment by which such a system of axes could be obtained, our results would only be accurate within the limits of experimental error. It may, therefore, be well to point out that, without inventing any new experiment, the angular velocity of any object may be accurately determined *within the limits of experimental error* in the following manner.

Select two stars, X and Y, whose proper motions are so minute that they have never been detected by the most refined observations. Through the sun and the two stars draw a plane, S X Y, and through the sun draw a line, S Z, perpendicular to this plane; and let the motion be referred to S X, S Z, and a line perpendicular to them as axes. Now in consequence of the proper motions of the sun and the two stars, these lines will not constitute a system of axes which are either fixed in space or which move parallel to themselves; consequently, if these lines be taken as axes of reference the observed (or calculated) value of the motion of rotation of any object will necessarily be *relative* to these axes, and its *absolute* value cannot be determined without ascertaining the angular motion of the axes of reference. But since the two stars have been specially selected from amongst those whose proper motions have eluded detection, the angular motions of the axes will be so small compared with ordinary standards that the error caused by neglecting them will lie within the limits of experimental error. If, for example, we were to endeavour to calculate by the above method, or some similar one, the *absolute* angular velocities of the hands of a clock, or of a fly-wheel, the value which we should thereby obtain would be as exact as the existing state of mechanical and instrumental appliances admits.

A series of observations extending over several thousand years might of course reveal a proper motion of any two selected stars; but in the case of angular motions whose periods do not extend much beyond a century, the above method leads to results which are for most practical purposes exact.

I am therefore disposed to think that all motions of rotation of which our senses are capable of taking cognisance are *relative*; but with the exception of certain astronomical motions of very long periods, the *absolute* value of any motion of rotation which can be observed may be accurately determined within the limits of experimental error. A. B. BASSET.

Fledborough Hall, Holyport, Berks.

THE gist of Mr. Love's long letter, on pp. 198-9, seems to be that since the specification of a force or a motion depends upon the choice of axes, therefore the force or motion itself is similarly dependent. A slight extension of the same principle would make a velocity—say the velocity of light—depend upon whether it was to be expressed in miles per second or in centimetres per hour; and no extension at all is necessary to make it depend on whether it is referred to Groombridge 1830, or to some more quiescent body, if indeed the term quiescence may be allowed henceforth to have any meaning.

Tycho Brahé is said to have held that there was only a question of language between the Copernican and the Ptolemaic systems; but, with the exception of a semi-ironical Church compromise attributed to Descartes, he has been unfortunate in not finding a disciple of importance until the present moment.

It appears now to be equally true to say that the earth rises to meet a stone, as to say that a stone falls to meet the earth; and considerations of energy are of no consequence!

I just want to add one word of my own on the subject, to the effect that whereas the *position* of a body in an infinite homogeneous stagnant ocean would be unmeaning and unspecifiable, except by reference to boundaries or other bodies, yet it does not follow that the *velocity* of a body through such an ocean would be either unmeaning, unspecifiable, or undiscoverable by experiment. It may be replied that such motion would still be relative to something; and to that I say by all means, but it is not relative to other bodies such as are competent to fix *position*, which is what Mr. Love is contending for.

As to the other question, about absoluteness of rotation, I shall be much interested in seeing what Prof. Greenhill, and with him Profs. Mach and Karl Pearson, have to say on the point. OLIVER J. LODGE.

Liverpool, December 29, 1894.

The Quarrying of Granite in India.

At Bangalore, in Southern India, the quarrying of granite slabs by means of wood fire has been brought to such perfection, that an account of the method may not be out of place. The rock is a grey gneissose granite of very irregular composition through unequal segregation of hornblende and the presence of numerous felspathic veins. But it is otherwise very compact, and forms solid masses uninterrupted by cracks for several hundreds of feet. Only near the surface the rock is found split parallel to the surface. In one quarry there is thus, for instance, a 4-feet thick horizontal layer of rather weathered rock, underneath this another layer of fresh rock 3 feet thick; but below this the rock is entirely fresh, and not split. These layers are probably due to the variations of temperature, daily and seasonal.

The undisturbed rock is quarried by means of fire, and it is remarkable what large plates may be detached. I saw one plate of 60 feet greatest length, and 40 feet greatest width, and half a foot thickness. This thickness varied only one inch over the greater part of the area. The whole plate had been detached in one piece by means of wood-fire. Afterwards the plate was cut with blunt chisels into strips of 2½ feet width. So easily are these strips and slabs obtained, that it is quite common to see palisades of them used instead of boundary walls, and also to see them used as posts for huts, for telegraphs, and for railings and posts in gardens.

In one case, I observed the operation of burning over an area. A narrow line of wood-fire, perhaps 7 feet long, was gradually elongated, and at the same time moved forward over the tolerably even surface of solid rock. The line of fire was produced by dry logs of light wood, which were left burning in their position until strokes with a hammer indicated that the rock in front of the fire had become detached from the main mass underneath. The burning wood was then pushed forward a few inches, and left until the hammer again indicated that the slit had extended. Thus the fire was moved on, and at the same time the length of the line of fire was increased and made to be convex on the side of the fresh rock. The maximum length of the arc amounted to about 25 feet. It was only on this advancing line of fire that any heating took place, the portion which had been traversed being left to itself. This latter portion was covered with the ashes left by the wood, and with thin splinters which had been burst off. These splinters were only of about ⅛th inch thickness, and a few inches across. They were quite independent of the general splitting of the rock, which was all the time going on at a depth of about five inches from the surface. The burning lasted eight hours, and the line of fire advanced at the average rate of nearly 6 feet an hour. The area actually passed over by the line of fire was 460 square feet, but as the crack extended about 3 feet on either side beyond the fire, the area of the entire slab which was set free measured about 740 square feet. All this was done with, may be, about 15 cwt. of wood. Taking the average thickness of the stone at 5 inches, and its specific gravity as 2·62, the result is 30 lbs. of stone quarried with 1 lb. of wood.

The old quarries have sloping sides formed of steps left by each successively split plate, each new plate extending to within about 2 feet of the step left by the preceding plate. Many plates are taken out in an inclined position, and as the directions of inclination differ, it follows that the action of the fire is quite independent of the original surface of the rock, and also of the direction of lamination and of the numerous veins in the rock. The action of the fire is thus very similar to that action which produces dykes and faults on a large scale, more or less independent of the nature of the rocks which are passed through.

The great uniformity of the thickness of the slabs formed by the above process is probably due to a regulating influence of the pre-existing crack. When the action of the fire is somewhat slower, it takes longer for the heat to penetrate down to the crack; when the action is quicker, there will be enough expansion produced in the upper layers, and the lower layers transmit the tension to the plane of the crack. Perhaps it will be possible some day to measure the temperature of the heated rock, when a certain agreement ought to be found between the tensile strength of the rock and the strain which the expansion by the heat produces in the so-far elastic rock.

Bangalore, December 19, 1894. H. WARTH.

Storm Statistics at Bidston.

THE Liverpool Observatory, erected at Bidston, on the Cheshire side of the estuary of the Mersey, stands on a slight eminence about 200 feet above the sea-level. The ascent is tolerably steep on each side except from the south, and with the Irish Sea on the north, and the rivers Mersey and Dee on the east and west sides respectively, there is nothing to obstruct or diminish the force of a passing storm. Self-recording anemometers of the Robinson and Osler types have been in position since 1867, and it is from the records of these instruments that these statistics have been prepared. How far these records are absolutely correct is beside the present question; all the data have been obtained with the same instruments, and are strictly comparable between themselves.

It has been the custom at the observatory to treat as storms or gales, all occasions in which the Robinson anemometer recorded the horizontal velocity of the atmosphere as equal to or exceeding fifty miles an hour. The total number of instances in the twenty-eight years under consideration is 321, or an average slightly below one a month. It will frequently happen in stormy periods that the critical velocity will be registered, and be followed by a partial lull in the storm, to be succeeded by gusts of greater force. If these periods of comparative calm have lasted for about twenty-four hours, the disturbance would count as two storms, although probably both are parts of the same atmospheric disturbance.

The greatest number recorded in a year is in 1868, when no less than twenty-eight gales were reported; and the least occurs in 1880, when there were only two, and these neither long in continuance nor great in violence. It is not without significance—though, of course, it is not intended to insist upon the coincidence—that when the number of storms is plotted as an ordinate with the time for abscissa, a rough curve can be drawn among them giving maxima at practically equal intervals of five years, from 1868, with minima at intermediate dates. Or, if we take the sums of the maximum years 1868, 1873, 1878, &c., it is found that in the six we have records of eighty-three storms; while in the six years of minimum, 1871, 1875, 1880, &c., we have only thirty-seven.

It is scarcely necessary to refer to the direction in which storms approach the observatory, after what has been said of its geographical position. Roughly, they are all from the west, with slight deflections to the north and south. As a matter of fact, only five have deviated from this rule, and they have been either east or south-east, and have been comparatively slight in their character.

The time scale on which the velocities have been registered is not a very open one. The recording drum moves through rather less than one inch in an hour, and the habit has been to read and record the distance travelled from the commencement of one hour to the beginning of the next. The maximum hourly velocity is, therefore, not to be understood as the greatest in any sixty consecutive minutes, but as the greatest in one whole hour as marked by the clock. In this sense the following table, which exhibits the number of times the greatest hourly velocity has exceeded noticeable amounts, is to be understood:—

Recorded hourly velocity in miles.	No. of instances
50–60	220
60–70	68
70–80	21
80–90	10
Exceeding 90	2

Here again, curiously enough, we find some slight evidence in favour of a five-year period. Not only do the two instances of the greatest velocity recorded in the observatory occur in years already noticed as those of maximum disturbance, but the average velocity of all storms in the years 1868, 1873, &c., is 59·2 miles, as compared with 57·9 miles in the years of minimum number.

Intimately connected with velocity, though probably a less accurate measure of the true force of a storm, is the pressure recorded per square foot. Here Bidston has long held a record for the British Isles, having placed to its credit a pressure of ninety pounds on March 9, 1871. The accuracy of this measure has often been questioned, and probably it is too great owing to the momentum in the moving parts of the machinery, but it is certainly the record of a far greater pressure than has ever been witnessed since. Considering over what small areas these excessive pressures are exercised, and the great variation that exists from moment to moment in the velocity of the wind when a storm is raging, it is not an impossible amount, but it would certainly be misleading to conclude that such a pressure was a measure of the force of the wind a few feet, or even a few inches, away from the pressure-plate. The hourly velocity on the occasion when this pressure was registered has frequently been exceeded, without reproducing similar pressures. Herein is represented the great difficulty in determining a simple relation between pressure and velocity, or, rather, the square of the velocity; for one may regard Hutton's law of wind-pressure on a given obstructing surface as satisfactorily proved. The fact of such accidents as that referred to destroys any value that can be drawn from averages, but as a mere matter of figures, it appears that the average maximum hourly velocity for all storms is 58·4 miles, and the mean of the maximum pressures 37·6 lbs. This would require the factor for multiplying the square of the velocity in miles, to obtain the pressure per square foot, to be greater than one-tenth, which is evidently and necessarily erroneous, since we are comparing the accidental momentary pressure with the average velocity obtained throughout the entire hour. Taking the extreme pressures for what they are worth, the numbers come out as follows:—

Pressure in lbs. on sq. ft.	Number of instances.
20–30	132
30–40	111
40–50	50
50–60	11
60–70	12

On two occasions the pressure registered was greater than 70 lbs. to the square foot, and on three did not reach twenty. Since the extreme pressures are in a sense accidental, and do not represent with any accuracy the force of a storm, it does not seem desirable to determine the relative pressures in what have been called years of maximum and minimum storm occurrence. But the general features are again borne out. The explanation here is probably that the greater the number of storms the greater the chance of finding a high pressure. It seems more profitable to inquire what is the average length of a storm, how long may a violent disturbance be expected to last. This question is unfortunately complicated by the fact already alluded to, that a storm may subside for a few hours and then reappear with its original violence. If the interruption is only for an hour or two, as already explained, the depression is considered as one, but only those hours are counted in which the registered velocity exceeds fifty miles. There are only two instances in which this amount of violence has been maintained for thirty consecutive hours, viz. in February, 1868, and again last February. The total number of stormy hours in the twenty-eight years is 1732, giving an average of 5·4 hours for each storm. Our local disturbances are therefore not of long duration.

An examination of the dates when gusty weather is most prevalent, goes neither to substantiate the ancient myth of the equinoctial gales, nor to uphold the evil supremacy which has long been assigned to the winds of March. Since no attempt has been made to equalise the lengths of the months, February has been somewhat unfairly treated in the following table; but notwithstanding this handicap, it possesses the unenviable privilege of compressing within its shortened limits more tempestuous weather than any other month.

Month.	No. of storms.	No. of stormy hours.
January	47	260
February	42	281
March	47	238
April	14	63
May	7	27
June	3	19
July	7	21
August	17	65
September	16	77
October	26	180
November	44	254
December	51	247
Total	321	1732

Of course one would expect to find the greatest number of storms in the winter; but that three-fourths of the whole should be compressed within five months of the year, is a greater disproportion than was expected. The variations of the barometer during these storms, and the dependence of this variation upon the direction of the wind, are of considerable interest, but may not very well be entered upon here while treating simply of numerical statistics.

