THURSDAY, APRIL 7, 1898.

## A MALPIGHI BICENTENARY VOLUME.

*Marcello Malpighi e l'opera sua. Scritti varii.* Pp. 338. (Milan: Vallardi, 1897.)

THE great Malpighi—Marcello Malpighi—to give him his full name, anatomist, physiologist, botanist, pathologist, biologist, and above all natural philosopher, striking and powerful man of science in the latter half of the seventeenth century, was born on March 10, 1628, in the house of his father, a farmer in easy circumstances in the outskirts of the town of Crevalore, which lies in the neighbourhood of Bologna.

Last year the town of Crevalore, with the help of others, erected in its market-place, opposite the town hall, a bronze statue of their great townsman as a tangible token of how much they felt his worth. Dr. Pizzoli, the Secretary of the Committee for the erection of the monument, conceived the happy idea of combining with the memorial of bronze one of another kind—one which should not be stationary at Crevalore, but wander far and wide—a printed book in which several men of science of different lands and pursuing different paths of inquiry might state what they knew and thought of their great common master of old times. Circumstances prevented the two memorials being completed in 1894, which would have been the bicentenary of Malpighi's death, this taking place on November 29, 1694; but the statue was unveiled last November, and the memorial volume is now before the world.

It would be out of place in a notice such as this to dwell at length on Malpighi's place in the history of biological science, or to attempt to discuss the value of his many and varied labours. I must content myself with giving a brief account of the contents of this memorial volume.

The several contributions are very varied, both in length and character; and as one reads them in succession, a great deal of repetition is met with; but this is unavoidable in a work written in the way in which this is written; and it may at least be said that all the contributions will reward perusal.

G. Atti (of Bologna) gives a biographical sketch, the shortness of which is, I cannot help thinking, much to be regretted; and though Prof. Atti has written at length elsewhere, I feel sure that a fuller relation of Malpighi's life, some genial narration of his personal story, free from any critical account of his scientific labours, would have been a very acceptable addition to the volume.

Virchow contributes an éloge, Haeckel an appreciative estimate of Malpighi as a philosophic naturalist, De Michelis (of Ravenna) an essay on Malpighi's place in the History of Thought, Todaro (of Rome) a sympathetic view of him as a pioneer in biological studies and as an advocate of experimental medicine being considered as an integral part of the study of living things, and De Giovanni (of Padua) an exposition of his place in the development of pathological science. All these are short, while the contribution of Weiss (of Messina), entitled a general introduction, dealing as it does with the several aspects of Malpighi's scientific activity, is necessarily longer.

Kölliker supplies a very brief but pregnant and admirable statement of the many notable discoveries in general anatomy which we owe to Malpighi, Romiti (of Pisa) an estimate, also short, of Malpighi's place in the history of topographical human anatomy, while Eternod (of Geneva) dwells more in detail on his worth as being one of the earliest to grasp the value of that research into minute structure, whether of plants or animals, which we now call Histology, and indeed as being one of the founders of a branch of biological science which has, especially in these latter days, gathered in so many and such important truths. Cattaneo (of Genoa) expounds at length and in detail the great man's many and varied contributions to comparative anatomy; and Perroncito (of Turin) adds a detailed account, which by reason of its very detail is most interesting, of Malpighi's famous work on the silk worm, "De Bombyce." It will be remembered that Malpighi was led to undertake this investigation in consequence of a letter which the Royal Society of London addressed to him, through the hand of its Secretary Oldenburgh, and that the volume containing the account of the investigation was published by and on the financial responsibility of the Royal Society, being the first of a series of works by Malpighi thus published. Indeed after this onward nearly all Malpighi's inquiries were published by the Royal Society.

We learn from Dr. Pizzoli's sympathetic preface that it had been intended to include a contribution on Malpighi as an embryologist, one of Malpighi's works being "De formatione pulli in ovo." Through misadventure this intention failed; but the value of Malpighi's work in this direction is touched upon by more than one of the contributors just mentioned.

Two contributions deal with Malpighi's botanical researches. At its meeting of December 7, 1671, there was read before the Royal Society a preliminary sketch by Malpighi of his botanical investigations under the title of "Anatomes Plantarum Idea"; and at the same meeting our countryman Nehemiah Grew laid before the Society a copy of his work entitled "The Anatomy of Plants begun," which the Society in the previous spring had ordered to be printed. Much controversy has arisen in respect to the relative merits of Malpighi and Grew as the founders of the anatomy of plants. One of the above two contributions is a short essay by Strasburger in which, while giving Grew all his due as an original inquirer, he claims for Malpighi a higher place as being a mind of wider grasp, as being one who in investigating plants was seeking a clue to the secrets not of plants only but of all living things. The other contribution, by Morini, is much longer and deals in detail with all Malpighi's botanical studies, incidentally touching also on the controversy about Grew, and giving a brief sketch of the condition of botany before Malpighi began his work.

I have myself contributed a condensed account of Malpighi's relations with the Royal Society, explaining in a simple manner how the correspondence between the one and the other began, how the Society undertook in succession the publication of Malpighi's most important works, and how cordial and close was the intercourse between the great Italian inquirer and the learned

English body. Some of the letters which passed between Malpighi and the Royal Society appear in the "Opera Omnia." But many others are preserved in the archives of the Society, and I thought that it would be well if all these saw the light. I accordingly have added these letters—some from Malpighi to the Society or to one or other of the Secretaries, others from the latter to Malpighi, in all forty-two in number—as an Appendix to what I have written. In doing this I received most valuable assistance from Mr. Herbert Rix, the late Assistant Secretary to the Society. Probably some printer's and other verbal errors have escaped the notice of both of us.

Lastly the volume contains an account, by L. Frati, of the various medals issued in honour of Malpighi, and a bibliography, by C. Frati, both of Malpighi's own writings and of various writings about him.

Dr. Pizzoli may certainly be congratulated on having produced an interesting and useful volume, the reading of which cannot but do good. To stand back from the present rush of inquiry and controversy, to look across two centuries at a great man, struggling with the beginnings of problems which have since come down to us, some in part solved, but others with their solutions put still further off by the very increase of knowledge, is a useful lesson to every one of us. In any case the great men who in the past opened up for us paths of inquiry—and among these Malpighi takes a foremost place—ought not to remain mere names, known to us chiefly through being attached to some structure or to some piece of apparatus. We ought all of us to be able to form some idea of what they were and what they thought. The present volume will be a great help to any one, who can read Italian, towards such an end in respect to Marcello Malpighi.

M. Foster.

## THE ARYO-SEMITIC SCHOOL OF MYTHOLOGY.

*Semitic Influence in Hellenic Mythology, with special reference to the recent mythological works of the Right Hon. Prof. F. Max Müller and Mr. Andrew Lang.* By R. Brown, junior. Pp. xvi + 288. (London: Williams and Norgate, 1898.)

It has been a well-known fact for many years past that the breach between the linguistic and anthropological schools of mythology was growing steadily, and it was evident that a serious rupture must eventually occur. It was felt that the venerable linguistic method was being slowly but surely undermined by many workers, and that the anthropologists were consolidating their position in a remarkable manner. The rupture, however, might have been delayed, and the two schools might have made concessions mutually in the interests of the peace and progress of the science, the advancement of which each party professed to have at heart, had they been allowed to do so. But it was not to be, and the immediate cause of battle between the rival schools was the publication of Prof. Max Müller's "Contributions to the Science of Mythology," wherein the great writer discussed with his characteristic learning the subjects on which he is the first authority at present. This work was violently attacked by Mr. Andrew Lang, who, it cannot be denied, impressed many by his skill in word trickery and brilliant phrases, and the unwary reader may quite well be forgiven if he was led astray by a flood of journalistic eloquence. Those, however, who had any knowledge of the subject saw at once that Mr. Lang did not represent the anthropological school, and that he had no right to pretend to do so; for as is well known he has shown no evidence that he possesses any special knowledge of any one of the subjects which go to form that complex whole called mythology. Prof. Max Müller may have made mistakes, but he knows his languages; Mr. Lang has a competent knowledge of no Oriental language, and can never now acquire even a working hold upon the dialects of the East, wherein Prof. Max Müller was an authority thirty years ago. To us it seems doubtful if Mr. Lang has sufficient knowledge of Eastern linguistics to understand all the points of Prof. Max Müller's position. In any case Mr. Lang's attack upon the Oxford Professor was futile, and all it served to do was to show that Mr. Lang had mistaken his own powers, and that he had without any proper authority assumed to himself the right to act as spokesman for the anthropological school of mythology. Now, it seems, another combatant has joined in the fray in the person of Mr. Robert Brown, junior, who, though wishing to support Prof. Max Müller against Mr. Lang, has a few objections to urge against the venerable scholar, and an axe of his own to grind. Mr. Brown, like Mr. Lang, makes himself the spokesman of a "School," which, he says, "for present purposes, I may style the Aryo-Semitic," and though he recognises "the vast results that have sprung from the scientific application of Aryan linguistics," he is "in entire sympathy with the researches of anthropology in general, and of folk-lore in particular." The cynical outsider will have some difficulty in understanding the position of such a Mr. Facing-both-ways. As far as we can see, Mr. Brown has printed his book to prove that Hellenic mythology owes a pretty big debt to Semitic peoples; but then, no one, so far as we know, ever doubted this obvious fact. Mr. Brown has also taken a great dislike to Mr. Lang, the evidence of which forces itself upon the reader in several places. Mr. Brown's dislike is so strong that in order to relieve his feelings, he is obliged to write a number of childish things, which any friend of his would have excised from his manuscript before it was printed. Mr. Brown also falls foul of Mr. Frazer, the author of the "Golden Bough," and when, like Mr. Silas Wegg, Mr. Brown is obliged to "drop into poetry," and to print in a book intended to be serious the silly lines (p. 14),

> O Mr. Frazer, Mr. Frazer, what a man you are!
> I never thought when you set out that you would "go so far,"

we can only regret that Prof. Max Müller has been "taken up" by Mr. Brown. Moreover, to talk of a "Covent-garden-market theory of mythology" (p. 15) is hardly the language which we should expect from one who calls himself a supporter, and, in some respects, a disciple of Prof. Max Müller.

It is time to ask now what Mr. Brown's qualifications are for his self-assumed *rôle* of defender of Prof. Max Müller. In reading over his pages we see that a great many languages are quoted, and that a vigorous attempt has been made by Mr. Brown to mark the quantities of the vowels which occur in the extracts; the pages look not only learned but terrible. But it is one thing to be able to find words in a dictionary, and another to know the language to which the dictionary is the key. Mr. Brown has written many papers on astronomical matters, and we are willing to assume, for the sake of argument, that they may be of value; but from the manner in which he writes the words of one of the languages which he quotes, that is to say Hebrew, we are convinced that his knowledge of it is of an elementary character. An example or two will show what we mean. On p. 115 he speaks of Sanchouniathan, meaning Sanchon-yathan (we leave out the vowel quantities because they are not necessary); this spelling shows that Mr. Brown took the name from a non-English book, and did not know that Sanchôn was the form of the god's name. The spelling Aschthârth (pp. 115 and 182) is another example of the same thing. On p. 116 (*bis*) he prints Qarnaîm for *Qarnayim*, which shows that he does not know how to transcribe the dual ending in Hebrew; the *a* cannot be long here unless it carries the accent. On p. 133 he gives *dayon* as the Hebrew for the word "judge"; as a matter of fact it is *dayyân;* on p. 149 he writes Ai lênu for *î lânu;* on p. 181, Qastu for *Qashtu;* on p. 182, Dagîm for *Dâgîm;* on p. 142, Kîyûn for *Kîyyûn;* on p. 133, anoshîm for *ănâshîm;* and so on in many places. These are not mere misprints, and they show the want of knowledge of elementary principles of Hebrew grammar. He often vocalises Phœnician words in defiance of all the laws which governed the Masoretes in their deliberations, and yet when he has good authority for adding the lengths of the vowels he fails to do so; see on p. 182, where he writes Kimah for *Kîmâh.* We cannot attempt to follow Mr. Brown in his Accadian, and "Hittite," and other little-known dialects, but the general impression which we gather from his book is that he is little more of a genuine expert in linguistic mythology than is Mr. Lang; and Mr. Lang is a brilliant, amusing writer, whilst Mr. Brown is not. The silly remarks on p. 85 are in very bad taste. The scholars of Oxford, Cambridge and London are only too glad to help on learning in any shape or form, and no honest worker is pushed aside at any of these places because he does not live there, or is not a graduate of the University. When professors of the Aryan and Semitic languages are convinced that Mr. Brown has a competent knowledge of these tongues, they will be prepared to believe that he knows accurately Accadian and "Hittite," and to accept his conclusions; meanwhile Mr. Brown's present work will delay that result.

## DEVELOPMENTAL MECHANICS.

*Programm und Forschungsmethoden der Entwicklungsmechanik der Organismen, leichverständlich dargestellt.* Von Wilhelm Roux, o.ö. Professor der Anatomie und Direktor des anatomischen Instituts zu Halle. Zugleich eine Erwiderung auf O. Hertwig's Schrift Biologie und Mechanik. Pp. 203. (Leipzig: Verlag von Wilhelm Engelmann, 1897.)

IT is questionable whether Dr. Wilhelm Roux does not do more harm than good to the cause which he has at heart by his excessive fondness for programmes. The work which lies before us is at least the fourth of a series of expositions of the nature, aims and methods of the subject of developmental mechanics, and it differs but little from its predecessors (consisting as it largely does of extracts and quotations from them, with explanatory and justificatory additions) in the complacent, not to say assertive, manner in which its author extols his own methods and aims at the expense of those which have hitherto been in use among zoologists. To our thinking Dr. Roux's weakness lies not in his aims, which are legitimate and praiseworthy, nor in his methods, which are carefully considered, but in the persistence with which he lectures his colleagues on their shortcomings and on his own rectitude. Different persons are differently affected by oft-repeated homilies: some will acquiesce, the greater number will escape by indifference, and others will be goaded into active hostility to what they regard as the pretensions of the author. To the last category belongs Dr. Oscar Hertwig, who has recently attacked Roux in an unsparing manner, asserting that his programme is obscure and wanting in novelty; that since it is not new the very name of developmental mechanics is superfluous and, moreover, incorrect; that the method, in so far as it is new, cannot lead to any progress in biology; that it is inapplicable to the subject; and finally, that in so far as it has been applied by Roux, it has been applied in so faulty and slovenly a manner as to have produced error instead of enlightenment.