WILLIAM E. PLUMMER.

Peculiarities of Psychical Research.

MR. DIXON asks in his first letter: Could an abnormal distribution of the cards affect the result, if certain precautions were taken? In his second letter he says there was nothing in his first letter to indicate that he under-estimated the importance of "abnormal distributions." Well and good, if the S.P.R. have not under-estimated the importance of examining the actual distribution of cards cut and of cards guessed, they will have kept a record of each card cut and each card guessed, card for card. If they have not done so, then their experiment is scientifically of no value; if they have done so, then the analysis of the distributions of the cards cut and the cards guessed ought to have accompanied any publication of these experiments. It is an obvious, but by *no means sufficient*, condition for a proper experiment. If the Secretary of the S.P.R. will place in my hands the actual analyses of the cards cut and the cards guessed made by a competent mathematician, *before* the publication in their *Proceedings* of the card guesses, and proving that they did at that time fully consider the point, and take this obvious precaution against deception, my estimation of the "scientific acumen" of the S.P.R. will at any rate on this point be modified.

I, of course, do not refer to my friend Prof. Edgeworth's investigations, which do not touch the question of the distributions of cards cut and cards guessed. KARL PEARSON.

MAY I call attention to Prof. Lodge's method of "silencing" me in your issue of January 10. It bears very closely upon this question of the effect of psychical research upon the investigator's reasoning. He quotes the preface of Mr. Podmore's book to show that that gentleman is not a "bigoted upholder of the certainty of telepathy," and the casual reader would scarcely guess that, in truth, I never asserted that he was. I complained of the very air of open-mindedness in that preface to which Prof. Lodge's quotation witnesses, and showed by an instance, that in the body of the book question-begging occurred which was all the more dangerous on account of the liberal tone of the opening portion. I made no objection to the individual prosecution of psychical research—only to its public recognition before it has produced more definite results than it has done so far. So much for the "silencing." It shows either that Prof. Lodge has not read my review, or that he has misunderstood it; and in either case it enforces my contention that these investigators are over-hasty. The phrase "irresponsible detractor," points in the same direction. H. G. WELLS.

The Suspended Animation of Snakes.

IN NATURE of December 6, p. 128, Mr. G. E. Hadow asks whether the snakes feign death for protective purposes, with intent to deceive, or whether the strange action is the result of a general nervous inhibition, produced reflexly by the action of fright, which would render it more or less analogous to a fainting fit. He and others of your readers will be interested in an additional observation that, in a measure, answers his question. The snake, a "hognose," "spreading adder," or "blowing viper," *Heterodon platyrhinus*, upon which Dr. L. C. Jones based his note in NATURE, November 29, p. 107, the origin of the discussion, was presented to me about five months ago. While in my possession it has repeatedly verified Dr. Jones's statements; and, besides, it has proved that it does not depend upon the feint alone. The latter is preceded by another action that apparently has not been published hitherto. After being teased a little, the animal, vigorously bending from side to side, the tail abruptly raised and the vent slightly protruded, begins to smear itself over the back with urine and excrement, the odour of which is so excessively nauseous that observers are quickly driven back, the better satisfied if they escape without a spatter in their faces. If the teasing stops with this, the victim glides away to hide; but if still more worried, it takes up the contortions that end in the trance-like condition, lasting ten minutes to half an hour, or until the creature feels that it may safely revive. The specimen still lives, and does not discard its filthy habit on prolonged acquaintance Much handling and familiarity with annoyance make little difference in behaviour, or in disposition to take advantage of the peculiar tactics. In the inception of the habits these actions most likely were due to terror; possibly the trance was a real faint; but, however their utility may have been discovered, it is evident at the present time that confidence in them as means of securing immunity from torment induces their practice on occasions when the existence of actual fright is hardly possible. At such times it would be difficult to convince witnesses that the snake is not intelligently employing what it knows to be its best methods of protection.

Cambridge, Mass., Dec. 27, 1894. S. GARMAN.

I DO not think that Mr. Vincent can be right in supposing that the suspended animation of grass snakes has nothing to do with simulation. I have never observed it in the case of a snake when unmolested in a glass case (as he has), though I have kept hundreds so, but noticed it first when catching snakes in the New Forest. After much struggling and the usual offensive methods of defence had proved vain, one has, in several instances, suddenly hung limp and apparently lifeless on my hand. It could hardly be a faint or anything but death-feigning, for as soon as I put it on the ground, or allowed it no longer to feel my hand, it recovered at once, and was off like a shot. I took particular pains to test this, as I was much surprised at the circumstance, which I did not remember having seen mentioned in any book. In all cases it was a *dernier ressort*, the ejection of food and the effusion of smell having preceded. W. KENNEDY.

"Finger-Print" Method.

IN my letter on the subject (NATURE, December 27, 1894, p. 199), I have introduced my assertion of the old Japanese usage of the "thumb-stamps" on legal papers, with a qualifying clause—"although at present I have no record to refer to." Continuing in my search, I have come across a passage which gives confirmation to the statement. It is in the *Fûzoku Gwahô*, No. 50, p. 6, Tōkyō, February 10, 1893, where the details of the bastinado inflicted on criminals during the *ancien régime* are given, and reads as follows:—"When the criminals' guilt was ascertained, and they signed with '*thumb-stamps*' on papers in the Court, they were sent to prison with the magistrate's words, 'Sentence shall follow,' which they used to understand as the signal of the approach of the day of punishment."

December 31, 1894. KUMAGUSU MINAKATA.

A White Rainbow.

THE white rainbow is so rare as to deserve noting. One was visible at Westnewton, Aspatria, for more than half an hour on Saturday, January 5. The band was much broader than in the ordinary bow, and the arc was formed in the upper intermediate cloud drift. This drift consisted of a light pallium of irregular cirro cumulus. It is important to observe that cumulus was forming, from above, at the time; *i.e.* the cirro cumulus was melting and descending into ordinary cumulus. A patch of this cumulus formed (under observation) and crossed beneath the bow. It then became coterminous with the western section of the arc, which blended with the cloud, and was of similar tint. Hard, dry frost continued and lasted till January 13. Barometer steady at time.

SAMUEL BARBER.

Westnewton, Aspatria, January 9.

P.S.—Connote with the above the condition of the weather on the Continent; also violent thunderstorms on following day in Cornwall; also snowstorms in Cumberland and Scotland within few days.—S. B.

AMERICAN TOPOGRAPHY.[1]

WE have it on the authority of Prof. Gannett that, at the present rate of progress, the series of topographical maps of the United States, which was commenced in 1882, will require no less than fifty years for completion, and that the cost of this great undertaking will not fall far short of twenty million dollars. The map is primarily intended to meet the needs of the geologists of the Survey; but it has been thought economical to make such arrangements that the resulting map may be adequate to serve all purposes for which general topographic maps are used. Its scope is limited to the representation of the larger natural features, and the artificial features which are of general or public interest, to the exclusion of those which are purely of a private character, and therefore liable to rapid changes.

In the vast area covered by the United States, there is a great diversity both of natural and cultural features, and the extent of the survey and the scales of the maps

1 "United States Geological Survey. A Manual of Topographic Methods." By Henry Gannett, Chief Topographer. (Washington: Government Printing Office, 1893.)

are varied accordingly. The scales adopted at the commencement of the work were 1 : 62,500, 1 : 125,000, and 1 : 250,000; or very nearly 1, 2, and 4 miles to the inch respectively. With the progress of industrial development, the maps came to be in great demand in connection with all sorts of enterprises in which the nature of the ground required consideration, as in the projection of railways, water-works, drainages, and the like. Maps on a larger scale, and showing more detail, have in many instances become necessary, so that it has been determined to altogether discontinue the four miles to the inch map, but only to make new maps of the areas already represented on this scale in cases where they are specially required. It is believed that on the scales of one and two miles to the inch, it is possible to represent with faithfulness all necessary details.

The relief of the maps is represented by contours, or lines of equal elevation; in the larger scale maps the intervals range from 5 to 50 feet, and in the smaller ones from 10 to 100 feet, according to the nature of the area mapped. For the now discarded scale the intervals are from 200 to 250 feet.

The methods adopted in the preparation of these maps form the subject of the twenty-second monograph of the United States Geological Survey, which constitutes an excellent manual of topography. It is not intended as an elementary treatise on surveying, nor as a general treatise on topographic work, "although it may, to a certain extent, supply the existing need of such a work." It is primarily intended for the information of the men actually engaged upon the survey; but we believe that it will have a much larger field of usefulness.

We may look upon this manual as consisting of two essential parts: first, that dealing with the methods employed in the surveys; second, that giving a brief account of the origin of the various topographical features. The latter part we hope to refer to on another occasion, and for the present it is sufficient to say that its object is to act as a guide to correct delineation in filling in the details of the sketching.

A map, whatever its character, is defined as a sketch, corrected by locations. "The work of making locations is geometric, while that of sketching is artistic, and however numerous the locations may be, they form no part of the map itself, but serve only to correct the sketch, while the sketch supplies all the material for the map." Hence, the education of the topographer, as Prof. Gannett tells us, should consist of two parts, the mathematical and the artistic. "The first may be acquired from books, and this book knowledge must be supplemented by practice in the field. The second, if not inherited, can be acquired only by long experience in the field, and by many can be acquired only imperfectly. In fact, the sketching makes the map, and therefore, the sketching upon the Geological Survey is executed by the best topographer in the party, usually its chief, whenever practicable to do so."

In making a map, four principal operations are involved. (1) Astronomical observations for locating the map upon the earth's surface; (2) the horizontal location of points; (3) the measurement of heights; (4) the sketching of the map.

With regard to the methods now employed, "it is to be understood that they are not fixed, but are subject to change and development, and that this manual describes the stage of development reached at present." Five principal instruments have been employed in the Survey: theodolites of a powerful and compact form, for use in the primary triangulation; plane tables of the best type with telescopic alidades, for secondary triangulation and height measurements; plane tables of simple form with sighted alidades, used for traversing and minor triangulations; "odometers," for measuring distances; aneroids, for the measurement of details of heights.

All these instruments are described with sufficient fulness, while other instruments, such as transits, chains, tapes, and telemeters, which are commonly figured and described in all works on surveying, receive no special attention.

A single instrument of a very convenient form suffices for the astronomical determinations of position. This is a combined transit and zenith telescope, and consists of an ordinary transit instrument provided with a zenith micrometer eye-piece, and resting on a graduated circular base in such a way that the whole instrument can be made to revolve when using it as a zenith telescope. The telescope has an aperture of two and a half inches, and a focal length of twenty-seven inches. We are not acquainted with any other instrument so convenient for the double purpose of finding latitudes and longitudes with accuracy. Examples of the observations made with the instrument are given, and these, with the various steps in the reductions, form an admirable guide to the astronomical work.

Triangulation is employed in preference to primary traversing wherever the country presents sufficient relief for the purpose, as it is more accurate and cheaper. The initial step in this process is, of course, the measurement of a base line, and in our British survey this was accomplished by Colby's compensation bars. This method of measurement was also employed in the United States up to 1887, when it was decided to adopt a system of measurement by steel tapes. The tape in use has a length of 300 feet, and it is claimed that it is easy to obtain the required degree of accuracy in a far shorter time and at much less expense. A special apparatus for using this tape has been devised, and full instructions for its use and reduction to standard are given.

The description of the base-line measurement is naturally followed by hints as to the selection of stations and the erection of signals for triangulation. A very convenient form of observing tower, or combined instrument support and signal, for use when surrounding objects have to be overlooked, is figured and described. We learn that vernier theodolites have now been discarded in favour of others in which the circles are read by micrometer microscopes, although the circles are only 8 inches in diameter. An excellent and concise account is given of the various errors to which angular measurements are liable, and of the methods of eliminating them from the final results. Some of these errors are instrumental, others personal; and in this connection, Prof. Gannett remarks that, "after learning how to make good observations, the observer should place the utmost confidence in them, and never yield to the temptation of changing them because they disagree with some preceding observations. Such discrepancies are in general an indication of good, rather than poor, work."

In some districts it is almost impossible to carry on a triangulation, and in such cases primary traverse lines are resorted to, these simply differing from ordinary traverse lines in being more elaborately and carefully executed. These traverse lines, it may be said, consist of a series of measurements of distance and directions, and when they are intended to replace the triangulation, they are made with the steel tape, to which reference has already been made, and theodolites.

The account of the secondary triangulation is remarkable chiefly for the great prominence given to the plane table. Speaking of this, Prof. Gannett says that "much misapprehension exists, especially in this country [the United States], regarding the character and application of this instrument. This arises, apparently, from the fact that it is little known. For making a map the plane table is a universal instrument. It is applicable to all kinds of country, to all methods of work, and to all scales. For making a map it is the most simple, direct, and economic instrument; its use renders possible the making of the map directly from the country as copy, and renders unnecessary the making of elaborate notes, sketches, photographs, &c., which is not only more expensive, but produces inferior results." As the instrument is perhaps not widely known in our own country, we may say that it consists of a drawing-board mounted on a tripod in such a way that it can be levelled, turned in azimuth, and clamped in any position. At the centre of the board is pivoted the alidade, consisting of a ruler with a graduated bevelled edge, to which is attached a pair of sights for rough work, or a telescope for work of a higher class. A small graduated arc is provided in the better-class instrument for the measurement of vertical angles, but the horizontal directions are plotted directly, by means of the alidade, on a sheet of paper stretched on the board. The edge of the board is set in the same direction when the instrument is in use at different points in the area being mapped, and horizontal locations are thus readily determined by intersections.

The simple form of plane table is now exclusively used by the Survey for the ordinary traverse work. Distance measurements in this class of work are made in the usual way by counting the revolutions of a wheel, an "odometer" being used for this purpose.

The manual abounds in practical hints on the various points connected with surveying, and concludes with a brief account of the office work which is so important a supplement to work in the field.

The numerous appendices consist of tables to be used in the various computations, and are complete enough to include even a table of logarithms.

It is not too much to say that Prof. Gannett has produced a manual which will be of interest to many not actually engaged in surveying, while at the same time it forms a very valuable supplement to the ordinary works on the subject.

A. FOWLER.

LIFE AT THE ZOO.[1]

A SIGN of the increasing interest shown by the outside world in all questions concerning life, and more especially animal life, is evidenced by the far greater number of books published every year on popular natural history.

The past year witnessed the commencement of several large works, such as the "Royal Natural History," edited by Mr. Lydekker; the republication of "Jardine's Naturalists' Library," edited by Dr. R. B. Sharpe; and the "Cambridge Natural History," of which, so far, only one volume has appeared. Besides these there have been issued a number of smaller works not extending over so wide a ground.

The present volume consists of a number of short articles on various natural history topics more or less directly connected with the Zoological Society's Gardens, illustrated with reproductions of some of Gambier Bolton's successful photographs of the animals found there. A

FIG. 1.—The Tiger listening to soft music.

considerable number of these sketches have already appeared before in the pages of the *Spectator*, but many chapters have been added, and the whole forms a very agreeable *repertoire* of gossip, with, in some cases, pretensions to higher things in the shape of accounts of experiments on the æsthetics of the animal world.

One of the most interesting of the articles directly connected with the Zoo is that on "Elephant Life in England." The number of elephants now in Europe, chiefly in circuses and menageries, is considerable. Mr. Cornish gives it at about 120, of which England possesses about thirty-four. With some half-dozen exceptions, all these elephants belong to the Indian species, and are mostly imported from Burma, where they are bred in a half-wild state. The African elephant, according to our

[1] "Life at the Zoo. Notes and Traditions of the Regent's Park Gardens." By C. J. Cornish. 8vo. (London: Seeley and Co., 1895.)

author, does not appear to possess quite so even a temper or so docile a nature as the Indian species, but still, judging from what has been done with them in our Zoological Gardens, there seems to be no reason why they should not be caught and tamed exactly in the same manner as their Indian relatives. Indeed, a few years ago an officer, in the service of the German colony of East Africa, made a special tour in India for the purpose of investigating whether it would be possible to introduce the *keddah* system into East Africa. Whether this enterprise has ever come to anything, does not appear to have transpired. It is also known that the subject occupied the attention of General Gordon shortly before his death.

In an article on the Wild Cats of the Zoo, Mr. Cornish discusses the origin of our domestic tabby. Besides the European Wild Cat (*Felis catus*), which now appears to be increasing slightly in numbers in Scotland, owing, doubtless, to the increased reservation of so much of the area of that country for deer-forests, he suggests the Chaus Cat of India and Northern Africa as a possible ancestor of our domestic form. There are, however, two other species, which both seem in many respects to have greater claims. One of these is the Cat of North Africa (*Felis caffra* or *maniculata*), a species held in veneration by the ancient Egyptians, large quantities of the mummified remains of which have been imported to this country for manure. An argument in support of the opinion that this is the true ancestor of the domestic cat, is the fact that the sole of the hind-foot of this species, like that of most varieties of domestic cats, is black, and not spotted, as in the European Wild Cat (*Felis catus*).

Another possible candidate for the ancestry of the domestic cat is the Waved Cat (*Felis torquata*); this cat has been obtained in various parts of India, but is never very common. It resembles very closely the Indian domestic breed. It is, however, more than probable that whatever the origin of the domestic cat may have been, it has interbred with the wild cats of the various countries to which it has been conveyed by man. This has certainly been the case in India, where hybrids between the native domestic cats and both the Jungle Cat (*Felis chaus*) and the Leopard Cat (*Felis bengalensis*) are fairly well known.

A plea for the repeal of the absurd and oppressive Act of Parliament that prohibits the use of dogs for draught purposes, forms another short essay. This Act was based entirely on the *à priori* and ridiculous argument that dogs "were not created" for such a purpose. Mr. Cornish shows that on the Continent, where dogs are freely used in this way, no ill effects ensue to them, and that their employment is an enormous boon to the poorer classes, who are unable to afford horses.

Several chapters of this work are devoted to the inhabitants of the Reptile House at the Zoo, and among them is specially mentioned the Heloderm, the only known poisonous lizard in existence. A recent memoir by Dr. Shufeldt (*P.Z.S.* 1890, p. 148) has supplied a good deal of information respecting the anatomy of this lizard. The large poison glands are shown to lie on either side of the lower jaw, their ducts opening into the floor of the mouth, whence it is surmised that the poisonous secretion finds its way along the grooved teeth of the mandible to the inflicted wound. It is a curious fact that although some of the teeth of the upper jaw of this Lizard are also grooved, no trace of any poison gland has been found here. An interesting account of the effects of the bite of the Heloderm on the human subject has been already recorded in our pages (NATURE, vol. xxvii. p. 154), and

FIG. 2.—The Martial Hawk Eagle.

it has been shown that its effects on man, though painful at the time, are not of a serious nature.

One of the very few statements in this work with which we are unable to agree, is to be found in the account of the diving birds at the Zoo. With regard to the Penguin, Mr. Cornish says: "It cannot fly in the air; it cannot walk, but hops as if its feet were tied together; it cannot even swim." If Mr. Cornish will turn to the account of the expedition of the Dundee whalers to the Antarctic Seas, given in the *Scottish*

Geographical Magazine for February 1894, he will find the following passage: "On one occasion, in the north of the Erebus-and-Terror Gulf, we saw large schools, numbering 300 to 500, of the common black-throated Penguin, swimming together; the movements of each school being controlled by a single individual of larger size, which followed in the rear." It is quite possible that this error may have arisen through the fact that, as a rule, the Penguins, when allowed, in the Zoological Gardens, to enter their tank, dive straight off the board leading into the water, and remain under water until all the fishes in the tank are caught and devoured. After that they usually betake themselves straight back to the board. But that they can swim duck fashion, when they wish to do so, admits of no doubt whatever.

The volume is illustrated by a series of Gambier Bolton's photographs, which are certainly the best ever taken of "Life at the Zoo." Of these we are kindly permitted to reproduce two. One of them represents the Tiger "listening to soft music," and forms part of a series made to illustrate "Æsthetics at the Zoo." The other shows the Martial Hawk Eagle, a rather rare bird from South Africa. On the whole, we can cordially recommend this volume for readers of the lighter literature of Natural History, and even as containing a certain amount of novel information on "Life at the Zoo."

INTERCOLONIAL ASTRONOMY AND METEOROLOGY.

WE learn from Mr. R. L. J. Ellery, F.R.S, that a conference representing the three colonies of New South Wales, South Australia, and Victoria, met at the Observatory, Melbourne, on October 29. Mr. H. C. Russell, F.R.S., from Sydney, and Sir Charles Todd, F.R.S., from Adelaide, were present.

(1) As regards Australian standard time, it was resolved to advise the respective Governments to adopt for that purpose the time of the meridian of 150° E.

It was also agreed to advise that the changes from one hour zone to another shall take place at the eastern and western boundaries of the several colonies.

(2) A proposal, by Sir C. Todd, was adopted to the effect that the three observatories should co-operate in a special series of observations for determination of co-latitude, and for testing the applicability of the present refraction tables to astronomical work at the various observatories.

(3) It was resolved, at Mr. Russell's suggestion, to carry out systematic cloud photography at each observatory, as an aid to weather forecasting.

(4) As to agricultural forecasts, it was agreed to: "That in each of the colonies represented a forecast of the weather shall be sent to all the principal telegraph stations each day, except Sunday (Saturday's forecast being for 48 hours), and that forms to contain a week's forecasts be used, which it is proposed shall be posted on a special board at the station receiving forecasts."

(5) It was resolved that the storm signals to be used be the same as used in England.

(6) On the suggestion of Mr. Russell, it was agreed that further determination of the differences of longitude of Adelaide, Melbourne, and Sydney, should be carried out, and that periods when there was high atmospheric pressure at one place, and low at another, be selected for the operations, with the view of ascertaining if large differences of atmospheric pressure between the eastern and western stations had any influence on the longitude results. Mr. Russell undertook to draw up a programme for this undertaking.

NOTES.

ENGLISH geologists will be gratified to learn that the veteran Prof. Prestwich has been elected a Vice-President of the Geological Society of France. It is, we believe, the second time only that this honour has been conferred on one who was not a French subject. The Council of the Geological Society of London has formally offered to Prof. Prestwich its congratulations on the distinction thus received.

M. MASCART has succeeded M. Tisserand as President of the Paris Société d'Encouragement.

THE death is announced of Prof. Karl v. Haushofer, Director of the Technische Hochschule at Munich, and Professor of Mineralogy in the University of that city.

DR. G. M. DAWSON, C.M.G., F.R.S., has been appointed Director of the Geographical Survey of Canada, in succession to Dr. A. R. C. Selwyn, who has been superannuated.

WE regret to note that the Duke of Argyll was attacked by sudden indisposition while addressing a meeting at Glasgow on Tuesday evening. His condition at first gave rise to serious concern, and it was not till a late hour that he recovered sufficiently to be conveyed to the residence of Lord Kelvin, where he is staying.

AT Fishmongers' Hall, this evening, the Marquis of Lorne will present the prizes and certificates obtained by students in connection with the City and Guilds of London Institute.

THE twenty-second annual dinner of the old students of the Royal School of Mines will be held on Friday, January 25, at the Criterion Restaurant. A number of distinguished visitors are expected to be present, and arrangements have been made for a large gathering of associates and old students.

THE annual meeting of the People's Palace Chemical Society will be held on Thursday, January 24, when Dr. T. E. Thorpe, F.R.S., will give an address on "Some causes and conditions of chemical change." The chair will be taken by Prof. Tilden at 8 p.m. Tickets may be obtained by application to the hon. sec., Mr. Thomas Yetton.

THE Association for the Improvement of Geometrical Teaching will hold a general meeting at University College, Gower Street, next Saturday. In the morning, the report of the council will be read, and the new officers will be proposed for election. During the day, papers will be read on "Algebra in Schools," "The Association's Syllabus of Geometrical Conics," "The Conics of Apollonius," and "Notes on Mensuration."

THE forty-eighth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday evening, January 31, and Friday evening, February 1. The annual report of the Council will then be presented, and the election of the President, Vice-Presidents, and Members of Council will take place. The following papers will also be read and discussed, as far as time permits:—"The Determination of the Dryness of Steam," by Prof. W. Cawthorne Unwin, F.R.S.; "Comparison between Governing by Throttling and by Variable Expansion," by Captain H. Riall Sankey.

THE freshwater biological station, which was established at Plön three years ago through the efforts of Dr. Otto Zacharias, seems to be assuming quite an international character. From a recent report of the director, we learn that nine investigators worked at the station during the summer semester of last year, and of these four were German, two English, two French, and one Russian. Much interesting work seems to have been accomplished, the results of which have been partially published in the *Forschungsberichte* of the station, while others are already in the press.

THE death of Mr. Thomas Andrews at Guildford (says the *Times*) removes one of the best-known and most successful pisciculturists of the day. His breeding ponds at Haslemere, on the borders of Surrey and Hants, are perhaps, next to Sir James Maitland's famous establishment at Howietown, near Stirling, the most important trout-rearing fishery in the kingdom. The series of ponds are picturesquely situated on the side of a hill, and were all planned under the immediate direction of the late owner, who was a keen observer and an enthusiastic naturalist. They afforded every facility for studying the various points in connection with the artificial cultivation of trout. Mr. Andrews had brought this particular branch of pisciculture (to which he had devoted many years of his life) to a very high state of perfection. The distribution of ova and fish from the Surrey ponds was not merely confined to our home waters. At various times consignments were despatched to foreign countries, Ceylon, Buenos Ayres, Mauritius, and elsewhere. Mr. Andrews contributed many important papers on fishery matters to various periodicals.

THE St. Petersburg Academy of Sciences has recently made some changes in the system of publishing papers communicated to it. In September 1894, it commenced the publication of a monthly number, under the title *Bulletin de l'Académie Impériale des Sciences*, which serves as the organ of the three classes of the Academy. This *Bulletin* is intended to include the *procès-verbaux* of the meetings, annual reports on scientific researches, reports on prizes conferred by the Academy, notes on the work of the museums, &c. In addition to notices of this kind, the *Bulletin* will contain short scientific papers. The *Mémoires de l'Académie Impériale des Sciences* will form in future the second means of publication. It will be divided into two independent series, dealing respectively with the physico-mathematical section of the Academy's papers, and the historical and philological section. The publication of the *Mélanges, tirés du Bulletin*, has been discontinued.