The issue between the new method and the old is very clearly raised, and the present work is chiefly concerned in repelling Hertwig's attack. It would take far too much space to attempt to describe the numerous questions which enter into the dispute, questions which involve discussions on the laws of causation, on the theory of mechanics, on nomenclature, and on numerous matters of fact.

Our general impression after reading Roux's article, is that he has come out of the contest with credit, and that in some particulars he has successfully overthrown Hertwig's attack. It must be remembered that Roux is by no means an empty theorist: he has preached, as we think, over-much, but he has also practised largely and with great success, and whatever *à priori* objections may be taken to the methods which he inculcates, he has been able to show us, by the results which he has himself achieved, that the method of experiment may be applied with great advantage to the elucidation of embryological phenomena. His contention in this and earlier essays is, that the biological methods lately in vogue are purely descriptive and based upon simple observation, and that therefore they do not, and cannot, give a causal account of biological phenomena. To obtain a knowledge of causal relations, one must, says Roux, have recourse to experiment, and further than this, to "causal analytical experiment."

It is not quite easy to understand the antithesis between simple experiment and causal analytical experiment, though our author evidently attaches special value to the latter term, for he repeats it again and again. Seemingly it means nothing more than that every experiment should be conducted with strict attention to the particular question to be solved and with due regard to secondary and disturbing influences, conditions which, to the ordinary uninstructed person, would seem to be necessary to every experiment worthy of the name. This, however, is a matter of secondary importance; Roux insists specially on the use of experiment—accurate painstaking experiment—in biological investigation. He further indicates that developing organisms afford the most fruitful field for the experimental method, for there one may most certainly hope to discover the formative forces which by their interaction co-operate to produce those formal changes which we have come to know by the method of simple observation. It is on this subject that Hertwig differs most widely with him. According to the latter author, there is no place for the experimental method in embryology. Experiment is nothing more than the production of changes of state in existences. In the inorganic world we have to deal with relatively stable existences, and before we can make any assertions of cause and effect about them we must bring about a change of state in them. In the organic world, however, the case is widely different. It is the characteristic of living bodies that they are always undergoing changes of state, and the changes are most characteristic and most conspicuous during the period of embryonic development. Thus nature does for man in the organic what he himself has to effect in the inorganic world, and it is only necessary for him to observe and record the natural successive changes in order to be able to state a series of relations of antecedent and consequent. Thus Hertwig says—

"Every antecedent state is the cause of that which follows it . . . a living frog's ovum is the antecedent which of invariable necessity leads to the establishment of a frog's gastrula as a consequent, if only the conditions and circumstances necessary to further development are fulfilled. For the words antecedent and consequent one may equally well substitute the words cause and effect. Hence embryological research, which 'describes' the change of the frog's ovum into the gastrula, asserts a causal relation, and in so far as it does this for all the stages of the development of the frog from the egg, it asserts the law of the development of the frog. In this sense the research of the last fifty years has brought to light the most important causal knowledge. Is not the recognition that the ovum and the spermatozoon are simple elementary organisms and that, as such, when the appropriate conditions are fulfilled, they unite in themselves all the causes (exception being made of *causæ externæ*) which are necessary to the production of a new being, and that they in fact bring it into existence, is not this a causal recognition?"

The above paragraph is quoted by Roux as illustrating very clearly the difference between his and Hertwig's standpoints. Hertwig imagines that the ends of science are fulfilled by the enumeration and description of different states, and holds that our task is finished when we are able to assert that any one state invariably proceeds from another state immediately preceding it. Roux admits the necessity and value of this knowledge, but declares that it is only a step towards a causal explanation of the phenomena, and is far from satisfying our desire for a full explanation.

An illustration will serve to make the point clear. Hertwig's position would be that of an astronomer who was content with the truth arrived at by Kepler, that the observed successive positions of the planets are due to their paths being elliptical. Having ascertained the nature of the planets' orbits, he would be justified in asserting that the observed positions of the planets were due to—that is, were caused by—the fact that their paths are elliptical. But this would not be a sufficient causal explanation of the planetary movements. There is clearly a further question as to why the paths are elliptical, and the elucidation of this question was reserved for Newton. Hertwig would suggest that embryological inquiry should stop short at a point analogous to that gained by Kepler, and that we should content ourselves with the assertion that the states which we observe in individual ontogenies are what they are because the organisms in question describe a sort of normal curve in the courses of their development. It is hardly possible to refuse one's sympathy to Roux when he declines to be content to stop at this point, and urges that the knowledge hitherto acquired is but a preliminary to further inquiry. Everybody who has studied and reflected upon the facts of embryology must have felt the necessity for further enlightenment as to why, and in virtue of what inherent energies the ovum is able to go through the complex succession of changes which lead to the establishment of the adult individual. Various theoretical solutions of the problem have been offered, but they have not proved satisfactory. Roux steps forward and shows that the only possible solution is by the method of experimental investigation. Since he himself admits that the problem was present to the mind of von Bär, it is clear that his aim is not new, and in this unimportant matter Hertwig is right; but if the aim is not new, it has only recently become practical, and Roux may lay claim to the chief credit of having seen that the time was ripe for trying to realise it.

But it is one thing to have a legitimate and definite object in view, another thing to devise the most appropriate means of attaining to it. Roux has entire faith in experiment. Hertwig objects to the experimental method, because in the act of making an experiment one disturbs the normal course of vital phenomena, and obtains abnormal results, from which nothing can with certainty be predicated as regards natural processes. Bütschli has in a similar sense objected that the introduction of disturbing factors into ontogeny involves a complication in the results, which can only be justly estimated when the elements of the mechanics of normal developmental processes are well ascertained. The answer to this is that no progress is possible if one allows one's self to be discouraged by *à priori* objections and difficulties, and that the method of experiment, so far as it has gone, has been successful almost beyond anticipation.

As regards the title "Developmental Mechanics" (*Entwicklungsmechanik*), which Roux justifies at some length, it need only be said here that the equivalent "Experimental Embryology" most generally used in England and America, though not expressly disavowed by him, differs in its connotation from the title which he has selected. Thus on p. 176, "Entwicklungsmechanik bedeutet also die Lehre von den Entwicklungsbewegungen": the essential idea is not contained in the term Experimental Embryology.

Roux's last task is to defend his practical methods and results against the criticisms of Hertwig, who has not hesitated to say that his preparations were so imperfect in point of histological technique that nothing could be inferred from them. Roux retorts with a criticism of Hertwig's control experiments on the same objects (frog's ova), and it is difficult to decide between two observers who mutually accuse each other of inaccuracy and want of attention to detail.

So far as one can judge the advantage in the polemic lies with Roux, the more so because he invites our confidence by asking any one who is interested to come and inspect his preparations of hemiembryos, and to judge for himself whether or not he has described them truly, and whether they do not support the theoretical conclusions drawn from them.

## BRITISH VERTEBRATES.

*A Sketch of the Natural History (Vertebrates) of the British Isles.* By F. G. Aflalo. 12mo, pp. xiv + 498. Illustrated. (Edinburgh and London: Blackwood and Sons, 1898.)

WITH the host of books in existence on British animals, it is a somewhat curious fact that, so far as we are aware, there is none which treats of all the vertebrates collectively, with the exception of Jenyns's "Manual," published in 1835. Still more curiously, that particular work happens to be omitted from the very useful bibliography Mr. Aflalo gives at the end of his little volume! Under these circumstances, the work before us fills a distinct gap; and as it is beautifully illustrated and brightly written, it ought to command a ready sale among those desirous of knowing something about the higher animals of our islands without being bored by technicalities.

Needless to say, it is not a book for the professed naturalist, and should not therefore be criticised from his standpoint. It has no pretence to be an advanced educational text-book; but is intended to appeal to those who have the "field-fever" strongly developed, and who are certainly in need of a cheap and portable volume dealing with all the vertebrates to be met with by field and flood in the British Isles. To be as accurate as possible without being dry, to produce a chatty little handbook, and not a dissecting-room manual, seems to have been the main object of the author; and in this laudable endeavour, in our opinion, he may fairly claim to have succeeded.

One very notable feature in the book is that scientific names are relegated to a series of tables, prefixed to the groups to which they refer, and that in the text the animals appear under the popular designations alone. This certainly renders the volume much more readable than would otherwise be the case. Special attention is given to the life-history of each animal treated; but descriptive details sufficient to distinguish the species from its British relatives are added, and in those cases where we have perused them, appear all that can be reasonably required.

Any nomenclatural list is now-a-days open to criticism, were we disposed to be critical on this subject. But in the main the author appears to have steered a fairly middle course between extreme innovations and old-fashioned views. In one case he is clearly wrong—namely, in calling the marten *Martes sylvatica*, and restricting *Mustela* to the polecats and weasels. In birds, we are glad to see he employs genera mostly in a wide sense, so that the blackbird and ouzels appear in the same genus as the song-thrush. But these are details in which his readers have probably little or no interest, and which his critic may therefore leave alone.

If we might suggest an improvement, it would have been to curtail the amount of space devoted to the sperm-whale, which scarcely comes under the designation of a British animal, and to give more details with regard to some of the smaller mammals. For instance, a little more might have been added as to the colour-changes of the squirrel, and the distinctive coloration of the tail of the British form; while further information as to the black variety of the water-vole being restricted to damp localities might have been desirable. Perhaps, however, the author is better acquainted with the tastes of his readers than is his critic; and personally we confess to much more interest in reading the anecdotes relating to ambergris than we should in wading through details of coloration of fur and feathers—important as these latter undoubtedly are in their proper place.

As regards paper, type, illustrations (from the facile pencil of Mr. Lodge), and freedom from misprints, the volume appears all that can be desired. As an Easter gift to friends, whether young or old, interested in the natural history of our own islands—which is the proper commencement of zoological studies—no volume could be more appropriate. R. L.

## OUR BOOK SHELF.

*Canada's Metals.* By Prof. Roberts-Austen, C.B., D.C.L., F.R.S. Pp. 46. (London: Macmillan and Co., Ltd., 1898.)

THE address which Prof. Roberts-Austen delivered at the Toronto meeting of the British Association last year, and afterwards repeated at the Imperial Institute, was so well received on each occasion that there must be many who will welcome its appearance in book form. The main object of the address was to indicate the nature and distribution of Canada's mineral wealth; but, to lend additional interest to the subject, and afford a base for experimental illustration, a specific metal—nickel—which is especially Canada's own, was given the most prominent place in the discourse.

How great is the mineral wealth of the Dominion is understood by all who know the work and publications of the officers of the Canadian Geological Survey. Report upon report have been published on the mineral resources of the various provinces, but they have mostly gone unrecognised in England, and British efforts have been tardy in developing the riches in Canadian territory. Ten years ago Dr. Dawson published his exhaustive and glowing report on the mineral wealth of British Columbia, in which he pointed out the richness of the region in auriferous deposits, and stated that alluvial gold would probably be found in the bed of every tributary of the Yukon. Had British capitalists known how to value reports of this character, they would long ago have developed the Yukon basin instead of waiting until the success of placer mining at Forty Mile Creek in 1896 called public attention to the extraordinary richness of the district in precious metals. The facts brought together by Prof. Roberts-Austen will, however, help to make the extent and variety of Canada's mineral deposits better known than they have been, and will also show that, large as is the output at the present time, it will certainly be enormously exceeded in the future.

From the general subject of Canadian mineral resources, and the need for their development, Prof. Roberts-Austen passes to a particular metal—nickel. The splash of a falling marble which is dropped into milk, and of a gold bullet dropping into molten gold, is shown, by means of reproductions of photographs, to bear a resemblance to the splash produced upon armour plates by projectiles. To prevent the marble from entering the milk, the surface of the liquid might be hardened by freezing it. Using this illustration, Prof. Roberts-Austen ingeniously explains that in a similar way an armour plate should have a face of rigid steel to break up a projectile, and a tough back to save the plate from fracture. These conditions are obtained by the addition of 4 or 5 per cent. of nickel to steel.

There are many curious points connected with the relations of iron and nickel, and several of scientific interest are described in the present volume. Every one interested in the properties of metals, or desirous of obtaining a concise and trustworthy account of Canada's mineral riches, should read what Prof. Roberts-Austen has to say upon these subjects.

*Hann, Hochstetter, Pokorny—Allgemeine Erdkunde, Fünfte, neu-bearbeitete Auflage.* II. Abtheilung: Die feste Erdrinde und ihre Formen. Von Ed. Brückner. Pp. xii + 368. (Wien: F. Tempsky, 1898).

IN undertaking to produce a new edition of Hochstetter's share in the *Allgemeine Erdkunde*, Prof. Brückner very wisely determined to rewrite the whole section, and so to bring it into line with contemporary methods and results. The scope of this treatise on the crust of the earth and its forms includes a sketch of petrography, geological structure, stratigraphy, the agencies which work on the earth's surface (classed as endogenous and exogenous), the forms of the crust, and the morphology of the land-surface.

Prof. Brückner follows Richthofen and Penck for the most part; but his range is wide, and he pays due regard to the work of British and American geologists. It is particularly noteworthy that an authority who knows the Alps so well should refrain from making them the main source of his illustrative examples. In speaking of the interior of the earth the author leans to the view of the central part being in a gaseous state, the gaseous rock being reduced by intense pressure to a higher density than any liquid known on the surface; but he quotes and very impartially discusses the more generally accepted view of a solid earth due to the raised melting-point of rocks under pressure. Earthquakes are treated at some length; but the work of Milne is not referred to, Rebeur-Paschwitz being the principal modern authority cited. In discussing the origin of land-forms, more weight is given than in most text-books with which we are familiar to the importance of tilted or vertically displaced blocks of crust, and relatively less importance is attributed to folded structures. In treating of the *régime* of rivers and the classification of land-forms, Prof. Brückner follows Penck closely.

A number of useful references are given to special works treating on the special departments under notice; and it is gratifying to find a fair proportion of English books amongst those cited. In speaking of caverns, however, the author fails to mention M. Martel's important researches, or to refer to the Speleological Society. The revision of the work is very thorough; the only serious misprint of proper names we have noticed is the citation of the author of the *Mundus Subterraneus* as "Kirchner" in place of "Kircher."

This important work, so well-written by a master of his subject, is simply one amongst many German books on physical geography, a class still very poorly represented in the English language. H. R. M.

*Elementary Botany.* By Percy Groom, M.A., F.L.S. Pp. x + 252. (London: G. Bell and Sons, 1898.)

IN his preface the author explains that his object has been "to place the subject before elementary students in such a way as to exercise to the full their powers of observation, and to enable them to make accurate deductions for themselves from the facts which they observe." The book is written on the assumption that a compound microscope is not employed; and in the section on physiology no knowledge of the histology of plants is assumed. There are already numerous books more or less suitable as guides to the student of elementary botany, some of them so excellent as to leave little, if anything, to be desired in their special fields. But they either omit a good deal that might readily enough be examined and verified even by beginners, or they require such a use of the compound microscope as is scarcely practicable in the teaching of botany in schools. A book on the lines indicated by Mr. Groom should prove very helpful alike to beginners and to teachers, and would doubtless be welcomed if felt to be the result of adequate personal experience. But we cannot altogether congratulate the author on his success in carrying out his objects, despite the merits of his work, especially if it is intended as a school-book. Children can scarcely be expected to benefit as much from the study of general morphology as from the examination of selected plants, in which they could observe and gradually become familiar with the various structures and life-histories.

The definitions of terms are at times scarcely in keeping with general usage; for example, those of *compound leaves*, *æstivation* and *vernation*, and *compound fruits*. It may be questioned whether the statement—"that portion of a single flower which persists after fertilisation until the seeds are ripe is termed the fruit"—is preferable to the usual definition. The classification of fruits also is unsatisfactory.

Such a statement as that "a root can only produce as lateral members branches like itself" is misleading, and indicates want of care. The production of buds by roots can easily be verified; indeed, the author refers to their growth on roots under "adventitious shoots."

In the physiology a knowledge of chemistry is assumed to an extent beyond what is to be looked for in many schools. In consequence a good deal of this section could be little more than words to those for whom the book seems to be intended. The plants treated of all belong to the flowering plants, though there seems no good reason why representatives of the larger cryptogams should not find a place in such a work. But the task of a censor is unpleasant; and although it has been necessary to criticise what must impair the usefulness of the book, we gladly recognise that it should often be found suggestive by teachers and others possessed of sufficient knowledge to avoid being misled where the risk exists. The book is well printed, and is of very convenient size, and the illustrations are good and numerous; but it would have made them more useful had some of them been repeated where more than once particularly referred to and explained. References to figures, sometimes many pages back, are apt to be irritating.

*Alembic Club Reprints.* No. 13. *The Early History of Chlorine.* No. 14. *Researches on Molecular Asymmetry.* Pp. 46 and 48. (Edinburgh: W. F. Clay, 1897.)

THE first of these reprints contains translations of papers by Carl Wilhelm Scheele (1774), C. L. Berthollet (1785), Guyton de Morveau (1787), and J. L. Gay-Lussac and L. J. Thenard (1809). This volume, together with the earlier reprint in this series (No. 9), containing Davy's researches, completes the history of chlorine from its discovery by Scheele to the proof of its elementary nature by Davy. The importance of this discussion upon the development of chemistry is obvious, but it is somewhat difficult to step back from what is now common-place knowledge, to the standpoint of these early pioneers. The paper of Scheele, although worded in terms of the theory of phlogiston, is remarkable for its terseness and lucidity, and for the clear and correct ideas expressed upon the nature of the new gas. Indeed, if the word hydrogen be substituted for phlogiston, Scheele's explanation of the action of hydrochloric acid upon the black oxide of manganese almost represents our present knowledge. Berthollet, on the other hand, writes very voluminously upon a very slender experimental basis, and as an ardent exponent of the views of Lavoisier, concludes that chlorine gas is the oxide of an unknown radical, and this fixed idea leads to quite erroneous interpretations of observed facts.

That the effect of a preconceived idea, however, is not always prejudicial, is shown in the two lectures by Pasteur on Molecular Asymmetry, which form the contents of the second of the reprints under notice. Here Pasteur distinctly states that but for his preconceived idea as to the inter-relation of hemihedry and rotatory phenomena, he would not have discovered the opposite hemihedry of the paratartrate and tartrate of soda and ammonia; a difference missed by so careful an observer as Mitscherlich.

The English translation of these famous lectures possesses all the charm of the original. In them we have a complete account of Pasteur's work on optically active compounds, and, as the editor states in the preface, it is remarkable that the three ways of separating optical isomers here described are still the only ones known, and that there is scarcely a statement which would be changed if the whole were to be written to-day.

*Practical Toxicology for Physicians and Students.* By Prof. Dr. Rudolf Kobert, late Director of the Pharmacological Institute, Dorpat, Russia. Translated and edited by L. H. Friedburg, Ph.D. Pp. xiii + 201. (New York: William R. Jenkins, 1897.)

THE work before us is a translation of a book by Prof. Kobert, the second edition of which was issued in 1887. While the author was engaged upon his "Lehrbuch der Intoxicationen," by which he is for the most part known in this country, and with which the present work must not be confused, he allowed the latter to run out of print. In 1894 he wrote the third German edition, and it is this which Dr. Friedburg has now translated and edited, three years after its issue. As we have not had the opportunity of seeing the third German edition of the original, we are unable to measure either the quality or extent of Dr. Friedburg's editing. With regard to his translating, it is the worst which has ever come under our notice. In fact the English language, in Dr. Friedburg's hands, is extremely difficult to understand. As this is a very strong statement it behoves us to give an instance, which, by the way, is not the worst we could find. Dr. Friedburg is speaking of a rise of blood pressure of peripheral origin. "If this is the case, the rise must obtain after the injection of the poison into the blood of an animal even if the marrow of the neck has been cut through and whose spinal marrow has been drilled out." We quote this instance, since it shows that the author is not only deplorably ignorant of the English language, but has no knowledge of the English equivalents of German physiological expressions. Dr. Friedburg's Latin is no better than his English; the plural of vagus is always written "vagii," and so polymorphic is the declension of this noun that we find the nominative singular written "vagis."

To turn from the manner of the book to the matter, it is undoubtedly full of information, and, if properly translated by some one acquainted with pharmacological method and the English language, would be valuable to both the pharmacologist and toxicologist. F. W. T.

*What is Life? or, Where are we? What are we? Whence did we come? and Whither do we go?* By Frederick Hovenden, F.L.S., F.G.S., F.R.M.S. Pp. xiv + 290. (London: Chapman and Hall, 1897.)

MANY matters are dealt with in this book, ranging from the stellar universe to cell structure. About half the text is made up of quotations from the writings and utterances of men of science, distinguished and otherwise, and the remainder consists of perplexing conclusions which the extracts are held to support. Excessive zeal is shown in establishing fundamental truths, but that may be forgiven. It is when the author expands into the ether, so as to embrace in his comprehensive idea such diverse subjects as the Pentateuch and the currency question, that we lose the connections of the argument. The chief conclusions arrived at are stated in the following words:—

"From the combining power of the strongest species of atoms under the influence of Ether, arises the formation of cells.

"Cells under the influence of the strongest cell group themselves to form highly complex structures or organisms, hence the most complex of all organisms—Man. The activity of cells forms that activity we call Human Life. Thus Life is the sum of the activity or energy of molecules formed of atoms.

"The power of the regeneration of molecules causes regeneration of cells, and this causes regeneration of Life. Life is eternal."

*La Tuberculose et son Traitement hygiénique.* Par Prosper Merklen, Interne des hôpitaux de Paris. Edited by Felix Alcan. Pp. 190. (Paris: Ancienne Librairie Germer, Baillière et Cie.)

THIS little book forms No. cxix. of the "Bibliothèque Utile" series, and is certainly calculated to serve a useful purpose. It addresses the public, and not the medical profession. The nature of tubercular disease is very clearly and accurately set forth in plain language, together with its chief manifestations in man, and the principles underlying its prophylaxis and treatment. It is indisputably true that in the case of a preventable disease like tuberculosis, which constitutes one of the main scourges of civilised man, a dissemination of sound knowledge on the subject is the first necessary step in educating public opinion up to the hygienic requirements and sanitary restrictions which are demanded to check its spread. The present brochure is a creditable effort in this direction: the author has succeeded in placing home truths on the subject in a very clear light, and his remarks cannot fail to be of direct benefit to the public.

*Marriage Customs in Many Lands.* By the Rev. H. N. Hutchinson, B.A., F.G.S. Pp. xii + 348. (London: Seeley and Co., Ltd., 1897.)

MR. HUTCHINSON, forsaking geological subjects for a time, presents in this volume a purely popular account of the quaint customs connected with marriage in many parts of the world. He has not attempted to discuss the scientific questions relating to the history and origin of human marriage, but has merely aimed at providing the general public with readable descriptions of curious nuptial ceremonies of various peoples and races. The readers for whom the volume is intended will find much to interest and amuse them in it; and the excellent illustrations—among the best of their kind—give the book additional attraction. Authorities may not agree with all Mr. Hutchinson says; but, as the book is a compilation, the mistakes are usually the mistakes of the sources from which the information has been derived, and the only criticism that can be offered is whether the author has exercised sufficient discrimination in the collection of material.

## 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.*]

### Misleading Applications of Familiar Scientific Terms.

MAY I, not as an expert in science, but as one who has made some research into the conditions of lucidity, venture to thank you for the protest which appears in your current number against a misleading application of the familiar term "Light"? This is not of course the only instance of the kind; but it seems especially regrettable as tending, by the very success and popularity of the Lectures reviewed, to introduce gratuitous confusion into youthful minds.

I may perhaps be pardoned for adding that I was fortunate enough in my little book, "Grains of Sense," published last year, to anticipate the verdict of your reviewer, and to point out how much, in this and similar cases, such modes of expression on the part of scientific men tend on the one hand to diminish our precious and too slender store of clearness of thought, and on the other to hinder the progress of science itself. V. WELBY.

April 1.

### The Kinetic Theory and Radiant Energy.

IN the course of the discussion which took place in your columns during the winter of 1894-5 on the kinetic theory of gases, emphasis was rightly laid on the difficulty of reconciling the law of partition of energy among the different degrees of freedom of molecules of gases with the large number of such degrees of freedom indicated by their spectra, and, generally, of explaining, on the kinetic theory, the relations between matter and the ether required to account for radiation. It was even suggested, by one writer, that the ether, with its vastly larger number of degrees of freedom, must ultimately absorb all the energy of the molecules. I instanced the case of a sphere moving in an infinite mass of perfect liquid as exemplifying a system where no such ultimate absorption of energy would take place, and pointed out that everything depended on the laws according to which transference of energy took place between the molecules and the ether.

The object of this letter is to show that the subsequent discovery of the Röntgen rays has suggested a theory of the radiation of heat which may possibly throw considerable light on the difficulties referred to by affording an answer to the question, "If the temperature of a gas is proportional to the mean translational kinetic energy of the molecules, how comes it that this kinetic energy can be transferred from one set of molecules to another by radiation through the ether?"

Consider the Röntgen rays: we know, firstly, that they are produced by the impact of the kathodic rays on the Crookes' tube, these latter consisting not improbably of streams of bombarding molecules; secondly, that they not only have the power of discharging electrified bodies, but also of modifying the electrical state of gases in such a way as to enable these to discharge bodies. In this modified air, to which Villari has applied the somewhat barbarous name of "aria Xata" or "xd. air," some kind of dissociation of the electrons must necessarily have taken place.

Arguing from analogy the idea suggests itself that the encounters between molecules of a gas, no less than the kathodic bombardments, may give rise to radiations, and these, too, when falling on another mass of gas may modify the electrical state of its molecules in such a way that their original electrical state is only restored by encounters between them.

Now taking, as a simple illustration, two oppositely electrified perfectly elastic conducting spheres; as these approach one another, they acquire kinetic energy in virtue of their attraction. On coming into contact they are discharged and the attraction ceases, so that their kinetic energy of separation is greater than that which they had previously to coming within each other's influence. Again, when a charged and an uncharged body impinge, the charge is distributed between them; they repel one another as they separate, and again acquire an increase of kinetic energy—as in the ordinargy pith-ball experiment.

It follows that the incidence of rays possessing the property suggested above will tend to increase the temperature of a gas.

The discharge which takes place at an encounter will, however, be an oscillatory one, and will lead, therefore, to further generation of undulatory rays.

Considering two masses of gas at unequal temperature, the impacts in the hotter gas, being the more frequent and violent, will give rise to the more copious emission of rays, and these falling on the cooler gas, will produce the greater electric dissociation resulting in the greater acquisition of kinetic energy in collisions between the molecules. The feebler rays from the colder gas will have less effect on the molecules of the hotter one, and the kinetic energy supplied in this way will not compensate for that lost by radiation. Thus the "theory of exchanges" will hold good.

A still more important consequence of such a theory is that no interaction will take place between the ether and molecules except where there are encounters between the latter, and, moreover, the interactions which occur in an isolated mass of gas will not affect the translational velocity of its centre of mass, nor the angular momenta about axes through its centre of mass. Thus it results that the celestial bodies go on in their course experiencing no resistance whatever from the ether.

On the other hand, the fact that light from distant stars is not absorbed before it reaches the earth, no longer implies the complete absence of matter in interstellar space. Isolated molecules will absorb no energy from the ether; and so long as the molecules moving about in interstellar space are assumed to be so few and far between that collisions practically never occur, there will be nothing to impede the passage of light or heat rays. It is only when such rays fall on assemblages of molecules sufficiently dense to possess the attributes of what we call *matter*—as, for example, when they reach our atmosphere—that absorption of energy will take place.

The phenomena of irreversibility and of degradation of energy would thus, so far as the present view goes, be restricted to material bodies, and hence the conditions necessary for the existence of life on our earth may have been brought about without the enormous waste of energy which would be required in the absence of *some* such theory.