SOME very low temperatures were reported to the Meteorological Office during the recent frost. At Braemar, in the east of Scotland, the sheltered thermometer fell below zero on four consecutive days, the lowest value being $-5°$ on the 9th instant. At Hillington, in the east of England, the lowest reading was $2°$ on the 12th instant; in other districts the minima varied considerably, being $8°$ or $9°$ in the north of Scotland and the north-east and midland parts of England. This severe weather came to a sudden termination on the night of the 12th–13th instant, owing to the approach of a serious disturbance from the Atlantic, which arrived off the Irish coast on Saturday, and remained comparatively stationary during Sunday and Monday. Strong easterly gales were experienced on the Scotch coasts, and very strong winds in other places. The gale was accompanied by a very high sea on our north and east coasts, and, as usual with easterly gales, many shipping casualties occurred.

WE learn from an article in *Das Wetter*, that a permanent meteorological station has been established by the Danish authorities at Angmagsalik, on the east coast of Greenland (Lat. 65° 37′ N., Long. 37° 16′ W.), and has been provided with self-recording instruments, in addition to the usual ones. This station will be of much importance, as it forms a link between those on the west coast of Greenland and the stations in Iceland, and as it is between Iceland and Greenland that the centre of the Icelandic barometric low-pressure lies, the varying position of which exercises a great influence on the weather conditions of Europe. It is known, from a year's observations made there by Holm ten years ago, that the climate is very rough and stormy; during the year in question the mean temperature was $5°$ below the freezing point, while the minimum reached $-13°$ of Fahrenheit's scale. Meteorologists may well be grateful to men who have undertaken to make observations in a place where, in all probability, they will be cut off from all other human intercourse for some years. Dr Nansen only succeeded, after considerable difficulty, in reaching Lat. $63\frac{1}{2}°$ along this coast in the year 1888.

IT will be news to most people that dealers in drugs, both wholesale and retail, cannot legally use kilogramme weights, or any of the metric system of weights and measures, for the export trade, though orders received from continental countries are given in that system. A short draft Bill has been prepared to amend the law, and will be presented to Parliament in the coming session. The chief clause reads as follows:—"That, on and after the passing of this Act, wherever the word 'trade' occurs in the Weights and Measures Acts of 1878 and 1889, it shall be so construed as not to prohibit or penalise the use of metric weights and measures, verified by the Board of Trade or local authorities, by export traders." The Acts of 1876 and 1889 have been disregarded by many traders, but the London County Council having recently intimated that they must enforce the law, the passing of an amendment such as that proposed has become necessary.

ON Tuesday, January 8, a paper was read before the Anthropological Institute, on the Samoyad race, by Mr. Arthur Montefiore. After dealing in some detail with the geographical distribution of the various branches of the Samoyads, Mr. Montefiore proceeded to give evidence of their affinity with the Finns, and, following Castrèn, placed them in the group which that authority called Ural-Altaic. The evidence consisted partly of physical measurements and characteristics, partly of similarity in ideas, habits and customs, and partly of identity in language. It was shown in the course of the paper that the language of the Samoyads is highly agglutinative and so inflectional as to form a link, as it were, between the Mongol and Indo-Germanic groups. After dealing with the myths and conceptions of deity held by this curious race, Mr. Montefiore read a number of notes made by Mr. Frederick G. Jackson (the leader of the Jackson-Harmsworth Polar expedition) during his sojourn among the Samoyads in the autumn and winter of 1893–94, and his subsequent journey across the Great Tundra between the Kara Sea and the Pechora River. Mr. Jackson's notes were very full, and contained some remarkable evidence of the completeness with which the Samoyad has adapted himself to the rigorous requirements of his environment. A number of Samoyad implements and other articles (including a highly curious Samoyad doll and some calculating sticks) were exhibited, together with a series of lantern slides from photographs, which are necessarily new to English students of anthropology.

DOES atmospheric dust exercise any perceptible influence on the intensity of the sun's rays transmitted through it? is the question which Prof. A. Bartoli answers in the *Nuovo Cimento*. He studied the effect of different thicknesses of air upon the sun's heat by measuring the heating power of the sun at various altitudes with the pyrheliometer, after the great eruption of Etna in July 1892. On July 25, the air was filled with an impalpable dust, which fell very gently, and gave to the sun a slight reddish tinge. There were no clouds, and there was a dead calm. By comparing the absorptive influence of the air under these conditions with that in a clear atmosphere, Prof. Bartoli found that 28 per cent. of the heat transmitted by the pure air was intercepted by the volcanic dust.

A FURTHER instalment of M. Raoul Pictet's fascinating experiments at very low temperatures is published in the *Comptes rendus*. The object of the experiments was to test the power of cotton-wool and other bad conductors to prevent the passage of low-temperature radiation. Copper cylinders were cooled down to − 170° C. and packed in layers of cotton-wool of various thicknesses. It was found that the cylinders rose to about − 80° very rapidly, and that the rate of warming was the same whether the cylinders were naked or packed in cotton-wool of 20 inches thick. The "bad conductor" behaved, in fact, like a perfect conductor transparent to heat radiation. Above − 80° the influence of the packing began to make itself felt, the rate of warming varying with the thickness of the layer.

IN the current number of *Natural Science*, Captain Marshall-Hall urges the importance of a fuller study of existing glaciers, as the necessary basis for any attempt at setting in order the chaos of opinion on the nature and causes of the Glacial period. He gives an account of the steps recently taken by the Alpine Club and the Glacier Committee of the International Geological Congress towards drawing up a systematic statement of the methods of observation, for the use of those who can visit and examine modern glaciers in any part of the world. For fuller details, readers are referred to papers in the *Alpine Journal* (February 1891, and November 1894); but a number of subjects for careful observation are suggested, and some necessary precautions mentioned. It is very satisfactory to see that the members of the Alpine Club, if primarily climbers, are able incidentally to do useful service to Geology.

IN the last number of the *Records* of the Geological Survey of India, Dr. Noetling announces an interesting discovery from Baluchistán. Certain beds which Mr. Oldham had recently described as intermediate between Cretaceous and Tertiary he finds really belong to the *Danian*—a stage to which the English chalk nowhere reaches (except perhaps in Norfolk), but which forms the summit of the Cretaceous in various parts of Europe. In examining the Echinoids from the Baluchistán beds, Dr. Noetling found a striking resemblance to those from the Danian beds in the Pyrenees. That the Pyrenean fauna should resemble that of such a distant region more than the nearer ones of Northern Europe, is another of the many pieces of evidence of ancient life-provinces—not improbably climatic in this case—that accumulate as geological research is extended into distant parts of the world. It must be coupled with the further fact that the Baluchistán Echinoids have little in common with those of the same age in Southern India.

THE last number of the *Comptes rendus* contains a paper by R. Colson, on the conditions which have to be fulfilled in order to obtain correct results when measuring liquid resistances with alternating currents and a telephone. The author, while working on the propagation of electrical waves of slow period, obtained results which have an important bearing on the above method of measuring liquid resistances. The results obtained depend on the supposition that, even in the case of alternating currents in liquid resistances, Ohm's law holds. However, this law does not hold good in the case of the propagation of waves of high potential, such as are furnished by the secondary of an induction coil in high resistances, such as threads saturated with a solution of calcium chloride, or capillary tubes filled with water. The author gives a series of tests which, when applied to any given arrangement of conductors, will show whether the above effects will have any influence on the result.

MR. B. D. PEIRCE has contributed a paper to the *American Journal of Science* (vol. xlviii. p. 312), on the thermo-electric properties of platinoid and manganine. Since platinoid and manganine wires are often used in potentiometers and slide wire Wheatstone's Bridges, the thermo-electric properties of these metals are of considerable interest. The author has primarily determined the electromotive force of the above metals and copper, since this is the most interesting case from a practical point of view. He finds that after the manganine wire has been well annealed, the thermo-electric phenomena are quite regular. While the mean electromotive force for a difference of 10° C. between the hot and cold junctions of a copper manganine couple is about 5·5 microvolts, in the case of a platinoid-copper couple the mean electromotive force for the same difference of temperature is about 170 microvolts. Incidentally the author has examined the thermo-electromotive force of couples consisting of different samples of commercial copper, and he finds that two specimens of annealed copper wire bought from different makers hardly ever yielded more than one or two C.G.S. units of E.M.F. per degree Centigrade.

A FEW particulars concerning the earthquake felt in Nicaragua and Honduras in November 1894, have been sent to us from Managua, by Mr. J. Crawford. Shocks were felt for twenty seconds at Managua, about 9h. 36m., and another series, lasting about thirty seconds, at eleven in the evening of November 19. Mr. Crawford has been able to trace the course of the waves for about 120 miles to the north-west and south-east of the city. At Managua, the undulations were short and rapid; they were stronger at the city of Masaya—twelve miles to the eastward, and still stronger at Granada—about twenty-four miles eastward. In the latter place, houses were thrown down by the vibrations, and also at Chinandega—seventy miles west of Managua, though at Managua itself the shocks were not of sufficient severity to fracture any of the walls of the houses, or displace any of the tiles on the roofs. It is remarkable that the earthquakes were most violent both east and west of the city.

MR. J. C. SHENSTONE has taken a census of remarkable oak trees in Essex, and he gives, in the *Essex Naturalist*, descriptions and illustrations of noteworthy specimens found by him; together with notes on a few oak trees outside the county The five trees with the largest trunks in Great Britain, stated in "Loudon's Arboretum," are: Cowthorpe Oak, Yorkshire, 78 feet; Merton Oak, Norfolk, 63 feet; Hempstead Oak, Essex, 53 feet; Grimstone Oak, Surrey, 48 feet; Salsey Oak, Northampton, 46 feet. Among trees having the widest stretch of boughs, are the Worksop Oak, 180 feet; and the Oakley Oak, 110 feet. All these trees are not, however, standing at the present time. The Hempstead Oak fell about twenty-five years ago, and a mutilated and decayed trunk is all that remains of this forest giant. A fine tree, thirty-one feet in circumference, exists in the park at Danbury Palace. The inside of the bole was completely burnt out more than sixty years ago, but the tree has continued to grow, and will probably yet survive many years. Several of the trees mentioned are said to be from five hundred to a thousand years old, but there is not sufficient evidence to decide the point at all accurately.

BRITISH ornithologists have long been aware that the increase and extension of range of the starling presents some interesting and phenomenal facts. They will therefore follow with interest a paper on the increase and distribution of the bird in Scotland, contributed by Mr. J. A. Harvie-Brown to the *Annals of Scottish Natural History* for January. By looking up old records dating back to the end of last century, and by collecting information through a special circular, it has been possible to give a consecutive statement of the steps of advance. The account, and the map accompanying it, show that, as regards Scotland, the starling is almost omnipresent. The statistics indicate "that two great centres of habitation have influenced the dispersal of the species: an earlier one in the Shetlands, Orkney, and the Outer Hebrides, and north coasts and north-east of Caithness, from north-east towards south-west; and a later one, entering Scotland in the south and passing north through the south and central districts of Scotland. Moray appears to have drawn its supplies from the northward, in comparatively recent years; but the districts to the south of the Grampians mostly, if not entirely, from the southwards. . . . It might not, perhaps, be too rash to predict that the day may yet arrive when the starling having increased still more prodigiously, and every crevice and cranny having become populated by these cosmopolites, a great struggle for existence even amongst themselves may become necessary to preserve the balance of nature. Before this can take place, however, the probability exists that some other weaker species may have to go to the wall. Indeed, there are already indications of such a fact in at least one instance and locality—by sheer force of numbers."

IN the eighth annual report of the Liverpool Marine Biology Committee, edited by Prof. Herdman, there is an interesting account of the work carried on during the past year at the biological station at Port Erin. A number of important additions to the fauna already known in that neighbourhood are recorded, including *Dicoryne conferta*, *Crisia ramosa*, *Amphicodon fritillaria*, and some forms of Copepoda new to science, *Pseudocyclopia stephoides* and several species of the genus *Ectinosoma*. Many observations have been made on the submarine deposits of the Irish Sea, and a preliminary general account is given in the report. The bottom, down to the depth of 10 fathoms, is chiefly covered with sand; at a greater depth, between 10 and 20 fathoms, there is a large admixture of mud; from 20 to 50 fathoms the bottom deposits are greatly varied, and here the richest fauna occurs; below 50 fathoms is found a bluish-grey tenacious mud, with a peculiar and characteristic fauna. Prof. Herdman very justly points out the importance of a thorough investigation of the submarine deposits round our coast; he regards the nature of these deposits as probably the most important of the various factors that determine the distribution of animals over the sea-bottom within one zoological area. "In practically the same water, identical in temperature, salinity, and transparency, at the same depth, with, so far as one can see, all the other surrounding conditions the same, the fauna varies from place to place with changes in the bottom—mud, sand, nullipores, and shell-beds, all have their characteristic assemblages of animals." The concluding part of the report contains a short account of a plan for the distribution of drift-bottles in order to obtain information with regard to the currents in the Irish Sea which affect small floating bodies; this experiment is based upon the plan employed by Prince Monaco in the Atlantic a few years ago. The observations made on the course of the drift-bottles up to the present have already been described in our columns by Prof. Herdman (NATURE, December 13, 1894, p. 151).