A photo-voltaic theory of photographic action formed the subject of exhaustive experimental investigation at the hands of Herr Luggin last year, and photo-voltaic theories of vision have also been proposed. It would thus seem that the analogy between the action of heat rays, visible-light rays, ultra-violet rays and Röntgen rays may be complete. The question still remains, *how* are ethereal waves able to affect the electric state of assemblages of molecules? But since Röntgen-ray physicists have proved that they do this, the question has to be faced in any case. It is now rendered no more difficult, and, on the other hand, our theories of the relations between ether and matter are simplified by referring radiation of heat to the same phenomenon. G. H. BRYAN.

### Note on Mr. Wood's Method of Illustrating Planetary Orbits.

I FEAR that Mr. Wood's beautiful method of illustrating planetary orbits by means of a bicycle ball rolling on a glass plate about the pole of an electro-magnet (NATURE, April 29, 1897), has rather fallen into disrepute in the minds of many physicists since its criticism by Mr. Anderson in NATURE, May 13, 1897. Mr. Anderson there states that the law of attraction in such a case would be that of the inverse fifth power of the distance. This could only be true if the ball were of very soft iron. A bicycle ball is far from this, and becomes strongly magnetised after brief use in the experiment, behaving like a permanent magnet of great coercive force. Under these conditions the attraction between the pole and the ball will vary approximately as the inverse third power. There is also another factor to be considered. If the true pole lies below the glass plate, only a certain component of the total force is active in producing the attraction towards the centre of motion. To determine what the law of variation of this component will be, I have had one of my students take a number of series of observations on the attraction of a bicycle ball along a plane perpendicular to the axis of a magnet.

In the experiments the magnet was horizontal, and the bicycle ball with its magnetic axis vertical was fastened to one end of a strip of spring brass, the other end of which was clamped fast in a sliding clamp so as to be raised and lowered. The bending of the brass strip under the attraction of the magnet on the ball was measured by means of a telescope and scale, the mirror being fastened to the end of the strip. As the motion of the ball was entirely in a plane perpendicular to the axis of the magnet, the law of variation of force must have been very nearly the same as in the orbit experiments.

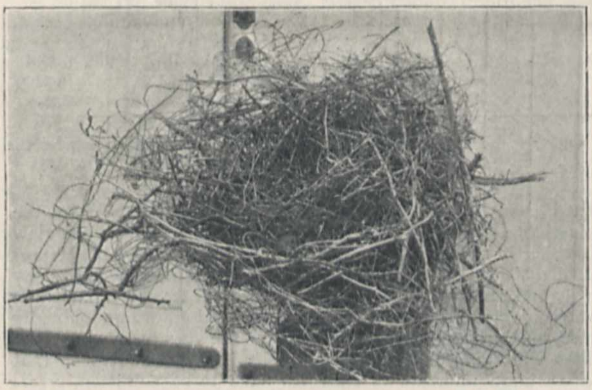
When the ball was directly over the true pole, which lay about 1 cm. from the end of the conical pole-piece, the law was nearly that of the inverse cube, the observations being taken between the limits of 3 cm. and 14 cm. from the axis of the magnet. Other series were taken with the plane of motion of the ball at different distances from the pole, and it was found that with the plane of motion at 2·8 cm. from the true pole the law of the inverse square was very closely obeyed between 4 cm. and 14 cm. from the axis. These limits cover the region in which the orbits would in most cases be formed. The exact law of force as determined by least squares from seven observations between the above limits was that of the 2·1 power of the distance.

LOUIS W. AUSTIN.

The University of Wisconsin, Madison, Wis., March 16.

### An Extraordinary Heron's Nest.

I SEND you a photograph of probably the most extraordinary heron's nest ever discovered in this or any other country. During a gale it was blown from the top of an elm tree in the heronry on Stoke Hall estate in Notts, the seat of Sir Henry Bromley, Bart. It is of unusual size, and almost exclusively composed of wire of varying lengths and thickness; the centre, or "cup," alone being composed of fine twigs, grasses and feathers. Several other nests of the heronry, which had also been blown down, contained pieces of wire cleverly worked in with twigs in the usual way, but this was the only one entirely composed of that material, as far as the main structure is concerned. There are happily now a very flourishing heronry at Dallam Tower, Westmorland, the seat of Sir Henry Bromley's son, Mr. Maurice



Bromley-Wilson, and although I have been familiar with it "off and on" for very many years, and with several other heronries in various parts of the country, I never knew of the birds using wire in the construction of their nests. I have several records of rooks using wire in large quantities in the construction of their nests. Particulars of one very remarkable instance were published in the *Yorkshire Weekly Post* of May 19, 1894, and of another in the same paper for June 23, 1894. Both of these freaks took place in India: one at Calcutta, the other at Rangoon. The other curious feature of the Stoke Hall phenomenon is that there is, and never has been, any lack of ordinary building material, and that all the wire used must have been carried a great distance.

G. W. MURDOCH.

Westmorland.

### "The Story of Gloucester."

REFERRING to your article (p. 221), I think you cannot have looked at pages 70 to 117 of the Gloucester Small-pox Epidemic Blue Book, by Dr. Coupland. I have analysed all these cases, and here is the result.

| Description. | Cases. | Deaths. | Deaths per cent. of cases. |
|---|---|---|---|
| "*Unvaccinated*"— These contain 21 cases, 10 deaths, whose description includes the word vaccination or vaccinated ... ... ... | 679 | 287 | 42·2 |
| "Vaccinated in infancy," no description of vacc. marks ... | 788 | 91 | 11·5 |
| Do. "no marks," very abundant small-pox eruption .. ... | 35 | 13 | 37·1 |
| Do. "one" vacc. mark ... ... | 30 | 3 | 10 0 |
| Do. two do. ... ... ... ... ... | 100 | 10 | 10·0 |
| Do. three do. ... ... ... ... ... | 141 | 13 | 9·2 |
| Do *4, 5, 6, 7 and 8 vacc. marks* ... ... ... .. ... ... | 197 | 13 | 6·5 |
| Do. (?) v. marks, very abundant eruption .. ... ... ... | 9 | 4 | 44·4 |
| Totals ... ... ... ... | 1979 | 434 | 21·9 |

The accepted fatality before Jenner's birth was ... 16 6

| There were— | Cases. | Deaths. | |
|---|---|---|---|
| *Re*-vaccinated cases at Gloucester | 173 | 9 | 5·2 |

These had all kinds of v. marks *up to 8 in number*, and some had been *repeatedly* re-vaccinated; one "often" re-vaccinated. If the same energy had been put into a critical proof of the vaccination of each one as was into avoiding condemning vaccination, there would be little to show, even in fatality, in the above *for* vaccination; as it is, it kills *every* vaccine dogma.

ALEX. WHEELER.

MR. WHEELER, it must be assumed, is wishful to prove that the fatality amongst the vaccinated is as high, or at any rate is not lower than amongst the non-vaccinated. It is surely not necessary for him to separate vaccinated cases into those "with marks" and those with "no marks," since to him it should be immaterial whether a patient be vaccinated or not.

Taking Mr. Wheeler's own classification, we find that of the unvaccinated cases, 679 in number, 287 died, giving a percentage mortality of 42·2; whilst of the vaccinated cases, 1300 in number, only 147, or 11·3 per cent., died. These figures should surely be enough to settle the question as regards percentage mortality, and the mere inclusion of the 21 cases and 10 deaths, whose description includes the word "vaccination" or "vaccinated," does not in any way invalidate the general conclusions to be drawn from these figures.

If now, however, a class for the "under-vaccinated" be included, the second class may be divided into "under-vaccinated" 89 cases with 27 deaths, or 30·3 per cent., and vaccinated 1211 with 120 deaths, giving a mortality of only 9·9 per cent. It is evident that Mr. Wheeler's table in no way conflicts with the figures given in the Report (except in one small particular, noted below), but is based on a misconception of the term "under-vaccination" as used by Dr. Coupland, who used the term to signify those cases of small-pox which had undergone vaccination at any time within the (generally accepted) period of incubation: *i.e.* fourteen days before the appearance of the rash. In the list of "unvaccinated" cases are included a few which were actually vaccinated in the invasion period. No doubt some of these should be placed in the vaccinated class; but others, again, should be grouped in the unvaccinated class. The Royal Commission reckoned the whole group, instead of a large proportion, in this latter class, which is perhaps not strictly scientific and accurate. Mr. Wheeler, however, goes far further astray in including them all in the vaccinated class, which is clearly erroneous. It may be pointed out in this connection that, in his recently published work, Dr. Cory gives some most interesting facts which tend to show that vaccinal immunity is not obtained until nine days have elapsed after inoculation. It would be easy, therefore, from the table on page 149 of the Report, to divide the total 89, there reckoned as "under vaccination," into two sections: (*a*) those vaccinated before, and (*b*) those vaccinated within eight days, of manifesting small-pox. If this were done, there would be added (*a*) to the "vaccinated" class 10 cases with 3 deaths; and (*b*) to the "unvaccinated" class 79 cases with 24 deaths.

Without checking Mr. Wheeler's figures by laboriously going through pages 70–117 of the Report, it is simply necessary to deduct those "under-vaccinated" from his several lists. His classes of "no marks" and "? marks" correspond with Dr. Coupland's groups of "alleged" and "doubtful" vaccination, except that Dr. Coupland's figures give one case less and one death more than Mr. Wheeler's. Although it is highly probable that many of these uncertain and doubtful cases were really unvaccinated, the Report includes them, as does Mr. Wheeler, in the "vaccinated" class (see page 153, &c.).

unvaccinated 255; this, too, in families of the same class, in the same streets, and living under similar sanitary (or unsanitary) surroundings as those in which every child was unvaccinated. May we not legitimately infer that had all the Gloucester children at these ages been vaccinated, only 1/7th of those that did suffer would have suffered, and the mortality would have been less than 1/60th of that to which it did attain? Vaccinators are said to be incapable of viewing this subject impartially, but Dr. Coupland is most judicious in the handling of his figures, and it is apparent that the evidence that he has collected from careful observation weighs with him as much or more than do the figures he has brought together; and it is certain

MR. WHEELER'S FIGURES DISTRIBUTED ON PLAN OF REPORT.

| Mr. Wheeler's table. | | | | Vaccinated. | | "Alleged" vaccination (no marks). | | Doubtful vaccination. | | "Under" vaccination. | | Unvaccinated. | | — | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| — | Cases. | Deaths | — | Cases. | Deaths. | Cases. | Deaths. | Cases. | Deaths. | Cases. | Deaths. | Cases. | Deaths. | Cases. | Deaths. |
| "Unvaccinated" ... ... | 679 | 287 | 42·2 | — | — | — | — | — | — | — | — | 679 | 287 | 679 | 287 |
| "Vaccinated in infancy," no description of v. marks ... ... ... ... | 788 | 91 | 11·5 | 730 | 70 | — | — | — | — | 59 | 20 | — | — | 789* | 90* |
| Do. "no marks," very abundant small-pox eruption ... ... ... | 35 | 13 | 37·1 | — | — | 35 | 13 | — | — | — | — | — | — | 35 | 13 |
| Do. "one" v. mark ... | 30 | 3 | 10·0 | 29 | 3 | — | — | — | — | 1 | — | — | — | 30 | 3 |
| Do. "two" do. ... ... | 100 | 10 | 10·0 | 92 | 8 | — | — | — | — | 8 | 2 | — | — | 100 | 10 |
| Do. "three" do. .. | 141 | 13 | 9·2 | 130 | 9 | — | — | — | — | 11 | 4 | — | — | 141 | 13 |
| Do. 4, 5, 6, 7 & 8 do. | 197 | 13 | 6·5 | 187 | 12 | — | — | — | — | 10 | 1 | — | — | 197 | 13 |
| Do. "?" v. marks, very abundant eruption ... | 9 | 4 | 44·4 | — | — | 5 | 3 | 3 | 2 | — | — | — | — | 8* | 5* |
| | 1979 | 434 | 21·9 | 1168 | 102 | 40 | 16 | 3 | 2 | 89 | 27 | 679 | 287 | 1979 | 434 |
| | | Fatality ... | | ... | 8·7 | — | 40 0 | — | 66·6 | — | 30·3 | — | 42·2 | — | 21·9 |

* Discrepancy due to inclusion by Mr. Wheeler of one death too many among "vaccinated in infancy," and one case too many among the "? vaccinated."

It is difficult to grasp Mr. Wheeler's point in presenting the figures in this way. It might be useful if these questionable cases had all been turned over to the "unvaccinated" class; but why does he detach them from the rest of the admittedly vaccinated? He could not have intended to show, as his own figures do, that post-vaccinal fatality diminishes with a rise in the presumed greater efficiency of vaccination as evidenced by the number of scars. Dr. Coupland does not enter into the question of marks. It has been done over and over again, and in both his Dewsbury and Leicester Reports Dr. Coupland makes a most valuable contribution to this question. The main object of the inquiry at Gloucester was to determine the broad question of the occurrence and fatality of small-pox in the vaccinated and unvaccinated.

Perhaps the most important point that the Gloucester epidemic illustrated is one that is passed over by Mr. Wheeler, and one which unfortunately appears as though the opponents of vaccination in their pursuit of a fad had become callous to the fate, in this instance, of the Gloucester children, but also of the children wherever there is an outbreak of small-pox.

About the effects of the vaccination or non-vaccination of children there can be no dispute. In this connection it is only necessary to refer to the figures of those attacked between one and ten years of age, and especially at the incidence rates given near the end of the Report. Indeed, if only those households are taken in which some vaccinated children are to be found, it appears that the incidence of small-pox *among the vaccinated children* was only 10 to 100, though amongst their unvaccinated brothers and sisters it was 10 to 14; whilst the death rate (per 1000 of those exposed to infection) was for the vaccinated less than 4, for the

that if those who deny the efficacy of vaccination could have the experience that he has had, they would cease to hold the view that he is prejudiced. Any one who considers his Report judicially must confess that he has presented the facts extremely fairly and impartially, and that he evinces far less bias than those who, on very slight and shadowy information, are undoubtedly unreasonably opposed to vaccination—the very people, in most cases, who bring the charge of partiality. Every one knows that where large numbers of statistics have to be collected, errors of fact may creep into records, and that, with fuller knowledge, slight modifications may have from time to time to be made. As regards the main facts of Dr. Coupland's records, however, the most exacting will find it difficult to trace any important inaccuracy. In respect to the records concerning children the facts are indisputable, and lead to the mournful conclusion that amongst these there would have been vastly less suffering and far fewer deaths in the Gloucester epidemic, had not infant vaccination been so widely neglected.

As regards re-vaccination it is difficult to see how Mr. Wheeler obtains the figure 173. In the table (p. 46) there are given 190 who were stated to have been re-vaccinated. Assuming that each of these was really and efficiently re-vaccinated—a large assumption—the fatality would be 4·7, or much below the general vaccination rate. There are, however, several difficulties to be surmounted before a satisfactory demonstration of the relationship of re-vaccination to small-pox can be arrived at; and one of these especially, that of the true interpretation of a failure "to take," is a most important one. This failure "to take" does not necessarily imply that the subject is immune. Then there is also the fallacy of recent re-vaccination which, like recent primary vaccination, may have been done too close to the date of the onset of small-pox to have any influence on the disease (see following table):—

*Small-pox in the "Re-vaccinated."*

190 persons who were *stated to have been* re-vaccinated were attacked by small-pox. Of these:

(*a*) 52 were "re-vaccinated" at various periods prior to epidemic, in some cases several years.

In 37 this re-vaccination was stated to have been successful, and 2 of these patients died.
In 15 this re-vaccination did "*not take*"—1 died.

(*b*) 30 were "re-vaccinated" between 3 months and 14 days of the attack of small-pox.

In 8 the vaccination "took."
In 22 ,, ,, "*did not take.*"

(*c*) 108 were "re-vaccinated" within 14 days of appearance of small-pox eruption, some of them even in early days of attack.

In 83 vaccine vesicles appeared—4 died.
In 25 the vaccination *did* "*not take*"—1 died.

*Where Re-vaccination believed to have been successful.*

(*a*) 37 cases—2 deaths—fatality 5·4 per cent.
(*b*) 8 ,, 0 ,, ,, *nil.*
(*c*) 83 ,, 4 ,, ,, 4·8 per cent.

*Where Re-vaccination known to have been unsuccessful.*

(*a*) 15 cases—2 deaths—fatality 13·3 per cent.
(*b*) 22 ,, 0 ,, ,, *nil.*
(*c*) 25 ,, 1 ,, ,, 4·0 per cent.

Or of whole number, 4·8 per cent.; or if we take whole number (190), irrespective of date or of success, a fatality of 4·7 per cent.

Mr. Wheeler's statement that the accepted fatality before Jenner's birth was 16·6 has very little bearing on the question, since the epidemic at Gloucester gave 21·9, and this, including the 42·2 per cent. unvaccinated fatality at all ages, which is less than that between 1 and 10 years, the period of most fatal small-pox, in the pre-vaccination days. The Gloucester outbreak was undoubtedly unusually virulent; but, surely, equally severe epidemics are on record.

THE WRITER OF THE ARTICLE.

## THE SOUTH KENSINGTON SCIENCE BUILDINGS.

WE are glad to see that the various important matters connected with the extraordinary proposal to spend some eight hundred thousand pounds in interlacing the Science with the Art buildings—chemical laboratories with picture galleries—are being considered by a Parliamentary Committee. This is more especially desirable, since, as we have previously pointed out, it is stated that about half the money proposed to be spent is sufficient for present needs.

The *Times* gives the following account of the meeting of the Select Committee on Friday last, Sir F. S. Powell presiding. Sir John Donnelly, secretary to the department, was further examined. Sir H. Howorth said it would be of great assistance to the Committee if they could get from the officials of the department an expression of their views as to the changes which were desirable or were not desirable in regard to the housing of the Science and Art collections. The witness said that was rather an awkward question; he really did not think it would be proper for him to volunteer any statement which might conflict with the present proposals of the Treasury and the Board of Works. He had already stated that, in his opinion, the Science collections should be on the west side of Exhibition Road and the Art collections on the east side. He believed that that was the proper solution of the South Kensington question, and he had seen no reason in what had taken place since he gave expression to that view to change his opinion. Sir H. Howorth: Mr. Akers Douglas has stated that, with the removal of the residences and of the secretarial offices to Whitehall, the Government find that they will have at their disposal a much larger space than had been previously contemplated, and that therefore they will be able to put the Science and Art collections on the one side of Exhibition Road. Do you think the space thus provided will be sufficient for the whole of the collections being placed together? The witness: I do not think so, and that was my reason for saying that I saw no ground for changing the opinion I have already expressed on the subject. I contemplate that the museums will increase, and I do not think it would be wise to consolidate the collections on one side of the road. In answer to further questions, Sir John Donnelly said he thought it was most desirable that the Geological Museum in Jermyn Street should be transferred to South Kensington. The library which was now in Jermyn Street would be of great value at South Kensington, and under the present system of division they had to duplicate many of the books. He would undertake to bring this view before the Lord President and the Vice-President of the Council. As to the Art side, the theory that it was better to have a large series of small rooms in which they could classify their objects rather than a series of very large halls or rooms was absolutely impracticable in their case. He was distinctly in favour of residences being provided for some of the officers—say four—either in the same buildings in which the collections were housed or very close to them. There was, he knew, a morbid fear of fire being caused when the residences were in the actual building, but he did not himself believe that this was a very great source of danger.

## PHOTOGRAPHY AND TRAVEL.[1]

THE globe-trotter of to-day is almost as notorious for his poor photographs as his ancestor of the Mandeville era was for his traveller's tales. Without instruction in the technical part of his work, and without the geographical training required to teach him what to look for and how to view it, he habitually brings home productions which may be of interest as studies for an impressionist artist, but are of little or no value to the student of nature. Hence it is with particular pleasure that we welcome the republication in a generally accessible form of a selection of Mr. Thomson's magnificent photographs made in China. These were taken before the days of dry plates and snap-shots, when it was necessary to prepare and develop the plates on the spot, and to employ a camera of large dimensions not easy to transport through regions where, to say the least, strangers are not received with overwhelming hospitality.

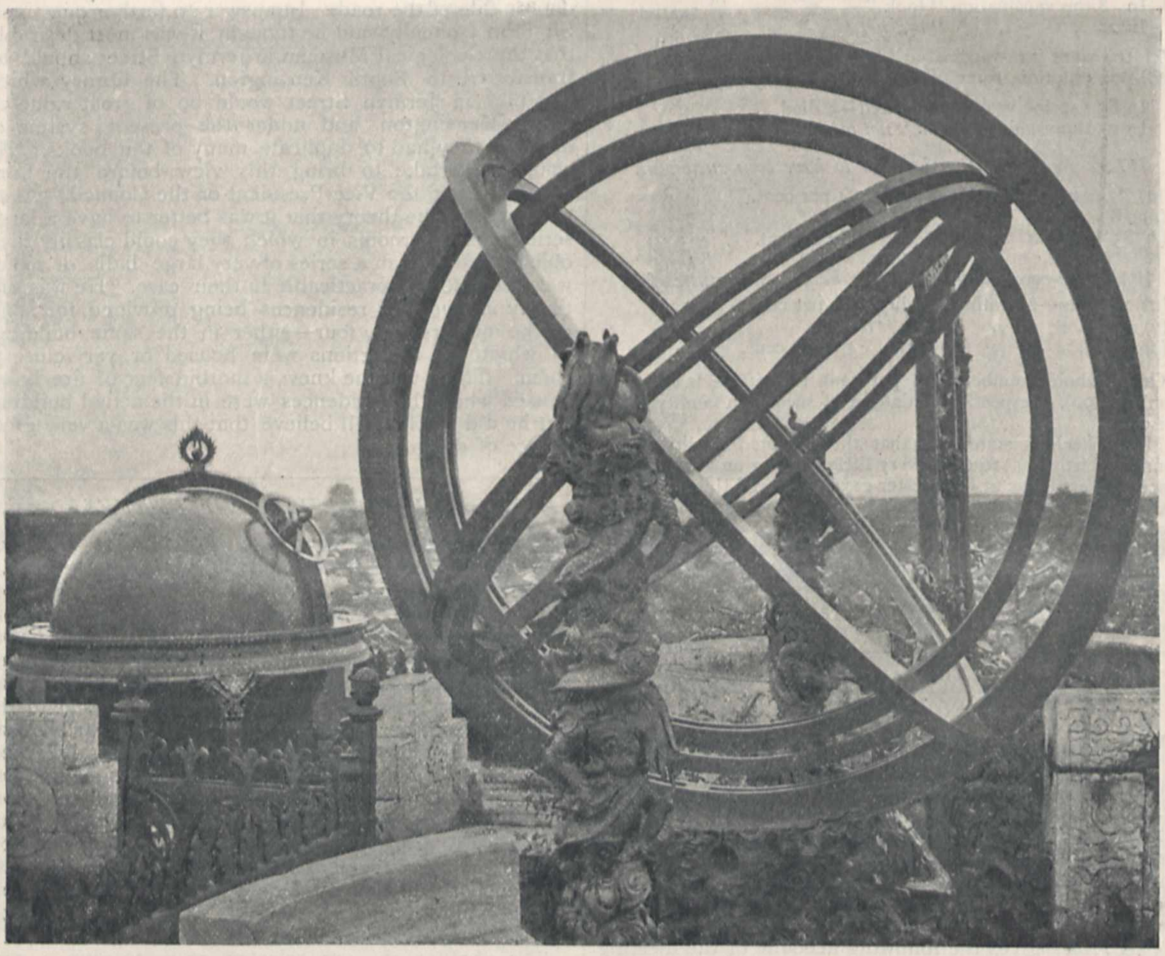
The photographs are selected so as to give a connected idea of life in China proper in all its aspects, and also to illustrate the natural scenery of many of the provinces and of Formosa. The pictures are so satisfactory from every point of view, that it is no slight to say that the letterpress takes a humbler place when one estimates the value of the book. The text for the most part is descriptive of travel, and illustrative of the photographs, incidents and anecdotes being introduced for that purpose. It would have been more useful if the exact order of the journeys and their date had been mentioned; and a map might very well have been added to show the situation of the regions visited.

Three introductory chapters deal with the condition of China now and in the past, and with the Chinaman abroad and at home. Having regard to the somewhat acute interest now being taken in China by the nations

[1] "Through China with a Camera." By John Thomson, F.R.G.S. With nearly 100 illustrations. Pp. xiv + 284. Small 4to. (Westminster: A. Constable and Co., 1898.)

of Europe, the perusal of these chapters should prove useful; and so should the description of the various great centres of population on the coast, in the Yangtze valley and Pekin.

Not the least interesting of the photographs is that which, by the courtesy of the publishers, we are able to give here. The illustration shows two ancient astronomical instruments of purely Chinese construction, which stand on the walls of Pekin, with instruments dating from the thirteenth century, and others constructed for the Chinese Government by the Jesuit missionaries of the seventeenth century. The circles of the instruments of the thirteenth century are divided into 365¼ degrees to correspond to the days of the year, each degree being subdivided into hundredths, but the later instruments have their circles divided into 360 degrees.



Ancient Chinese Astronomical Instruments.

## THE HEIGHTS OF METEORS.

It is perhaps surprising that the heights of meteors, and especially of that class known as fireballs, were not determined with any accuracy until the near approach of the present century. It is true that a few individual attempts were made in this direction but, considering the large number of brilliant meteors which appear every year, it is curious that some systematic attempts were not made at a much earlier date in this direction. It must, however, be remembered that many years ago there were comparatively few astronomical observers, that means of communication were slow, and that the importance of recording these objects as precisely as possible had not been recognised.

The present is perhaps an appropriate period to refer to this subject, for it was in 1798, just a century ago, that the first systematic attempt was made (by Brandes at Leipzic, and Benzenberg at Dusseldorf) to determine the heights of meteors. Schröter had in 1795 seen a shooting-star (in his reflecting telescope of 20 feet focus), the height of which he estimated at more than four millions of miles! Brandes and Benzenberg, however, found from 22 meteors which they mutually observed in 1798, heights varying between 6 and 140 miles. Brandes instituted some further observations in 1823, and of 62 meteors available for calculation 55 were found to have heights between 30 and 70 miles. On August 10, 1838, M. Wartmann, at Geneva, followed up Brandes's inquiries, and derived the average height of the meteors seen on that occasion as 550 miles, and their velocity 240 miles a second. These values, compared with modern observations, were far less accurate than Brandes's earlier ones.

It is not proposed in this paper to deal fully with the average heights of meteors, for that has been discussed by several authorities. The values are about 76 and 51 miles respectively for the mean elevations at appearance and disappearance. In the case of fireballs, however, they penetrate much deeper into our atmosphere than the ordinary shooting-stars, and their heights at extinction appear to be about 30 miles. For the present purpose it is intended to refer to the elevation of these objects at the beginning of their visible flights, for this elevation is so considerable in some cases, that, if atmospheric friction induces their combustion, the air extends to a much greater distance from the earth than is ordinarily supposed.