WITH the January *Journal* of the Chemical Society, we have received a supplementary number, containing title-pages, contents, and indexes of volumes lxv. and lxvi. (parts i. and ii.) of the *Journal*.

THE resolutions accepted by the International Congress of Hygiene and Demography, held at Budapest in September last, have just been published in the form of a pamphlet. They are printed in four languages—Hungarian, French, German, and English.

THE first part of a new catalogue of entomological works offered for sale, has been issued by R. Friedländer and Son, Berlin. The list, which is No. 416 of the "Bücher-Verzeichniss," published by Friedländer, is devoted to fossil insects, Coleoptera, and miscellaneous works on entomology.

WE regret to announce that, owing to the death of Father Denza, the publication of the *Bollettino Mensuale*, issued by the Moncalieri Observatory for the last fourteen years, is to be discontinued, and that the Italian Meteorological Society is to be dissolved. We hope that some other pioneer of Italian meteorology will take up the good work left by Father Denza.

THE publication of "The Cambridge Natural History" will shortly be commenced by Messrs. Macmillan and Co. The first volume to appear will contain "Molluscs," by the Rev. A. H. Cooke; "Brachiopods (Recent)," by Mr. A. E. Shipley; and "Brachiopods (Fossil)," by Mr. F. R. C. Reed. This will be followed, in the course of a few months, by two other volumes in the same series, on "Insects," by Dr. David Sharp, F.R.S. The whole series, which will include ten volumes, fully illustrated, is intended, in the first instance, for those who have not had any special scientific training; but an attempt will be made to combine popular treatment and popular language with the most modern results of scientific research.

REFERRING to the announcement that ten Surinam water-toads had been received at the Zoological Society's Gardens (NATURE, p. 85), Dr. C. Kerbert, the Director of the Koninklijk Zoölogisch Genootschap, informs us that a number of these interesting animals were received at Amsterdam in October 1893, and are still living.

THE Royal Horticultural Society has been established for ninety years, and there are at present three thousand Fellows on its roll. It works in various ways "for the improvement of horticulture in all its branches, ornamental as well as useful." The *Journal* of the Society usually contains much valuable information on scientific and practical gardening. In the January issue, there are a number of papers read at a conference on trees, and others read at a conference on British-grown fruit. Perhaps the most important section of the Society is the strong Scientific Committee appointed to examine and report upon instances, submitted by the Fellows, of diseases and injuries of plants, caused by insects or otherwise. The Committee give their advice on all matters connected with the prevention or cure of disease, and are glad to receive specimens of malformation, or other subjects of horticultural or botanical interest.

A DEFINITE and trustworthy answer appears at last to be given to the long-standing question of the closely approximating atomic weights of nickel and cobalt, in a communication from Prof. Winkler to the *Zeitschrift für Anorganische Chemie*. The great difficulty in deciding the actual values of the atomic constants of these interesting metals has been largely owing to the fact that the methods of analysis hitherto adopted have not been free from all source of error. During the last few years, however, very considerable progress has been made in the chemistry of the compounds of nickel and cobalt, and the sources of error are now more correctly appreciated, and consequently more amenable to elimination. It is inconceivable, in the light of the ample verification which the periodic generalisation associated with the names of Newlands and Mendeléeff has received since its inception, by the subsequent accumulation of experimental facts, that the atoms of nickel and cobalt can both be endowed with the same relative weight, as was for so long supposed to be the case. A short time ago, Prof. Winkler carried out a series of analyses of the chlorides, prepared from the electrolytically deposited metals, and obtained the numbers $Ni = 58·90$ and $Co = 59·67$. Having, however, still some doubts as to the absolute accuracy of the decimal places, owing to the possibility of a minute source of error in the preparation of pure neutral chlorides, another series of experiments have been carried out by a method which Prof. Winkler states is in his opinion (and there can be none higher as regards work with the two metals in question) quite unimpeachable. The older methods based upon the electrolytic determination of the metals were found to lead to an error in the case of cobalt, owing to the fact that a small quantity of the hydrated oxide $Co_2O_3 \cdot 2H_2O$ is contained in the deposit upon the platinum electrode, while in the case of nickel no oxidation whatever occurs. This discovery was consequent upon another, namely, that a solution of iodine in potassium iodide of decinormal strength is capable of instantly dissolving the deposited metal from the platinum terminal without in the slightest attacking the latter, producing a solution of the iodide of the metal. In the case of nickel the platinum is left perfectly clean, while deposited cobalt invariably leaves a stain due to about half per cent. of oxide. The electrolytically-deposited cobalt employed by Prof. Winkler was therefore all reduced in pure hydrogen before use; when subsequently dissolved in the iodine solution, no trace of oxide was ever left. The method of analysis consisted in determining, by titration with pure sodium thiosulphate, the excess of iodine left after solution of the pure metals to form the iodides. Two complete series of analyses, each consisting of a considerable number of individual determinations, were carried out with an interval of some months, in order to employ metals from totally independent mineral deposits. The results are most concordant, and lead to the final numbers, Ni = 58·72 and Co = 59·37, when H = 1 and I = 126·53. The atomic weight of cobalt must therefore be accepted as at least half a unit higher than that of nickel, a result likewise in accordance with the work of Prof. Winkler published a short time ago.

OUR ASTRONOMICAL COLUMN.

δ CEPHEI.—Further particulars of Dr. Belopolsky's spectroscopic study of this variable star (NATURE, November 1, 1894, p. 21) are given in the *Bulletin* of the St. Petersburg Academy of Sciences, November, 1894, and some of his numerical results are slightly changed. He has shown for the first time that orbital movement is as closely associated with this class of short-period variable stars as with those of the Algol class, in which the minima are produced by eclipses. Although the spectrum of δ Cephei is described as of Vogel's Class II.*a*, it is pointed out that it differs from that of the sun in many respects, some of the lines which are narrow and feeble in the sun being strong in the star, and *vice versa*. There does not appear to be any change in the character of the spectrum, other than a variation of intensity, as the light of the star changes.

The displacements of the lines with respect to the comparison spectra of iron and hydrogen indicate that the star has an orbital movement in a period corresponding to that of the light changes (5d. 9h.), and that the eccentricity of the orbit is 0·514. The form of the curve of velocities which is given, indicates that the major axis of the orbit must be very nearly directed towards the earth, and the system is approaching the earth with a velocity of about 12 English miles per second. With reference to the centre of movement, the maximum velocities of approach and recession are about 13 English miles per second; the star is receding for about a day after minimum, approaching for nearly 3 days more, and receding until minimum again. Periastron is the point of the orbit farthest removed from us, and is passed about a day after minimum.

The apparent semi axis major is about 818,800 English miles, so that the whole orbit is less than twice the sun's diameter. We are not aware that any attempt has been made to determine the parallax of the star, but unless it has a very high emissive power, its small size would indicate that it must be relatively near to us.

Notwithstanding that the time of a possible eclipse is a day after the minimum of the light curve, Dr. Belopolsky seems to be of opinion that the variation may be due to eclipsing. He appears to believe that further work may show that there is some systematic error, and that periastron may really coincide with the minimum. As the light changes appear to be going on continuously throughout the period, it is clear that the eclipsing cannot be of the simple kind with which we are familiar in the case of Algol.

THE VATICAN OBSERVATORY.—A day after the death of Father Denza, we received the fourth volume of the "Pubblicazioni della Specola Vaticana." Every annual report of the work done at the Vatican Observatory is more voluminous than the one preceding it. The volume which came to us last month, and to which Father Denza's death gives a melancholy interest, runs into more than six hundred pages, and is illustrated by forty-two plates. Among these are illustrations of the Dumb-bell nebula in Vulpecula, the Pleiades nebula, the Orion nebula after exposures of thirty minutes and of nine hours, two photographs of the eclipse of April 1893, a photograph of the sun with the big spot of August 1893, together with two enlarged pictures of the spot, and a map showing the direction of motion of the meteors observed on August 10–11 of the same year. Papers on the subjects of the illustrations make up the greater part of the astronomical section of the report. Altogether, ten photographs were taken at Rome during the eclipse of April 1893, and contact observations were also made. The numerous meteor observations made under Father Denza's direction in Italy, in August and November 1893, are catalogued and commented upon. It is to be hoped that the system of meteor-observation which the late Director instituted will not be allowed to lapse. In celestial photography we note that in addition to the work for the photographic chart and catalogue, 248 photographs were taken of various celestial objects, 150 being photographs of the sun. But the astronomical results by no means represent the total work carried out at the observatory. Meteorology and terrestrial physics come in for a large share of attention. To us it seems that the only thing wanting to make the Vatican Observatory a true astro-physical observatory is a section for spectroscopic investigations.

Many astronomers will be interested to know that a full and appreciative notice of Father Denza's life and work has been written by P. Armani, of the Collegio dei S.S. Biagio e Carlo. Another full notice appeared in *Cosmos* of December 22.

AN INDISPENSABLE ANNUAIRE.—This year is the centenary of the creation of the Bureau des Longitudes, the invaluable *Annuaire* of which has been received for 1895. It is quite unnecessary to remind astronomers of the merits of this veritable *vade mecum*, for they know its usefulness better perhaps than workers in the other branches of physical science to which it appeals. A few changes have been made since the previous issue. M. Berthelot has completely revised and corrected the tables relating to thermo-chemistry. M. Moureaux has inserted in the tables of the magnetic elements in France, the values determined directly by him in 1894, at nearly six hundred places. Prof. Glasenapp has added five new stars to his table of the elements of the orbits of double stars. The list of comets has been brought up to the end of 1893, and that of minor planets up to November 1894. There are five articles in the volume, the subjects and authors being: Lunar atmospheric waves, by M. Bouquet de la Grye; the Geodetic Congress at Innsbruck, by M. Tisserand; the Observatory on Mont Blanc, by M. Janssen; photographic photometry, by the same author; and a report on the proposition to unify the astronomical and civil days, by M. Poincaré (see the next note).

THE UNIFICATION OF CIVIL AND ASTRONOMICAL DAYS.—It will be remembered that in 1893, the Astronomical and Physical Society of Toronto invited replies from astronomers to the question: "Is it desirable, all interests considered, that on and after the first day of January 1901, the Astronomical Day should everywhere begin at mean midnight?" The result of the voting was noted in NATURE, April 5, 1894, p. 542; and to this may now be added the following resolution adopted by the Bureau des Longitudes upon the question (*Annuaire* for 1895):—"The Bureau des Longitudes is favourable, in principle, to the reform proposed by the Canadian Institute to change the time from which to reckon the astronomical day. The Bureau thinks that this reform, as has been observed by the Lords of the Admiralty, will be of little avail unless an understanding is come to between the Governments publishing the principal ephemerides. Finally, considering that the unification will not really be complete until the civil hour is reckoned from 0 to 24 hours, as is the case in Italy, the Bureau is of the opinion that this reform ought to be realised as soon as possible."

THE FOUNDATIONS OF DYNAMICS.[1]

THIS posthumous volume of Hertz's works, edited by Prof. Lenard, with a preface by von Helmholtz, has a doubly melancholy interest. It is the last work of Hertz upon which he was engaged until a few days before his death, and it contains a preface which is almost the last work of von Helmholtz. The pupil died shortly before his master, and by the departure of such a pupil and of such a master, science, and with science mankind, have lost many prospects of advances in the near future.

In his preface, von Helmholtz pays a touching tribute to the genius of his favourite pupil, from whom he hoped most, and who had drunk most deeply of his master's thoughts. In 1878 their intimacy began. At that time difficulties connected with various electrical theories of action at a distance were occupying his thoughts, and he offered a prize for the best essay on induction in non-inductively wound coils. Weber's theory would have involved an inertia of the electric current distinct from the magnetic inertia. The question is still interesting in connection with discharges between two charged conductors, one of which completely surrounds the other when a dielectric between them is suddenly made conductive. There is then no magnetic force. Is there *no* inertia? Can a medium become *suddenly* conducting? Is a conducting medium homogeneous? Is there inertia of ionic charges which represent the non-homogeneity of the medium? These questions still require answering; but in the seventies, in Germany, Maxwell's idea of magnetic force accompanying displacement currents was not generally received, and Helmholtz's question as to the induction in non-inductively wound coils really had reference to these displacements. Hertz won the prize by showing that at most only 1/20th or 1/30th of the extra-current could be due to electric inertia. By subsequent experiments on the possible effect of centrifugal force on the current in rapidly rotating plates, he reduced this estimate to a very much smaller value. Mr. Larmor has suggested that any centrifugal force may be balanced by a tension in the length of the current, much in the same way that the tension of a running rope will balance centrifugal force in the curves round which it may be running. In every way the subject deserves further investigation, for it is intimately connected with the most fundamental questions as to the nature of electricity and its connection with matter.

The next thing to which Hertz devoted himself was a prize problem proposed, at von Helmholtz's suggestion, by the Berlin Academy. The problem was to investigate Maxwell's postulate that changing electric displacement was an electric current. This was the bud from which Hertz's great work sprang. Of it von Helmholtz says: "It is a pity we do not possess more such histories of the inner psychological development of knowledge. Its author deserves our sincerest thanks for letting us see so deeply into the inmost working of his thoughts, and for recording even his temporary mistakes. By this work Hertz has settled for ever the question as to electromagnetic actions being propagated by a medium, and the only outstanding question of the kind is as to gravitation, which we do not yet know how to logically explain as other than a pure action at a distance." It thus appears that von Helmholtz to the last was unconvinced as to the probability of any hypothesis like Le Sage's or Osborne Reynolds's. He seems, on the other hand, to have been satisfied with the possibility of chemical actions being explained either by electromagnetic actions or by actions not at a distance. This latter term, of course, requires explanation as to what "at a distance" means. Any actions other than those of absolutely rigid bodies, such, for instance, as the fairly well-established forces of attraction of gaseous molecules for one another, and some of which can hardly be explained either by electricity, magnetism, or gravitation, seem to be actions at a distance that require explanation just as much as gravitation.

Following this short history of the work of his pupil which, coming from such a master, must have a permanent interest to all, von Helmholtz gives a *résumé* of the last work of Hertz. In it there is attempted a continuously elaborated presentation of a complete and self-dependent system of mechanics, in which each particular application of this science is deduced from a single fundamental law which can of course be itself only assumed as a plausible hypothesis. In order to explain how this is required, von Helmholtz gives a short history of the development of the science of mechanics. The first developments arose from the study of the equilibrium and motion of solid bodies in direct contacts with one another, such as the simple machines, the lever, the inclined plane, the pulley. The law of virtual velocities gives the most fundamental general solution of all such problems. Galileo subsequently developed the knowledge of inertia and of moving force as an accelerating agent. It was, however, conceived by him as a succession of blows. Newton was the first who arrived at the notion of force acting at a distance, and its more accurate determination by the principle of action and reaction. It is well known how strenuously he and his contemporaries resisted this idea of pure action at a distance. From this men developed the methods of treating all problems of conservation forces with constant connections whose most general solution is given by D'Alembert's principle. All the general principles of dynamics have been developed from Newton's hypothesis of permanent forces between material points and permanent connections between them. It was subsequently found that these laws held even when these foundations could not be proved, and it was thence deduced that all the laws of nature agreed with certain general characteristics of Newton's conservative forces of attraction, although it was not found possible to deduce all these generalisations from one common fundamental principle. Hertz has devoted himself to discovering such a fundamental principle for mechanics, from which all the laws of mechanics hitherto known as universally valid can be deduced; and he has carried out this with great acuteness, and by means of a very remarkable presentation of a peculiarly general kinematic conception. In working it out, he returns to the oldest mechanical theories, and supposes all actions to be by means of rigid connections. Of course he has to assume that there are innumerable imperceptible masses and invisible motions of these, in order to explain the apparent actions upon one another of bodies that are not in immediate contact. Though he has not given examples of how this may be the case, he evidently builds his expectation of being thus able to explain natural actions upon the existence of cyclical systems, rollers, &c., with invisible motions. The justification of such an assumption can only be obtained by its success. Von Helmholtz concludes this interesting preface by remarking how English physicists have so often based their work on dynamical and geometrical suppositions, as for example Lord Kelvin and his vortex atoms, Maxwell and his cells with rotating contents. These physicists, he says, "have clearly been more satisfactorily helped by such illustrations, than by the mere most general representations of the facts and their laws as given by the system of physical differential equations. I must confess that I have restricted myself to this latter method of investigation, and have felt most confidence therein; and indeed I might not have arrived at any important results by the methods which eminent physicists such as the three mentioned have employed."

Although so far it seems as if there were very little to choose between the old methods of supposing that natural actions can be explained by conservative forces between molecules and by systems of rigid connections, Hertz in his introduction shows that he is dissatisfied with the hypotheses, of these forces as entities, while von Helmholtz, by his silence, seems to hold the view that the old method was good enough for him. Hertz's method has, however, the advantage of turning our attention to something definite to be investigated and invented, namely, the structure of these rigid connections. It is apparently very closely related to Osborne Reynolds's and "Waterdale's," suggestions as to the structure of the ether, namely, that it consists of perfectly rigid particles in almost complete juxtaposition which, whether by their smoothness or by their rolling upon one another, waste no energy in internal heat motions.

In his own preface, Hertz says that he has culled many things from many minds, nothing particular in his work is new; what he presents as new is the arrangement and collocation of the whole, and the logical, or rather philosophical, aspect thereby attained.

To these prefaces there follows a long introduction, in which Hertz reviews and criticises the present foundations of dynamics. The great road by which this domain is now entered is one that was laid by Archimedes, Galileo, Newton, and Lagrange. It is founded on our notions of space, time, force, and mass. Force is introduced prior to motion, as the independent cause thereof. Galileo's notion of inertia only involved space, time, and mass.

[1] "The Principles of Dynamics developed on new lines:—Hertz's Collected Works," vol. iii. Pp. 310. (Leipzig: Barth, 1894.)

Newton first introduced all four notions. To this D'Alembert's principle gave the analytical method of treating generally connected systems. Beyond it all is deduction. Here Hertz introduces a discussion as to the so-called forces of inertia. From his discussing the case of a solid subject to centripetal acceleration by means of a string, the question is much more intricate than if he had taken the case of a body falling freely under gravity, where the force is applied directly by the earth to each point of the body, and not, as in the case of the string, distributed to each part by stresses in the solid. Hertz seems to consider that there is some outstanding confusion in applying the principle of equality of action and reaction, and appears to hold that by this principle the action on the body requires some reaction *in the body* whose acceleration is the effect of the force. He does not seem fully to appreciate that action and reaction are always on *different* bodies. From his consideration of this, and from a general review of our conception of force, he concludes that there is something mysterious about it, that its nature is a problem in physics, like the nature of electricity. We have a quite distinct conception of velocity: why not of force? He concludes that the mystery is not due to our not having enough ideas to associate with the word, but to our trying to put too much into it. These mysteries, however, do not invalidate in any way the deductions that have been made; they only require us to seek out a new foundation for our dynamics. He goes on to criticise this method of filling nature with forces of which, being ultimately between molecules, we can have no direct experience. A piece of iron on a table is acted on by gravitation, cohesion, repulsion, magnetic, electromagnetic, electric, and chemical forces. Some of these would drag it to pieces if unbalanced to a nicety by others. Is this a sound view of nature? Can we not get some more attractive one?

A second view may be elaborated by making our fundamental quantities, space, time, mass, and energy. There is no book in which this view of nature is fully and consistently worked out, at least none that Hertz was acquainted with. He sketches how it might proceed. Besides the postulate of the conservation of energy we require some definition of potential energy and experimental relations connecting it with space, and in addition we have a choice of relations with kinetic energy, of which Hertz suggests the choice of the integral form of Hamilton's principle known as that of least action. This is, no doubt, a recondite idea to use as a fundamental postulate, but it only implicitly involves the idea of force, which then comes in merely as a definition. To this method, which certainly has several great advantages, Hertz makes a number of objections. In the first place he objects that it requires the equations of connection to be integral equations, and we know such actions as pure rolling of one hard body on another cannot be so expressed. We must, in order to specify the subsequent motion, know the rate of rotation round the normal axis through the point of contact, and this cannot be specified except in terms of differentials. To such motions we cannot apply the proposed principle of least action, and yet we can hardly dispute that such rolling is possible in nature. If we treat it as the limits of frictional sliding, we introduce the whole of the difficulties of force, or of the irregular heat actions which have not yet been fully made amenable to accurate dynamical treatment. Again, difficulties arise as to the foundation of this method. There is great difficulty in specifying energy itself. How can it be satisfactorily measured without returning to the first method, and introducing the idea of force? Some have conceived of energy as a sort of substance; but when we try to form concrete conceptions of what is occurring, we get involved in perplexities. The very existence of two forms of energy is a very serious difficulty. Again, it is doubtful whether it can be sound to consider the integral of least action as a *fundamental* principle. It makes the present depend on the future. It sets the problem to nature to make a certain integral the minimum.

A good many of these objections could be got over by making all energy kinetic, which is what Hertz himself practically assumes in his own method.

This third method begins by assuming only three fundamental quantities, time, space, and mass, and puts aside as non-fundamental, force and energy. In order to explain how nature works, we already do postulate invisible underlying structures in nature. We postulate these in the atoms and molecules of matter. Hertz sees in all actions the working of an underlying structure whose masses and motions are producing the effects on matter that we perceive, and what we call force and energy are due to the actions of these invisible structures, which he implicitly identifies with the ether.

We must, however, assume certain connections between the three quantities, time, space, and mass. Between time and mass there is no direct connection. Space and mass, Hertz considers, are connected by the existence of a given mass at each point of space. He cannot mean here to assume a complete plenum, which would make serious difficulties in the way of the working of what he subsequently assumes to be a structure of rigid bodies; he must include a vanishingly small density at some points, though perhaps he may have had in view the filling of the interstices between his rigid bodies with a fluid. Any way, he goes on to say that some connection is required between all three quantities, and for this purpose he postulates his great fundamental single law of motion, which he considers is an extension to systems of Newton's first law of motion for a single body; it is that a system, which is unconnected with any others, moves with constant swiftness along one of its straightest paths. "Systema omne liberum perseverare in statu quo quiescendi vel movendi uniformiter in directissimam." In order to understand what Hertz here means by the path of a system, and by its being straight or curved, requires further explanation; but from this principle, which is capable of analytical representation, and from the assumption that the connections of a system are all rigid, he deduces all the fundamental principles, conservation of areas, momentum, energy, least action, &c. In considering the motion of any part of a system, we find that we may conveniently introduce certain actions of the other parts of the system upon it which are measured by forces, which thus come in as mere definitions. He does not seem to investigate anywhere the question as to the danger of his rigid connections becoming tangled. Analytically a postulate that the points of two different bodies that act on one another are in contact is easily expressed, but it does not follow that when we come to invent actual rigid connections to produce the observed effects, they will do so for any length of time without jamming. It is a seductive theory that gravitation or electrical actions may be due to vortex filaments ending on atoms; but the tangling of the filaments is a very serious difficulty that has not been satisfactorily got over. Hertz does not seem to feel this as a serious difficulty, but he does notice an obvious objection that is sure to be raised, namely, that rigidity in itself postulates forces. To this he replies that rigidity in itself is merely a matter of definition and of fact. How is our view of the fact that two points are at a constant distance apart, improved by saying that there is a force between them? As, however, real bodies are only imperfectly rigid, Hertz concedes that it may be that when we learn more about these invisible connections, they may turn out not to be absolutely rigid. It is a matter for further investigation. This very same view might have been urged, and has been urged already with reference to actions like gravity. The *law* of gravity can be perfectly well described without any reference to the notion of force. We may say, every element of matter moves towards every other element in the universe with an acceleration inversely proportional to the square of their distances apart. We can describe the law kinetically, just as Hertz proposes to describe the law of motion of parts of a rigid body. There is no *necessity*, however convenient it may be, to introduce the notion of force; the other bodies in the universe are a sufficient cause for motion of each, without postulating an entity, force. The principal reason for introducing this notion was to account for a body acting where it was not; force was invented to get over this; the body produced force, and this force existed where the body did not, and there acted on other bodies. This whole difficulty seems, however, to be partly due to want of distinct ideas connected with the question of where a body *is*. We are so accustomed to consider a body as having a definite boundary, that we think there is a definite boundary in reality. All we know of the atoms and molecules, however, would lead us to conclude that round the centre of each there is a very complexly structured region which may or may not change abruptly in structure, but which often extends to considerable distances from the atom, so that it is practically impossible to state absolutely where the atom ends and where the empty space begins. With this view of matter there is no serious reason why we may not rightly consider each atom as existing everywhere that it acts, that is, throughout the whole of space, for its action in causing gravitional accelerations exists, so far as we know, throughout space. A view of this kind entirely gets over any difficulty of a body acting where it is not; for all bodies *are* everywhere, and if we consider matter to be the cause of motion of other matter, there seems no very imperious necessity for imagining another cause which we call force.

There are two assumptions that Hertz makes which he considers can only be proved by their success. One is that all the connections in nature can be represented by linear differential equations. There are plenty of cases imaginable in which this would not be true, as, for example, connections depending on the curvature of the path. The other assumption is that forces can be represented by force functions. This, again, may not be a complete representation of nature.

Following this introduction comes the book itself, which is divided into two parts. The first part is purely kinematical, the second deals with the deductions from Hertz's fundamental postulate of motion in the straightest possible path.