It is not at all a rarity to find meteors which, at the instant of their first appearance, were more than 100 miles in height. I have looked through various lists of the computed real paths of fireballs and shooting-stars, and find that, out of 577 cases, 116 exhibited a beginning height of 100 miles or more, the average being 130 miles. In fact, one meteor out of five displayed incandescence when 100 miles or more from the earth's surface. The materials from which I obtained these results were by Dr. E. Heis, Prof. A. S. Herschel, Prof. G. von Niessl, and myself. The most extreme heights[1] were :—

| Date of meteor. | Height at beginning. Miles | Authority. |
|---|---|---|
| 1868 September 5 | 483 | G. von Niessl. |
| 1849 August 11 | 216 | E. Heis. |
| 1861 July 16 | 195 | A. S. Herschel. |
| 1862 February 2 | 190 | ,, |
| 1864 August 10 | 188 | E. Heis. |
| 1883 June 3 | 188 | G. von Niessl. |
| 1861 August 10 | 184 | E. Heis. |
| 1864 July 28 | 184 | ,, |
| 1870 September 27 | 184 | G. von Niessl. |
| 1877 March 21 | 184 | ,, |

The first of these is probably erroneous, for the observations, though numerous, were not accordant, and with such data it is possible for different computers to work out anomalous results. Thus, in the instance of the very long-pathed fireball seen in France and Germany in 1868, three paths have been computed, and they differ widely in their character. These differences are induced by the erroneous observations, and the difficulty of putting a consistent interpretation upon them. The radiant point, as adopted by the various computers, is dissimilar ; and this in itself must occasion a great discordance in the heights, for one observer putting the radiant 5° above the horizon will obviously obtain a lower elevation for the beginning point than another who places it 15° above the horizon—the angle of the meteor's descent being much less. In regard to the fireball of September 5, 1868, the following results were obtained :—

| Height at beginning. Miles | Height at ending. Miles | Length of path. Miles | Radiant. ° ° | Authority. |
|---|---|---|---|---|
| 483 | 115 | 1770 | 13 − 3 | G. von Niessl. |
| 69* | 191 | 1000 | 22 − 12 | A. Tissot. |
| 103 | 65—70 | 880 | 18 − 8 | A. S. Herschel. |

Thus, while von Niessl made it descend from 483 to 115 miles, M. Tissot concluded that it really ascended from 69 to 191 miles ! Prof. Herschel's results appear to be the best that can be derived from the materials available, for he obtains normal heights and a slight ascent of the meteor just before extinction. Its enormous length of path is quite beyond dispute.

In every instance where the observations are very inconsistent, it is clear that the results of investigations of this kind must depend largely upon the interpretation put upon them. And for strictly scientific purposes the real paths derived from such materials are of little use, for any critical deductions or trustworthy comparisons cannot be made from them. The instance above alluded to furnishes, however, a very exceptional case ; but it has been selected in proof of the great uncertainty attaching to deductions based upon conflicting observations.

It appears that about 20 per cent. of meteors are at least 100 miles high at the instant of their first visible apparition. This conclusion rests upon a considerable number of results, including a large proportion of fireballs, and may be trusted within small limits of error. From the materials I have examined, I believe the actual height at first appearance of a meteor is *very rarely* as much as 150 miles, and that it seldom reaches beyond 130 miles.

It is singular that in 1897 I found unusual elevations for several meteors, in fact 9 out of 26 (*i.e.* more than one-third), whose real paths I computed, indicated a beginning-height of over 100 miles. These were :—

| Date, 1897. h. m. | Mag. | Height at beginning. Miles | Height at ending. Miles | Length of path. Miles | Radiant. ° ° |
|---|---|---|---|---|---|
| Aug. 2, 11 5½ | 2 | 112 | 90 | 40 | 40 + 55 |
| 2, 11 24 | 5—4 | 139 | 124 | 28 | 73 + 66 |
| 8, 9 15 | > ♀ | 133 | 115 | 63 | 52 + 47 |
| 9, 13 27 | 3—1 | 140 | 77 | 81 | 46 + 56 |
| 9, 13 52 | 3 | 131 | 89 | 56 | 58 + 60 |
| 9, 14 18 | 3 × ♀ | 137 | 75 | 75 | 44 + 45 |
| Nov. 13, 15 28 | 1 | 125 | 77 | 75 | 136 + 9 |
| 13, 15 52 | 1 | 103 | 59 | 60 | 152 + 22 |
| Dec. 12, 8 6 | > ♀ | 112 | 19 | 151 | 80 + 23 |

It is possible that in several of these cases mistakes of identification may have occurred. It must sometimes happen, and especially during the occurrence of a rich shower, that two meteors are recorded at the same time at different places, which show parallax in the right direction, though they are entirely separate objects. Accidental coincidences of this kind would, however, not very often occur, and they would usually be detected by some features of mutual discordance.

There is another point in connection with the first appearance of meteors which merits attention—this is, that observers seldom secure an accurate view of it. The end point is more precisely determined as the eye steadily follows the object until its extinction. But it is rarely the case that even an habitual observer of meteoric phenomena happens to be looking directly to that point of the heavens where a meteor appears. He generally catches it after it has already traversed a section of its flight, and often estimates the extent of its backward trajectory, sometimes adding 5° or 10° to the observed starting-point. Now, a slight error in carrying the visible line of flight too far back may put 30 or 50 miles on the beginning-height of a meteor, especially if it is anywhere near its radiant. It would, therefore, be safer for observers to record the path actually witnessed, without assuming the extent of the portion which escaped them.

But apart from all the uncertainties (which have their outcome in the rough character of the observations) attaching to the subject, it is impossible to put aside the evidence that meteors are sometimes 130 miles and, in extremely rare instances, 150 miles high when they are first visible. There are grave doubts that any meteor has ever been visible at a height of 200 miles. And it is probable that many, if not all, of the instances where heights of about 170, 180 or 190 miles have been found, were due to the commencing points of the flights having been carried too far back by the observers, or that mistakes in the directions have led the computer to adopt erroneous radiants and deduce initial heights considerably in excess of the correct ones.

If photography could step in here, and dispel all the doubts arising from our hurried and often questionable

[1] Other instances of abnormal height might be quoted from the deductions of other authorities, but they are open to serious question. Thus, for the fireball of March 19, 1718, the height at first appearance has been given at 297½ miles ; but Prof. Herschel finds, from a careful rediscussion of the observations, that the meteor began at an elevation of only 80 miles.

* This is the lowest elevation of the meteor as found by M. Tissot, and quoted in British Association Report for 1869, p. 272.

observations, it would be a matter for congratulation. When a meteor is observed by two or more practised observers, the results usually work out very well; but in the case of large fireballs witnessed by a great number of persons, the descriptions are often very conflicting and dubious, and the discussion of such materials is seldom either profitable or trustworthy. W. F. Denning.

## *RUDOLF LEUCKART.*

Rudolf Leuckart, whose death removes one of the most eminent figures in the zoological world, was the son of a bookseller, and was born on October 7, 1822, at Helstedt, which until 1809 had been the seat of one of the universities of the state of Brunswick. A taste for the study of natural history was probably hereditary in the family, for his uncle, Friedrich Sigismund Leuckart (1794–1843), was a zoologist of no mean reputation. The subject of our sketch began his career as an author at a comparatively early age, for whilst still a student at the University of Göttingen he completed the "Lehrbuch der Zootomie" of his teacher, Rudolf Wagner. After serving for a time as assistant in the Physiological Institute of his *alma mater*, he received in 1850 the appointment of extraordinary professor at Giessen, which the genius of Liebig had then raised to a position of great importance among the universities of Germany.

He had already shown what manner of man he was by the publication of two treatises, "Beiträge zur Kenntniss wirbelloser Thiere" (in conjunction with Heinrich Frey, 1847) and "Ueber die Morphologie und Verwandtschaftsverhältnisse der wirbellosen Thiere" (1848), in which the great division *Radiata* of Cuvier was broken up into *Cœlenterata* and *Echinodermata*. He further recognised Metazoa as divisible into six types—*Cœlenterata*, *Echinodermata*, *Vermes*, *Arthropoda*, *Mollusca* and *Vertebrata*—and thus initiated a system which, in its main features, is still maintained at the present day, and must be recognised as a stroke of genius in a young man of some twenty-five summers, working at such an early stage in the history of morphological science.

In 1855 he was made ordinary professor, and in 1870 removed to Leipzig. As a teacher he was clear and stimulating, and his remarkable success in this department of scientific work is attested by the volume issued in commemoration of his seventieth birthday, in which about 139 men of science, including many of the most eminent zoologists of the day, are proud to acknowledge themselves his pupils.

As an investigator he fully realised the promise of his early youth. His knowledge was as accurate as it was extensive, and that to a degree which only becomes comprehensible when we remember that unaided he contributed for nearly forty years a masterly summary of current researches into the natural history of the lower animals to the pages of the *Archiv für Naturgeschichte.* It is clearly impossible to give anything like a detailed account of such an active and many-sided career in a moderate space: let it suffice to recall his insistence on the division of labour in the animal kingdom, his researches on the reproduction of bees and of the Cephalopoda, his recognition of the ciliated organ of Heteropoda and Pteropoda as an osphradium, and his reference of *Neomenia* to the Mollusca.

Undoubtedly, however, his greatest energy was devoted to the study of parasitic life in general and to the life-history of the parasitic worms in particular. He at once recognised the importance of the methods of experimental helminthology introduced by Küchenmeister, and demonstrated the life-history of nearly all the bladder-worms then known by rearing them in suitable hosts. He was the author of epoch-making researches on *Trichina* and on the *Pentastomida*, and contemporaneously with the Englishman, A. P. Thomas, worked out the life-history of the Liverfluke. His work on the "Parasites of Man," the first volume of which has been translated into English, is a perfect cyclopædia of information derived from the writings of others and from his own observations. He has passed away full of years and full of honours, leaving a name which will ever be venerated by zoologists of every tongue and nation.

## *NOTES.*

The first soirée of the Royal Society, to which gentlemen only are invited, is fixed for Wednesday, May 11.

On Saturday last (April 2) the Council of University College, London, elected Prof. H. L. Callendar, F.R.S., to the Quain Professorship of Physics, about to become vacant by the resignation of Prof. G. Carey Foster, who in a few months will have held his Professorship in University College for thirty-three years. Prof. Callendar, who has been Professor of Physics in McGill College, Montreal, will enter upon his duties in London in October next.

Sir William Turner, F.R.S., professor of anatomy in the University of Edinburgh, has been elected a corresponding member of the Berlin Academy of Sciences. He has also been elected president of the General Medical Council, in succession to the late Sir Richard Quain.

Prof. H. C. Bumpus has been appointed director of the laboratory of the United States Fish Commission Station at Wood's Holl.

Sir Samuel Wilks has been re-elected president of the Royal College of Physicians of London.

M. Richet has been elected a member of the Paris Academy of Medicine.

A "Jardin de Kew" is to be established in the neighbourhood of Nantes by a rich citizen of that town. The new botanical garden will be planned on the same lines as the Royal Gardens at Kew, and special attention will be given to the cultivation of plants useful in French colonies. It is hoped that the garden will eventually do for French colonial possessions what Kew does for British colonies.

The Paris correspondent of the *British Medical Journal* announces that a recent decree authorises the University of Paris to borrow 68,000*l.* for the purpose of building laboratories where physical science, chemistry, and natural history will be taught for the benefit of students who are preparing for the examination for Science Certificate. Part of the money is to be applied to the completion of the Laboratory of Vegetable Biology belonging to the University of Paris at Fontainebleau.

The policy exemplified by the following appointment, announced in *Science*, might be adopted with advantage in this country:—Dr. Charles Wardell Stiles, of the United States Department of Agriculture, has been appointed *attaché* to the United States Embassy in Berlin. Dr. Stiles's duty will be to keep the Agricultural Department informed on important discoveries and other matters of interest to agricultural science, to defend American meats, fruits and other exports against unjust discrimination, and to advise the Secretary of Agriculture from time to time concerning the purity of the food products that are shipped from Germany to the United States. It is said that the appointment of Dr. Stiles will probably be followed by other similar appointments, and it consequently represents an important advance in the application of scientific principles to diplomatic and commercial affairs.

A LETTER received a few days ago by Prof. Milne from Mr. H. Hamilton, Montserrat, West Indies, contains somewhat startling information. It appears that since the flood of November 29, 1896, which caused great injury to life and property in Montserrat, innumerable earthquake shocks have been experienced. There are several craters and sulphur springs in the island, and it is thought that the mouth of one of the numerous craters was filled up by a landslip caused by the flood referred to, for several shocks of earthquake—the first experienced for a great number of years—were felt on the night of the flood. It is suggested that the filling up of this crater has been the cause of all the earthquakes which have lately occurred in the island. But whatever may be the cause, there is no doubt that since November 1896, the island has been in a very disturbed seismological condition. Scarcely a day passes without a few shocks being felt, and as many as thirty distinct disturbances have often been experienced in one day. On February 15, 18 and 20 of this year, alarming shocks were felt; and it is affirmed that the worst shock on February 15 (11.16 a.m.) was just as severe as the great earthquake of 1843, but being of shorter duration it did not do so much damage. Several buildings have, however, been very badly damaged by the constantly-occurring disturbances, and innumerable cracks have appeared in nearly every stone building in the island. These earthquakes, says Mr. Hamilton, which have been continually felt since November 1896, are causing great anxiety among the inhabitants, and it is feared that the shocks will culminate in a volcanic eruption, or that the numerous stone buildings, weakened as they already are by the continual shocks, must in course of time be thrown to the ground unless the disturbances cease. The whole subject demands scientific inquiry, and it is to be hoped, both in the interests of science and of the people of Montserrat, that the Colonial Office, which has probably received official reports of the earthquakes, will send some one to the island to investigate them.

THE numerous cases of enteric fever which have been traced to the consumption of contaminated oysters, clearly points to the need of a change in the present condition of the law relating to the culture of oysters and other shell-fish. For the purpose of submitting a memorial in favour of an alteration of this law, a deputation from the corporations of twenty-five provincial towns, and the London County Council, waited upon the President of the Local Government Board a few days ago. As the law now stands, local authorities have no means of preventing the sale of shell-fish within their districts, even though they possess the clearest evidence that the consumption of the shell-fish has produced typhoid fever, and that the shell-fish is derived from a source known to be contaminated with sewage. In reply to the deputation, Mr. Chaplin said that he considered that the time had arrived for legislative action, and he had been engaged for some time on the measures necessary and appropriate to deal with the matter. As to the dangers which might arise from the sale of infected shell-fish other than oysters, he had not sufficient information to act upon, but with regard to oysters he hoped it would be possible for him soon to take action which would be satisfactory to the deputation.

A HOLIDAY course of science lectures and demonstrations will be held in Berlin from Wednesday, April 13, to Saturday, April 23. Lectures on most branches of science have been arranged, and visits will be made to museums and other places of scientific interest.