The first part begins by explaining what is meant by the path of a system of points. To get at this we calculate the mean square of the displacements of a system of points when they are displaced: the square root of this, Hertz calls the displacement of the system of points. If there is a mass at each point, then the displacement of the system is the square root of the mean squares of the displacements of the points, each multiplied by the mass at it. Thus, if s be the displacement of the system, and $s_1 s_2$, &c., the displacements of each point of masses $m_1 m_2$, &c. Then

$$(m_1 + m_2 + \ldots .)s^2 = m_1 s_1^2 + m_2 s_2^2 + \ldots$$

By taking s_1, s_2, &c., as the displacements in the element of time, we evidently get a similar expression for the velocity of the system, and for its acceleration. The mean square of the velocity of the parts of a system is well known in connection with the principle of least action. Further than this, however, Hertz defines the angle between two displacements. This is defined by the equation

$$(m_1 + m_2 + \ldots .)ss' \cos \epsilon = (m_1 s_1 s_1' \cos \alpha_1 + m_2 s_2 s_2' \cos \alpha_2 + \ldots .)$$

s and s' being the two displacements of the system as calculated above, and $s_1 s_1'$, &c., the two displacements of each point and α_1, α_2, the angles between these latter, then ϵ is the angle between s and s'. Hertz remarks that these can all be very interestingly expressed in terms of space of multiple dimensions, in which analytical diagrams are supposed to be drawn. This, however, represents the real by the unattainable. There follow, then, several chapters expressing these displacements in terms of various systems of coordinates, and discussions as to the conditions that the connections of a system should fulfil in order that they may be represented by equations not involving differentials. The curvature of the path is here studied. It is defined as $c = \frac{d\epsilon}{ds}$, and from this it follows that, representing $\frac{d^2x}{ds^2}$ by x'', &c.

$$(m_1 + m_2 + \ldots .)c^2 = \Sigma_1{}^1(m_1 x_1''^2 + y_1''^2 + z_1''^2).$$

The problem then of making the path of the system straightest, is to make c a minimum consistently with the connections of the system. Now, in accordance with his assumption that the connections of the system are linear differential equations of the form

$$\Sigma_1{}^1 P_1 x'_1 = 0,$$

whose differentiation gives

$$\Sigma_1{}^1 P_1 x'_1{}' + \Sigma^1 \Sigma_1{}^1 . \frac{dP_1}{dx_2} x'_1 x'_2 = 0,$$

we are to determine the minimum value of

$$c^2 = \Sigma_1{}^1 \frac{m_1}{m} . x''_1{}^2,$$

when

$$m = m_1 + m_2 + \ldots .$$

In determining the variations of these, we must recollect that the positions and direction of displacement, *i.e.* the first differentials of the system, are supposed given, and that it is *only* the second differentials that can be varied in order to make c a minimum. Calling, then, a system of indeterminate coefficient λ, μ, &c., corresponding to the equations of condition, we evidently get a system of equations of the form

$$\frac{m_1}{m} x''_1 + \Sigma_1{}^1 P_1 \lambda = 0,$$

which are sufficient to determine the second differentials required.

From this form of result one can see how the ordinary equations of motion are derivable from the conception of the straightest path, and how, when dealing with part of a system, these indeterminate coefficients introduce what are equivalent to forces. This method of deducing the equations of motion lends itself particularly well to the deduction of the principles of least action, and the other general methods in dynamics. So far, he deals with free systems subject only to internal constraints. It is where he investigates how to deal with parts of systems that he requires to consider the nature of the constraints joining one part to another. For this purpose he defines two systems as coupled when coordinates can be so chosen that one or more of them are the same for both systems. Force is then defined as the action one system has on another. Now, when a coordinate is the same for two systems, one of the equations of condition is $p = p'$, p and p' being coordinates of the coupled systems, and for this equation the coefficient P becomes the same in the two systems, being unity for each, so that the equations of motion involve the indeterminate coefficient λ corresponding to this equation equally with reference to each system. It is thus that the equality of action and reaction appears, being thus bound up with the constant equality of the common coordinate. This seems to be where the assumption that the connections are rigid is introduced. When rigid bodies act upon one another by non-slipping contact, certainly the coordinates of the point of contact are common to the two systems. It is also quite evident that if we assume rigid bodies acting upon one another by contact only, we can have no potential energy, and all *necessity* for talking about the forces disappears. In Hertz's system there are no forces like Newton's acting between bodies which have no common coordinate, like the earth and the sun. We would have to invent connections to explain the motion before we could be certain that action and reaction are equal in this case.

The proof of the principle of virtual velocities by substituting for the forces between parts of a system a number of pulleys which produce the same effects, is quite analogous to Hertz's supposition that the actual connections are by rigid bodies. It is not, however, liable to the objection that the connections may become tangled, for it is only applied to the case of infinitesimal virtual displacements, while Hertz postulates the possibility of his connections existing as the real ones for all time, and throughout all finite displacements of the system.

The work considers many other matters, and shows how all the general methods in dynamics are deducible from his fundamental postulate of the straightest path. It includes discussions on how best to deal with systems whose connections do not involve differentials, how to treat cyclical coordinates, and many other matters. It is most philosophical and condensed, and gives one of the most—if not the most—philosophical presentations of dynamics that has been published. It is worthy of its author: what more can be said?

G. F. FITZGERALD.

PSEUDO-SATELLITES OF JUPITER IN THE SEVENTEENTH CENTURY.

IN the New York *Nation* for January 11, 1894, Dr. D. C. Gilman, President of the Johns Hopkins University, called attention to an interesting letter from John Winthrop, jun., to Sir Robert Moray, concerning the satellites of Jupiter. In this letter, which was written from Hartford, Connecticut, on January 27, 166⅘, Winthrop described an observation of Jupiter which he had made on the night of the previous 6th of August, when he had very distinctly seen five satellites about that planet. He was naturally "not with out some consideration whether that fifth might not be some fixt star with which Jupiter might at that tyme be in neare conjunction," and expressed the wish that more frequent observations might be made upon that planet with a view to ascertaining whether it is not impossible to discern a fixed star, when it is so near to the planet as to appear "within the periphery of that single *intuitus* by a tube which taketh in the body of Jupiter," and if so, whether his star is not a new satellite. He further proceeds:—"I am bold the rather to mention this as an inquiry whether any such number of Satellites or moons hath beene seene by your honor or Mr. Rooke [? Hooke] or any mathematitians or other gentlemen that have good tubes and often have the curiosity to view the planet, for possibly it may be new to me which hath beene more usually knowne by others, though the notion of such a thinge is not new to my selfe, for I remember I mett with the like narration many years since in a little booke intituled Philosophia Naturalis per Joh. Phociliden, though then I thought that was but a mistake of some fixed starrs." Now that Prof. Barnard has discovered a genuine fifth satellite of Jupiter after the lapse of nearly three centuries since Galileo's telescope detected the Medicean stars, a greater interest is given to those imaginary satellites which from time to time have claimed a place in the solar system. In a paper in the Johns Hopkins University *Circular* for last May (referred to in NATURE, vol. l. No. 1283, p. 113), Mr. Frank H. Clutz has given a probable identification of Winthrop's supposed satellite with B.A.C. 6448. In a postscript to his article it is pointed out that the work to which Winthrop refers is presumably the "Philosophia Naturalis, seu Physica Vetus-Nova" of Johann Fokkens (born at Holwarden in Friesland in 1618; died at Franeker, February 19, 1651). At President Gilman's suggestion, I have followed up this clue, and have succeeded in examining the work in question. I find that it contains more than a casual reference to additional satellites of Jupiter; in fact, a whole chapter is devoted to a discussion of certain observations, which seems to be sufficiently interesting to deserve a brief notice.

Phocylides' treatise was published at Franeker in 1651, shortly after the death of the author. On the title-page it is described as written "Ab Eximio Viro/JOH. PHOCYLIDE HOLWARDA/ L.A.M. Med. Doct. & Philosophiæ,/dum viveret, Profess. Ordinario." There is also a portrait. The third of the three parts into which the book is divided is entitled "Physica Cœlestis," and it is doubtless this portion of the Philosophia which Winthrop had in mind when writing his letter to Sir Robert Moray. It is not Phocylides himself who claims to have discovered new moons of Jupiter; nor does he believe in their existence. On the contrary, he treats the supposed observations with the severest disapprobation; and while in general he is enthusiastic over the revelations which the telescope had already made and was daily making, this particular discovery, which he regards as spurious, rouses him to the most strenuous exertions in order to effect its refutation. The discoverer, by name Antonius Maria Schyrleus de Rheita, stated that he had seen, not one, but five new satellites of Jupiter, all farther removed from the planet than the four Galilean moons, and revolving in a contrary direction. Phocylides' account of the matter may be briefly summarised as follows: First he refers to five stars near a certain known star of the fourth magnitude. They are "intra duodecimum & decimum quintum Piscium gradum" (p. 205) "versus sinistram secundum Signorum successionem." (p. 205 and p. 284.)

These stars, he continues, have given rise to no little controversy among learned and experienced astronomers (astrophilos) whether they are fixed stars or erratic, and, more particularly, whether they are companions (commilitones) of Jupiter. The discussion of this point is then postponed. Chapter xvi. treats of the number of the planets and their division in kind. Both satellites and planets proper are included in the term "planetæ." After rejecting the sun from his ancient place among the seven planets, and adding to their number the two *laterones* about Saturn and the four about Jupiter, discovered in the "current century by the aid of new and admirable instruments, such as Tubi Optici, specula, and others of the same sort," he adds the Earth to the twelve hitherto obtained. It is thus certain, he says, that there are at least thirteen planets, since it is not yet known whether Mars, Venus, and Mercury have any *laterones* revolving about them. "But as to the five other circumjoviales which P. Anton. Mar. de Rheitâ boasts that he has observed and called Urban Octavianæ, learned men are justly in doubt whether they should be referred to the fixed or to the wandering stars." Nothing further is added at this place, but he proceeds to divide the thirteen planets into primary and secondary. The former (six in number) are named in order from Mercury to Saturn, with the Earth in the Sun's former place. The latter are Saturn's two, and Jupiter's four satellites. The seventh is the Moon. Anyone who doubts this, he adds, will be convinced by what is soon to be said, as well as by the considerations contained in the chapter "De Lunæ corpore & motu." After several interesting chapters, including one on Saturn and his laterones, and another on Jupiter and the Medicean stars, we come to chapter xxi., entitled "De Pseudo-Jovialibus: & priorum consectaria." Phocylides' attitude is at once disclosed. He refers to the pseudo-discoverer as "quidam Antonius Maria de Rheita, (homo in Papatu arrogans, invidus, judicio præceps & fatuus, aliorum gloriam pro propriâ injuste & cum pudoris oblivione captans & vindicans)."

The discovery was alleged to have been made at Cologne near the end of the year 1642, and was announced in a letter to Puteanus. But it so happened that Hevelius had noticed and described these very stars several months previously. His observations were made in August 1642 and the succeeding months, and Phocylides regards the claims of Rheita to have been the first to discover them as "fœtidum mendacium," as, by his own admission, he did not observe them before the end of the year. The identity of Rheita's pseudo joviales with these fixed stars is shown by a comparison of the actual positions of Jupiter on the dates when the observations were made. Our author points out that on December 29, the day of Rheita's discovery, Jupiter occupied the same position as on the previous 4th of September, when Hevelius observed him and noticed the five stars. This position is defined by Phocylides in the words, "decimo tertio gradu Piscium, cum quindecem minutis." Again, Hevelius saw Jupiter on August 28 "in decimo quarto gradu Piscium, cum quindecem minutis," where Rheita observed him on January 4 of the following year (1643). The new circum-Joviales are then, says Phocylides, none other than the stars recently discovered in Aquarius. They do not move round Jupiter, as they would do if they were secondary planets, and are therefore fixed stars, "manebuntque [fixæ] usque ad sæculi consummationem." They were observed by Hevelius to remain stationary, while Jupiter left them behind him. Phocylides challenges any doubter to look for these stars himself, when all five will be found in their original position and at the same distances one from another. He concludes with the words (p. 289): "Hallucinatur itaque crassè & inani nimis glorio â Rheita se ipsum titillat."

The following will throw further light on Rheita's tendency to form hasty conclusions. According to Phocylides he declared that he had observed three of the Medicean stars to be in a straight line, but the fourth "tantam obtinuisse latitudinem, quæ, si ex centro Jovis Eccentricum ad illum excurrentem metiamur, ad quindecem gradus excrescat." This, says our author, is contradictory to the observations of all "mathematicians," who unanimously agree that the Medicean stars never recede from Jupiter in latitude more than three minutes, whether to the north or to the south. He then states that this fourth star, so far from being a true Galilean satellite, was not even a pseudo-jovial, but another fixed star. Gassendi observed the satellites on the same night, and saw all four in a straight line, the second slightly to the north; but they were differently disposed with regard to Jupiter. In all probability, Phocylides concludes, the first (innermost) satellite was obscured by the planet, if, as is likely, the observations were not made at precisely the same hour; for Jovial Mercury (as he is called) runs his course round Jupiter in a few hours more than a whole day. Phocylides supposes that, as Rheita was well aware that there were four satellites, he mistook this fixed star for the fourth satellite.

The existence of Rheita's new satellites was opposed by Gassendi in his "Judicium de novem stellis circa Jovem visis" (see Delambre, "Hist. de l'Ast. Mod.," ii. p. 351), and by Hevelius in his "Selenographia," *ib.* p. 437). Lobkowitzius defended Rheita against Gassendi ("Phocylides," p. 295). Delambre devotes several pages to Rheita, or Schyrle, and gives some account of his work "Oculus Enoch et Eliæ, sive Radius sidereomysticus," Antwerp, 1645. In this book, according to Delambre, he admits that he has not succeeded in observing his satellites again, but asserts anew that he noticed changes in their mutual distances. He explains their disappearance by connecting them with variable stars, which he supposes to have large orbits and long revolutions. He believed, moreover, that the spots of the suns were planets, and that the sun itself rotated but once in a year.

Phocylides also mentions circum-martiales and circum-saturnales, which, no less than the circum-joviales, must be held to be fixed stars.

It will thus be seen that Winthrop was right in remembering

Phocylides' treatise as a work in which the discovery of new satellites was mentioned. But apparently he did not remember very accurately the position assumed by the author, for the book contains but little to encourage an observer in the belief that he has discovered new satellites.