A MEETING of the Institution of Mechanical Engineers will be held on Wednesday and Friday, April 27 and 29. The chair will be taken by the President, Mr. Samuel W. Johnson, who will deliver his inaugural address at the opening meeting. The following papers will be read and discussed, as far as time permits:—"First Report to the Gas-Engine Research Committee: description of apparatus and methods, and preliminary results," by Prof. F. W. Burstall; "Steam Laundry Machinery," by Mr. Sidney Tebbutt.

ATTENTION has already been drawn in NATURE to the publication by the Geological Survey of a colour-printed map of the London area and great part of the Weald. This was Sheet 12 of the General Map on the scale of an inch to four miles. We are now able to state that all the fifteen sheets of this map have similarly been issued in the colour-printed form, at a uniform price of 2*s.* 6*d.*, with the exception of the title-sheet, the price of which is 2*s.* The total cost of the map, which if mounted would measure about 8 by 6 feet, is 1*l.* 17*s.*

A CORRESPONDENT from Bangor writes:—"An instance of a locally acquired habit in birds, on which it would be interesting to collect information from different districts, is afforded by the behaviour of sparrows towards the flowers of garden crocuses. Here in Bangor we have had crocuses blossoming two years in succession without a single flower being eaten off; in gardens at Cambridge, and other places, every flower is pulled to pieces almost before it has fully opened. It would seem that the flowers contain some agreeable flavouring matter which the Bangor birds have (fortunately) not yet learnt to appreciate."

MR. G. MARSHALL WOODROW, Professor of Botany at the Royal College of Science, Poona, went to Jeur at the time of the recent total solar eclipse, and made some botanical observations which he communicates to the *Gardener's Chronicle* (March 19). This station was not very suitable for luxurious vegetation, as the daily range of temperature during January was too great, the thermometer ranging from 45° F. to 145° F. and in the shade from 50° F. to 90° F. He, however, collected 130 species, including 26 Gramineæ, 27 Leguminosæ, 14 Compositæ, 9 Acanthaceæ, 5 Asclepiadaceæ, 5 Euphorbiaceæ, 5 Malvaceæ, 5 Cucurbitaceæ, 5 Convolvulaceæ, 2 Solanaceæ, 4 Labiatæ, 2 Urticaceæ, and 2 Capparidaceæ. Of the Gramineæ he mentions that the most frequent one, *Aristida setacea*, was in ripe seed, and it was interesting to observe its manner of distributing them. Its three-branched awns "twist together in such a manner that a perfect sphere is formed by their extended points, and the balls roll hither and thither in every breeze." Another grass of interest is the species Isachne, which has the habit of setting loose its entire inflorescence, a large open panicle of most elegant form, which is rolled about by the wind till it is caught in some bush. This species has an inflorescence larger than any other known; and since it was found while preparing to observe the eclipse, the name *Isachne obscurans* is proposed for it. Prof. Woodrow mentions that as the sunlight began to fade away, owing to the passage of the dark moon, Leguminosæ began to fold up their leaves, as is their manner at evening time.

MR. H. C. RUSSELL, Government Astronomer of New South Wales, has communicated a second paper to the Royal Society of that Colony, on the subject of icebergs in the Southern Ocean, from reports collected from masters of vessels trading to Sydney and from other sources. The first paper dealt with the icebergs in the South Atlantic which had been reported up to July 1895, and the present paper continues the discussion down to September 1897, during which time the great mass of the bergs has drifted from the South Atlantic to between longitude 40° and 80° in the South Indian Ocean, and have been subsequently reported south-eastward of New Zealand. It is somewhat remarkable that for months at a time very few icebergs were met with by vessels trading to Australia, and their motion into and out of the tracks of vessels made it seem probable that it was affected by the prevalent winds. A reference to the weather charts showed that when there was a prevalence of north-west wind no ice was reported, while with southerly winds bergs were frequently observed. Mr. Russell states that the records are too short to settle the question, but he is of opinion that by careful study of the winds in connection with the movements of the bergs it will be possible to forecast their positions from the winds prevailing in South Africa and Australia.

THE Pilot Chart of the North Pacific Ocean for the month of March, published by the Hydrographer of the United States Navy, contains tables and charts giving the mean temperatures of the surface waters for each quarter and for the year for that part of the North Pacific Ocean comprised between latitude 30° and 60° N., and the west coast of North America and longitude 180° W. The material has been obtained from observations in the possession of the United States Hydrographic Office, supplemented by the data contained in the Russian Admiral Makaroff's work, "The *Vitiaz* and the Pacific Ocean." The coldest region is in 55°–60° N., and 155°–180° W., having for the months of May to September a mean temperature of 43°. In the same longitude, and latitude 50°–55° N., the mean annual temperature is 42°. The warmest region is in latitude 30°–35° N., longitude 140°–165° W., having a mean annual temperature of 68°. The yearly range of monthly temperature is highest in latitude 35°–40° N., longitude 150°–180° W., being 18°·5, and lowest 8°·5, in latitude 30°–35° N., longitude 115°–145° W.

THE report of Mr. S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1897, has just reached this country. Following the custom of several years, Mr. Langley gives in the body of the report a general account of the affairs of the Institution and its bureaus—the U.S. National Museum, the Bureau of American Ethnology, the International Exchanges, the National Zoological Park, and the Astrophysical Observatory—while more detailed statements by the officers in direct charge of the various branches of the work are given in an appendix. We regret to see, in the report on the work of the National Museum, that the complete manuscript of an important and comprehensive work by the late Prof. Cope on the reptiles of North America, based on the museum collections, is withheld from the printer for want of funds for its publication, and at least four others, equally valuable and extensive, now in an advanced stage of preparation. Delay in the publication of these works will prove a hindrance to the progress of American natural history. The Bureau of American Ethnology has been very active. The field operations have been extended into a large number of states and territories, and incidentally into those districts of neighbouring countries occupied by native tribes closely affiliated with the aborigines of the territory now comprised in the United States. During the year covered by the report special attention was given to the classification of the tribes in such manner as to indicate their origin and development, and to this end the rich archives of the Bureau, comprising the accumulations of eighteen years of research, have been subjected to careful study, and important conclusions have been reached. The International Exchange Service continues to increase; and the fact that exchanges are now made with 28,000 correspondents in every part of the civilised world demonstrates, to some degree, the far-reaching influence of the Institution. The National Zoological Park has been improved by the construction of roads and a new bridge; but the buildings and enclosures of the Park are altogether inadequate, and there are no funds to supply the wants. Among the needs are suitable houses for the preservation and care of birds, a vivarium for small animals, and ponds for aquatic birds and mammals. The operations of the Astrophysical Observatory have consisted chiefly in experiments in the bolographic analysis of the infra red solar spectrum. The report upon this work has been completed; and it contains, in addition to introductory, historical, descriptive, and theoretical matter and accounts of subsidiary investigations, tables of positions of 222 absorption lines in the infra-red solar spectrum in terms of angular deviations and refractive indices for a rock-salt prism, and of the approximate wave-lengths corresponding. It is to be hoped that this report, containing results of great interest and value to physical science, will soon be published.

THE election of Prof. James E. Keeler, director of the Allegheny Observatory, to the directorship of the Lick Observatory was announced in last week's NATURE. We now learn that Prof. Keeler has written a letter to the Chairman of the Allegheny Observatory Committee stating that he is prepared to decline the call to the Lick Observatory if within two weeks 200,000 dollars can be collected for the erection of a new observatory with a thirty-inch telescope, and towards the endowment of a chair of astronomy in the Western University of Pennsylvania. Efforts are being made to obtain this sum of money, and as much as 137,000 dollars has already been subscribed, while Allegheny City has given a site for a new observatory in an elevated position surrounded by parks, and comparatively free from smoke. We wonder how many British cities and citizens would show in such a substantial way their anxiety to keep a distinguished scientific investigator within their borders.

AN interesting observation upon the development of a taste for honey by starlings is recorded by Mr. W. W. Smith in the *Entomologist* (April). In a previous note referring to some enemies of humble-bees in New Zealand, Mr. Smith stated that he had observed the newly-introduced starlings killing and conveying humble-bees to their nests to feed their young. The tui or parson-bird (*Prosthemadera novæ-zealandiæ*) has now been detected killing them at Akaroa on Banks Peninsula. The case is remarkable in illustrating how new habits are acquired, or family habits are developed in some species of birds when certain conditions are present. As the tui belongs to the starling family, and is one of the native honey-suckers, it is possible it also was killing humble-bees to feed its young when it discovered the honey-sac of the insects. The tui, while engaged in killing the bees, would discover their honey-sac, which would also lead to a continuance of the habit as a ready means of procuring their favourite food. An analogous case is also presented in some recently acquired habits of the starling. For two seasons Mr. Smith has observed what is undoubtedly an acquired taste and habit in the starling in New Zealand. Like the tui, this bird now frequents the flax-flats and sucks the honey from the richly mellifluous flowers. It appears probable that the eating of the humble-bee's honey-sac by the starlings developed, or is now developing, the taste for honey in these birds.

FROM the many papers before us dealing with kathodic rays, Röntgen rays, and the closely-allied phenomena of "electro-dispersion," we extract the following:—Prof. Battelli and Dr. Garbasso (*Nuovo Cimento*, vi. 4) examined more closely the action of kathodic rays on insulated conductors, with the view of testing the existence of indeformable rays in the interior of the Crookes' tube. Their results agree with the hypothesis that the different modes of action of kathodic and Röntgen rays depend on the different conditions of the medium in which the conductor is placed.—M. P. de Heen (*Bulletin de l'Académie Royale de Belgique*, 1898, pp. 188, 191) publishes two papers relating to the electro-dispersive power of Röntgenised air, and also of air modified by a Bunsen burner. In the first paper the author obtains, by the Bunsen burner, results which cannot be accounted for on Villari's theory of Röntgenised air, but indicate the existence of a special kind of energy, to which he applies the name *infra-electric*. In the second paper he describes four experiments dealing with the propagation of what he calls *anti-electric* energy behind shadows. The papers leave us a little uncertain as to M. de Heen's distinction between the terms *infra-electric* and *anti-electric;* "anti"-electricity, we are told, includes both "infra"- and "ultra"-electricity, but the latter we do not see defined, at all events, in these two writings.

FROM Mr. A. A. Campbell Swinton we have received a reprint of his paper on adjustable X-ray tubes, read before the Röntgen Society.—Part iv. (vol. vii.) of the *Atti dei Lincei* contains two papers, one on the cryptoluminescence of metals, by Prof. A. Róiti; the other on the diffusion of Röntgen rays, by Drs. R. Malagoli and C. Bonacini. According to the two latter writers, (1) the two electrodes contemporaneously emit *ortho-kathodic* rays, but that which communicates with the negative pole of the excitor develops them the most intensely; (2) from the electrodes, at a certain stage of rarefaction, there seem to start two cones of radiation, one enclosed in the other or partially separate, carrying opposite charges; both are displaced by magnets subject to the same laws; (3) the violet anodic light, like the ortho-kathodic rays, is intensely affected by magnetic action, but it follows the opposite law, behaving like an electric current from the anode to the anti-anode; (4) it seems to follow that the anti-anodic system of Maltèzos is, perhaps, only a feeble anti-kathodic system, for between the two systems there is identity rather than mere analogy, and that not only in their effect on the glass.—The *Bulletin de la Société Française de Physique* (Nos. 108, 111) contains abstracts of two papers by M. Villard, the first dealing principally with the rays which produce the hemispherical illumination of "focus" tubes above the plane of the anti-kathode; the second dealing with the laws of variation of the resistance of a Crookes' tube, the electric attraction and repulsion of the seat of emission, the production of Goldstein's rays, and the nature of the kathodic rays. From their action in reducing crystals, silicates, oxides of copper, and other substances, M. Villard suggests that the kathodic rays are formed of molecules of hydrogen due to the traces of moisture left in the tube.—Dr. Josef R. v. Geitler, of Prague, contributes to the *Wiener Berichte* a paper on electric and magnetic decomposition of kathodic rays. The subject has been somewhat foreshadowed by Birkeland, and the investigation bears close analogy to one published by Prof. J. J. Thomson in October 1897. Like him the author, experimenting on the shadow of a wire placed in the kathodic pencil, has obtained a broadening out of the shadow, which appears bordered by a series of green fluorescent striæ separated by dark interspaces. Dr. Geitler claims, however, that his experiments are in many respects essentially distinct from Prof. Thomson's.

BY the death recently reported from Allahabad of Sir Saiyid Ahmad Khan, Indian Moslems have lost a leader who devoted many years, with great success, to their educational welfare and to the extension of scientific knowledge. He may well be described as the apostle of education to the Mahomedans of India. His institute at Aligarh, with its own printing-press and journal, his Anglo-Oriental college at the same place, on the model of a college of Oxford or Cambridge, for the education of Mahomedans of the upper classes, are splendid monuments to his breadth of mind, his wisdom, and his energy. The following particulars of Sir Saiyid Ahmad's career are from an article in Wednesday's *Times*:—This great leader of his people and pillar of British rule, as he has been called, was born in Delhi in 1817. His ancestors, who claimed descent from the Prophet, are said to have originally come from the Herat Valley, and for several generations held high office in the Court of the Moghul Emperors of Delhi. In 1837, after his father's death, the young man entered the British service in the Court of the Judge at Delhi, and from that time until he finally retired from the service he remained in the judicial branch. It was immediately after the Mutiny also that he threw himself heart and soul into the cause of Mahomedan education, and one of his earliest steps was to establish a translation society which should prepare suitable books, the want of which he greatly felt. A few years later this useful association expanded into the Scientific Society of Aligarh, with its own press, from which translations of numerous works on history and various modern sciences have been issued for the use of Mahomedans. It was after a visit to England in 1870 that he bent his mind to the great undertaking of the Anglo-Oriental College at Aligarh, which was opened in 1873 by Sir William Muir, while the foundation-stone of the building now in existence was laid with much ceremony by Lord Lytton in 1877. Having retired from the service in 1876, Saiyid Ahmad was in 1878 appointed a member of the Viceroy's Council by Lord Lytton, the appointment being renewed for a further period by Lord Ripon. He has also been on the Legislative Council of the North-West Provinces. In 1888 he was made K.C.S.I. For many years past Sir Saiyid Ahmad's home at Aligarh has been the goal of the pilgrimages of many of the greatest personages in India, and his reception by his fellow-Mahomedans when he has gone to the Punjab or to Haidarabad has been semi-regal. His last years were wholly devoted to the prosperity of his college and institute, and most of his journeys have been made on their behalf. Anglo-Indians who knew him best are as enthusiastic in his praise as the Indian Mahomedans. To the end he never changed the main article of his social faith—that education was the one indispensable requirement of Indian Mahomedans if they were to maintain under the British Raj the high position which was their due.