It is perhaps worth noticing that the Philosophia was published less than fourteen years before the date of Winthrop's letter; it must therefore have been quite a new book when Winthrop perused it, as he says, "many years since."

CHARLES W. L. JOHNSON.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Prof. J. Burdon Sanderson has been appointed Regius Professor of Medicine in succession to Sir Henry W. Acland, whose resignation was announced last Term. In accepting the Regius Professorship, Prof. Sanderson vacates the Waynflete Chair of Physiology, which is more valuable in a pecuniary sense. It is naturally a matter of regret that he should formally sever his connection with the school of Physiology which he may be said to have created in Oxford, but it is recognised that no better appointment could have been made to the headship of the Medical School which he has done so much to encourage, and whose interests he will have further opportunities of promoting in his new position.

At a meeting of the Royal Statistical Society held on Tuesday, a paper was read, by Mr. L. L. Price, on "The Colleges of Oxford and Agricultural Depression." The accounts of the Oxford and Cambridge Colleges have been published year by year for some time past, and in Mr. Price's paper the accounts of the Oxford Colleges for the years 1883–93 were brought under review. The gross external receipts of the Colleges were in 1893 some £11,000 less than in 1883, and the net external receipts some £13,000. Though the external receipts are not entirely derived from agricultural estates, it seems within the facts to regard agricultural depression as responsible for a loss of upwards of £60,000 of income in 1893. Turning to the effects of the depression upon the emoluments of the Heads, Fellows, Scholars, and Exhibitioners, to which the College revenues are mainly devoted, it appears that these effects have been mitigated by the circumstances that the external receipts are not exclusively agricultural, and that the emoluments are also partly derived from internal receipts and from Trusts. Still the emoluments of the Heads have fallen from £22,811 to £20,905, and of the Fellows from £83,820 to £74,749. The emoluments of the Scholars and Exhibitioners have however increased from £44,776 to £48,378, and their number has grown by upwards of ninety; and if the increased contributions made by the Colleges to the University are taken into consideration, the fall in the total payments is only about 5 per cent. But there are Colleges, where diminutious have occurred of more than 25 per cent. in the emoluments of the Fellows, and the figures generally are altered considerably for the worse by eliminating a few prosperous Colleges.

CAMBRIDGE.—Mr. A. Hutchinson, Fellow of Pembroke College, has been appointed Demonstrator of Mineralogy and Assistant-Curator of the Museum, in place of Mr. Solly, who has retired.

The Downing Professor of Medicine (Dr. Bradbury) announces that the newly-organised Museum of Materia Medica and the Pharmacological Laboratory are now open daily to students of medicine, and that demonstrations will be given therein by Mr. Marshall, the assistant to the Professor.

Dr. Gaskell, F.R.S., has been appointed an additional member of the Board for Biology and Geology.

THE Royal Agricultural Society has now issued its revised regulations and syllabus for the society's senior examinations, framed in accordance with the important modifications recently decided upon. The council have resolved to place annually at the disposal of their education committee five life memberships of the society, to be awarded to the five candidates who stand highest on the list of winners of first-class certificates, and who obtain not less than two-thirds of the maximum number of marks. The gold medal of the society will be bestowed upon the candidate who stands highest on the list of winners of life memberships, provided that he has obtained not less than three-fourths of the maximum number of marks, and silver medals upon the other winners of life memberships, including the candidate at the head of the list, if he does not reach the standard required for a gold medal.

THE Association of Head Masters held its first meeting as an incorporated body on Thursday last. One of the items of the agenda was a paper in which Mr Stuart described the usual practice of teaching science, and said most of them were satisfied that such a system had no educational value at all. All experiments must be capable of being performed and the observations made by the students. The experiments must be chiefly quantitative and especially at first. Books and lecture demonstrations must be avoided. He thought that a good grounding in science might be given by doing practical work in an ordinary class-room, upon common tables, with home-made apparatus. The following resolutions were afterwards passed by the meeting:—

(*a*) "That the association is of opinion that examining bodies should encourage a more rational method of teaching science, by framing the syllabuses in such a manner that the practical work required may be strictly illustrative of the theoretical instruction given."

(*b*) "That it be referred to the general committee to appoint a small sub-committee, so that a report may be presented to the next summer general meeting containing detailed suggestions which it is proposed to make to examining bodies concerning examinations in science."

THE Research Scholarship given by her Majesty's Commissioners for the Exhibition of 1851, to Mr. Edward Taylor Jones, of the University College of North Wales, in 1892, has been renewed for a third year. Such renewal is only made in cases of exceptional merit, where valuable scientific results are likely to be obtained by a continuance of the scholar's research work. Mr. Jones has just completed, at the University of Berlin, an experimental investigation solving an important problem in magnetism. An account of the research has been communicated to her Majesty's Commissioners, and will shortly be published.

THE Professorship of Mathematics in the Government Training College, Ireland, vacant by the retirement of Principal Corbett, has been filled by the appointment of Mr. Dilworth, of Trinity College, Dublin.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, January 7.—M. Marey in the chair.—A list of the present members, foreign associates, and correspondents of the Academy is given.—M. A. Cornu was elected Vice-President for 1895.—Preparation, in the electric furnace, of *graphites foisonnants*, by M. Henri Moissan. For all varieties of graphite prepared by intense heat, the temperature of intumescence after soaking in nitric acid is about 165°–175° C. They resemble natural graphites in this and other respects, hence the probability that the latter have been produced at a very high temperature under moderate pressures in masses of iron which have since disappeared.—The vasomotor nerves of the veins, by M. L. Ranvier. From the results of experiments quoted, the author concludes that veins as well as arteries are supplied with vasomotor nerves.—On the first scientific voyages of the *Princess Alice*, by Prince Albert I. of Monaco.—An addition to Le Verrier's theory of the movement of Saturn and rectification of the Tables, by M. A. Gaillot.—On the approximate development of the perturbation function, by M. N. Coculesco.—On roots common to several equations, by M. Walther Dyck.—On the theory of a system of differential equations, by M. A. J. Stodolkievitz.—On the theory of exchangeable substitutions, by M. Demeczky.—On the absolute value of the magnetic elements on January 1, 1895, by M. Th. Moureaux. The values are given for (A) Parc Saint-Maur, Long. 0° 9′ 23″ E. and Lat. 48° 48′ 34″ N.; (B) Perpignan, Long. 0° 32′ 45″ E. and Lat. 42° 42′ 8″ N.

Elements.	Abs. values Jan. 1, 1895.		Secular variation in 1894.	
Declination	(A) 15° 12′·7	(B) 4° 3′·4	(A) −5′·3	(B) −5′·0
Inclination	65 4·9	60 9·9	−1·2	−0·8
Horizontal component	0·19641	0·22345	+0·00017	+0·00025
Vertical component	0·42277	0·38961	−0·00003	+0·00021
Total force	0·46617	0·44914	+0·00005	+0·00031

Use of the critical temperature of liquids for the recognition of their purity, by M. Raoul Pictet. A convenient method of determining the critical points of liquids is described. Any impurity causes a difference in the critical temperatures in the same sense as the difference produced in the boiling points, but in the former case the difference is of far greater magnitude than in the latter.—On the qualitative separation of nickel and cobalt, by M. A. Villiers. The author avails himself of the property of sodium tartrate in preventing the precipitation of nickel sulphide while allowing the complete precipitation of cobalt sulphide. Tartaric acid is added to the clear solution of the two metals, then soda (not potash) in large excess and hydrogen sulphide is passed. The nickel passes into the filtrate as a nearly black solution, mere traces give a brown tinge. The method is not quantitative.—Some points in the spermatogenesis of the Selacians, by M. Armand Sabatier.—On the genesis of intestinal epithelium, by M. Étienne de Rouville. Observations confirm the author's views that: (1) The conjunctive tissue continues more or less, during life, to be the matrix giving rise to the elements of other tissues; it is a post-embryonic blastoderm. (2) Epithelial tissues are only, in most cases, the forms limiting the free surfaces of conjunctive tissue.—Physiological researches on the Lamellibranchs (*Tapes decussata*, &c.), by M. Piéri.—On some lakes in the Alps and Pyrenees, by M. A. Delebecque. The depths and altitudes of most of the important mountain lakes are given.

BERLIN.

Physical Society, November 30, 1894.—Prof. von Bezold, President, in the chair.—Dr. Aschkinass described his experiments on the influence of electric waves on the galvanic resistance of metallic conductors. Gratings made of tinfoil when placed near a Hertz exciter showed a diminished resistance which was quite independent of the action of light due to the primary sparks, and was persistent after the cessation of the electric oscillations, but could then be restored to its original value by mere mechanical percussion. A series of experiments proved that it is really the electric waves which altered the resistance of the grating, and the results were extended to other metallic conductors. The speaker drew attention to analogous observations made by English and Swiss physicists who had found that filings of iron and other metals enclosed in glass tubes had their resistances altered by electric sparks discharged in their neighbourhood. In their case, also, the original resistance was restored by mechanical vibration.—Dr. Gross spoke on the electrolysis of a solution of mixed nitrate and sulphate of silver to which a little nitric acid had been added. Silver was deposited on the kathode, and a black substance on the anode; the latter he had not as yet obtained free from silver, but it did not contain any sulphur, although 60 per cent. of sulphuric acid had disappeared from the solution.

Physiological Society, December 7.—Prof. du Bois Reymond, President, in the chair.—Prof. L. Lewin gave an account of some experiments made with an alkaloid obtained from a North Mexican cactus called "Peyotl." It is well known that this plant has an intoxicating action, and in larger doses produces sleep and a state of nervous excitation accompanied by a so-called "power of prophesying," similarly attributed to the sulphurous exhalations of the temple at Delphi. Small doses of the alkaloid when given to frogs produced tetanic cramps and a greatly increased reflex irritability, analogous to strychnine; but with this difference, that by carefully apportioning the dose the effects were permanent for several days. Similar results were obtained with rabbits, and Prof. Lewin regarded the new alkaloid as specially adapted to further the study of the nature of tetanus. In rabbits it was noticed that during each paroxysm of cramps, the blood-vessels of the ears were widely distended. The speaker had also found alkaloids with powerful actions in many species of Cactus hitherto regarded as harmless by botanists, notably one closely resembling curare.—Dr. G. Joachim had investigated sphygmographically the effect of suspension by the head on the circulation, and in the case of a number of invalids, of whom some were suffering from heart-disease, had observed only a slightly increased frequency of pulse, which is probably merely attributable to psychic excitation.—Prof. Gad communicated the results of an investigation, made by a new method by Mr. Seeler, of Cleveland, on the terminations of motor nerves in muscles, which had shown that in addition to the motor fibre a non-medullated fibre leaves the sheath of Henle, and is distributed to the capillaries of the muscle-fibre, whereas the medullated motor-fibres spread out o the muscle itself.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Logic: Dr. C. Sigwart, translated by H. Dendy, 2 Vols., 2nd edition (Sonnenschein).—Les Abîmes: E. A. Martel (Paris, Delagrave).—Essays on Rural Hygiene: Dr. G. V. Poore, 2nd edition (Longmans).—Annuaire de l'Académie Royale des Sciences, &c., de Belgique, 1895 (Bruxelles).—Astronomische Chronologie: Dr. W. F. Wislicenus (Leipzig, Teubner).—Handbuch der Theorie der Linearen Differentialgleichungen: Dr. L. Schlesinger, Erster Band (Leipzig, Teubner).—Laboratory Exercises in Botany: Prof. E. S. Bastin (Philadelphia, Saunders).—Smithsonian Report, 1893 (Washington).—Geological Survey, Alabama, Report on the Geology of the Coastal Plain of Alabama (Montgomery, Alabama).—Tables and Directions for the Qualitative Chemical Analysis of Moderately Complex Mixtures of Salts: M. M. P. Muir (Longmans).

PAMPHLETS.—Eighth Annual Report of the Liverpool Marine Biology Committee and their Biological Station at Port Erin: Prof. Herdman (Liverpool, Dobb).—On the Search for Coal in the South-East of England: W. J. Harrison (Birmingham).—Eine Discussion der Kräft der Chemischen Dynamik: Dr. L. Stettinheimer (Frankfurt a.M., Bechhold).—The Varieties of the Human Species: Prof. G. Sergi (Washington).—Royal Horticultural Society Report for 1894–5 (Victoria Street).—Ditto, Arrangements for 1895 (Victoria Street).

SERIALS.—Geographical Journal, January (Stanford).—American Journal of Science, January (New Haven).—Gazetta Chimica Italiana, Anno xxiv. 1894. Fasc. vi. (Roma).—Proceedings of the Physical Society of London, January (Taylor).—Journal of the Chemical Society, January (Gurney).—Ditto, Supplementary Number (Gurney).—Record of Technical and Secondary Education, January (Macmillan).—Beiträge zur Petrographie der Östlichen Centralalpen speciell des Gross-Venedigerstockes: Dr. E. Weinschenk, I. and II. (München).—Morphologisches Jahrbuch, 22 Band, 2 Heft (Leipzig, Engelmann).—Journal of the Franklin Institute, January (Pilhadelphia).—Journal of the Royal Horticultural Society, January (Victoria Street).—Engineering Magazine, January (Tucker).

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