AN interesting address, by Prof. Thomas Gray, on the development of electrical science, in which the history of electrical progress since the beginning of the seventeenth century is traced, is published in *Science* of March 18 and 25.

HERR FREIHERR V. RICHTHOFEN, President of the Berlin *Gesellschaft für Erdkünde*, contributes a note to the *Verhandlungen* on the spelling of Chinese names. With the ordinary German pronunciation, *Kiautschou* represents the Chinese name more correctly than *Kiaotschau*, *Tschifu* than *Chefoo*, *Niutschwang* than *Newchwang*, *Futschoufu* than *Foochowfoo*.

THE whole of the first number of the new volume of the *Mittheilungen von Forschungsreisenden und Gelehrten aus den deutschen Schutzgebieten* is taken up with an exhaustive account of the drum language of the Duala, by Herr R. Betz. This method of conversation at a distance reaches a higher development in the Duala region than in any other part of the Kameruns. The paper contains no fewer than 275 examples of signs and phrases.

FROM an advance proof of the tables relating to the output of coal and other minerals in 1897, published by the Home Office, we learn that 202,119,196 tons of coal were mined in the United Kingdom last year. This was an excess of nearly seven million tons over the output for 1896. Next to coal, the largest outputs were:—ironstone, 7,793,168 tons; fireclay, 2,682,472 tons; oil shale, 2,223,757 tons.

PROF. DR. J. WALTHER contributes a further instalment of his studies of deserts to the *Verhandlungen* of the *Gesellschaft für Erdkünde zu Berlin*. Prof. Walther made an expedition into the waste regions of Transcaspia and Bokhara after the Geological Congress at St. Petersburg last year, and describes his observations on the erosive action of wind, of great ranges of temperature, and of the saline deposits in dried-up lakes. The paper forms an important addition to the author's geological work in similar regions of North America and North Africa.

THE "Statesman's Year-Book," edited by Dr. J. Scott Keltie, with the assistance of Mr. I. P. A. Renwick, annually improves in character and increases in usefulness. The volume just published by Messrs. Macmillan and Co. is the thirty-fifth; and it contains in the 1166 pages the latest statistical and other data referring to all the States of the world. The special features this year are maps showing, by means of different colours, the distribution of British commerce throughout the world, a map illustrating the Niger question, and a series of coloured diagrams exhibiting the course of trade in leading countries during the past twenty-five years. Trustworthy information upon all questions of political and commercial geography can be obtained from the volume, which keeps its place as the most handy and complete annual of geographical statistics in existence.

THE additions to the Zoological Society's Gardens during the past week include a Molucca Deer (*Cervus moluccensis*, ♂) from the Molucca Islands, presented by H.G. the Duke of Bedford; a Great-billed Touracou (*Turacus macrorhynchus*) from West Africa, presented by Mr. R. J. Nicholas; two Cambayan Turtle Doves (*Turtur senegalensis*) from West Africa, presented by Sir Edward Burne-Jones; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Captain Francis W. Bate; two Arctic Foxes (*Canis lagopus*) from the Arctic Regions, four Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; a Caucasian Wild Goat (*Capra caucasica*, ♂, juv.) from the Caucasus, received in exchange; a Burchell's Zebra (*Equus burchelli*, ♀), born in the Gardens.

## *OUR ASTRONOMICAL COLUMN.*

SPECTRUM ANALYSIS OF METEORITES.—A research of great interest has been undertaken by Messrs. W. N. Hartley and Hugh Ramage on the wide dissemination of the rarer elements and the mode of their association in the more common ores and minerals. The outcome of this work has led us to believe that the rarer metals are more widely distributed than was ever dreamt of, the authors showing that out of ninety-one iron ores obtained from the Dublin Royal College of Science, thirty-five contained the extremely rare metal gallium, while most of them contained constituents of an unusual character. Thus rubidium was commonly present: the magnetites invariably contained gallium, but no indium; the siderites all contained indium, but lacked gallium. In a more recent research they have investigated spectroscopically numerous meteoric ores, siderolites and meteorites (*Scientific Proc. of the R. Dublin Soc.*, vol. viii. (N.S.) Part vi., No. 68), the range of spectrum being between the wave-lengths 6000 and 3200, and the results they obtained in this case, arranged in tabular form, are of great interest. It is shown that the composition of different meteoric irons is very similar, though the proportions of constituents differ somewhat. Meteoric irons, different varieties of iron ores, and manufactured irons contain copper, lead, and silver. Gallium is a constituent of meteoric irons, but not of all meteorites, and occurs in varying proportions. Sodium potassium and rubidium are constituents of meteoric irons, but only in very small proportions. Meteoric stones, but not the irons, contain chromium and manganese. Nickel was found to be a principal constituent in all meteorites, meteoric irons, and siderolites, cobalt occurring in the two last varieties. The authors describe the chief points of difference between telluric and meteoric iron to be the absence of nickel and cobalt in any considerable proportion from the former, and the presence of manganese. Meteoric irons, on the other hand, contain nickel and cobalt as notable constituents, and, except in minute traces, manganese is absent. In referring to the photographic spectra of iron meteorites obtained by Sir Norman Lockyer from the Nejed and Obernkirchen meteorites, the authors point out that of the two lines, one described as "unknown," and the other as "doubtfully ascribed to iron," the former is certainly, and the latter probably, a gallium line. At the conclusion of their paper the authors give three plates, which reproduce the flame spectra of six metallic irons and three siderolites with comparison spectra.

STELLAR PARALLAXES.—Dr. Bruno Peter, during the years 1887 to 1892, made a series of parallax observations with the Leipzig heliometer. The results of this investigation have been published in vol. xxii. No. 4, and xxiv. No. 3, of the *Abhandlungen der Math.-Phys. Classe der K.S. Gesel. der Wissenschaften*; but Dr. Peter makes a short abstract in the *Astronomische Nachrichten*, No. 3483, which we briefly refer to here. In the following table, which brings together these results very clearly, $\epsilon$ represents the mean error of the parallax, and $\epsilon'$ that for one evening. In the three references to the star Lal 18115, (1) relates to the preceding component, and (2) to the following one, while (3) deals with the pair as a whole. The last column gives the comparison stars employed in each case.

| Star. | | Proper motion. | Parallax. | $\epsilon$ | No. of obs. | $\epsilon'$ | Comparison stars. | |
|---|---|---|---|---|---|---|---|---|
| | m. | " | " | " | | " | ° | ° |
| η Cassiopeiæ ... | 4 | 1·20 | +0·18 | 0 0,0 | 45 | 0·15 | +57·112 | +57·172 |
| μ ,, ... | 5·5 | 3·74 | ·13 | ·037 | 23 | ·16 | 53·207 | 54·241 |
| Lal 15290 ... ... | 8·5 | 1·97 | ·02 | ·043 | 32 | ·16 | 31·1648 | 30·1620 |
| Lal 18115 (1) ... | 8·0 | | ·18 | ·027 | 22 | ·11 | } | |
| ,, (2) ... | 8·0 | | ·18 | ·032 | 21 | ·12 | } 53·1309 | 53·1330 |
| ,, (3) ... | | 1·69 | ·18 | ·020 | 43 | ·11 | } | |
| δ Ursa Maj. ... | 3 | 1·11 | ·09 | ·035 | 22 | ·14 | 52·1389 | 51·1536 |
| A-Oe. 10603 ... | 6·5 | 1·45 | ·17 | ·013 | 27 | ·12 | 50·1707 | 49·1946 |
| β Comæ ... ... | 4 | 1·20 | ·11 | ·042 | 42 | ·18 | 28·2207 | 28·2184 |
| 31 Aquilæ ... | 5·5 | 0·96 | ·06 | ·015 | 40 | ·16 | 11·3802 | 12·3929 |
| Bradley 3077 ... | 6 | 2·08 | ·13 | ·012 | 39 | ·14 | 56·2956 | 56·2978 |

## *JAMES WATT, AND THE DISCOVERY OF THE COMPOSITION OF WATER.*[1]

WHEN your Secretary did me the honour to communicate the wish of the Committee that I should deliver this lecture, he was good enough to send me a list of the names of my predecessors in the position I was invited to occupy, together with a statement of the subjects on which they had addressed you. I confess I read his letter with very mingled feelings. To be asked to form one of such a distinguished company was in itself an honour which I deeply appreciated. On the other hand, it seemed well-nigh hopeless to find any theme associated with the life and work of the great man whose services to humanity we are this day called upon to commemorate, that had not been dealt with by one or other of those who preceded me. Naturally, and as befits the subject, the greater number of those who have spoken on these occasions have been distinguished engineers and mechanicians, and they have been able to speak with a fulness of knowledge, and a weight of authority, on the outcome of the great engineer's labours to which I, who know nothing of engineering or machinery, can have no pretensions.

It occurred to me, however, on reflection, that there was one incident in Watt's career, which, so far as I could learn, had not been handled by any one of those whom you have invited to appear here, and to which, as it comes within my own province, I thought I might venture, without presumption, to engage your attention. I was the more impelled to select it in that it illustrates one side of Watt's intellectual activity which those who regard him only as an inventor and a mechanician are apt to undervalue or lose sight of altogether. It serves, too, to throw additional light upon his mental character and moral worth, and thus enables us to form a fuller and more just appreciation of the attributes of the man we wish to honour. The incident, in a word, relates to Watt's share in the establishment of the true view of the chemical nature of water.

To the historian of science this is doubtless an old story, on which it would be difficult to say anything new. The literature concerned with it occupies many volumes, largely owing to the circumstance that it has given rise to a controversy which has engaged the active interest of some of the strongest and subtlest intellects of this century. Some of the disputants have been men like Brougham, Jeffrey and Muirhead, skilled in the arts of advocacy and in the faculty of eliciting and weighing evidence, who have stated their conclusions with all the "pomp and circumstance" of a judicial finding; others are men like Arago, Dumas, Harcourt, Whewell, Peacock, Kopp, George Wilson,

[1] The Watt Memorial Lecture, delivered in the Watt Memorial Hall, Greenock, on March 11, by Prof. T. E. Thorpe, LL.D., F.R.S.

eminent in science and literature, who have defended their convictions with great power, ample knowledge, much argumentative force, and occasional eloquence. At one time the contest was waged with no little fury and bitterness; it threatened, indeed, like the famous controversy on the proper form of a lightning-conductor during Sir John Pringle's presidency of the Royal Society, or like the equally famous controversy on the discovery of the planet Neptune, to attain the dignity of a national question, far more acute, I should imagine, than that which has just occasioned all right feeling Scotchmen to approach the Queen in Council on the subject of Scotland's proper place and designation in Imperial concerns.

But the acrimony and ill-feeling have happily long since passed away. There is no longer any need to discuss the question either as an advocate or as a partisan. What I shall attempt to-night is to treat it dispassionately, and, within the compass of an hour, to assess, as impartially as I am able, Watt's true place in regard to this discovery.

It was, indeed, an epoch-making event. The discovery of the composition of water was as momentous for science as the greatest of Watt's inventions was for social and economic progress. The very fact itself, apart from all that flowed from it, was of transcendent interest. But to those who had eyes to see, its supreme importance was in its fruitful and far-reaching consequences. It signified nothing less than the passing away of an old order of things, the downfall of a system of philosophy which had outlived its usefulness, in that it no longer served to interpret natural phenomena, but which was rather a hindrance and a stumbling-block to the perception of truth. The discovery at once led to the inception of a more rational and more truly comprehensive theory, which not only explained what was already known, in a fuller, clearer and more intelligible manner, but pointed the way to new facts hitherto undreamt of, which, in their turn, served to strengthen and extend the generalisation which led to their discovery. No wonder, then, that those who loved and revered Watt, and who were rightly jealous of his honour, should have sought to do all in their power to vindicate what they honestly conceived to be his just title to so signal and so fundamental a discovery.

No man has a juster claim to be regarded as a scientific man, in the truest and noblest sense of that term, than James Watt. The scientific spirit was manifest in him even in boyhood. The very circumstances of his condition, his weakly frame, the solitariness of his school-life, and the early habits of introspection thus induced in a mind forced to feed only on itself, served to strengthen and develop the instinct. Even his early struggles, and the jealousy of the Glasgow Guilds which forbade him to practise his trade in the burgh in which he had not served an apprenticeship, conduced to mould his character and to determine the bent of his mind. Hard and illiberal as it seemed at the time, the *Zunftgeist* which drove him to the shelter of the old College in the High Street, and secured for him the abiding friendship of Black and Robison, was in reality the most fortunate circumstance in his career. It brought him directly under the influence of one of the greatest natural philosophers of his age, and so stamped him permanently as a man of science. It would not be difficult to trace how this influence reacted upon all that Watt subsequently did—from the time of his earliest speculations on the loss of energy in Newcomen's engine down to the very last of his mechanical pursuits in the dignified retirement of Heathfield Hall. He approached the question of the improvement of the steam-engine as a scientific problem, and under the direct inspiration of the doctrine of the great discoverer of the principle of latent heat. It was this same mental attitude towards scientific truth, the same receptivity for scientific doctrine, the same love of pondering over and speculating upon the true inwardness of things that brought him the friendship of Priestley, Withering, Wedgwood and De Luc, and that ultimately made him a cherished member of the foremost scientific academies of the world. It will occasion little surprise to one who has formed a true perception of his character to learn that Watt was wont, even at periods of great mental depression, and of physical suffering, amidst all the toil and anxious worry of a business surrounded with difficulties, to find peace in the contemplation of natural phenomena, and to spend time in philosophical speculation. The shrinking, diffident man, in thus communing with himself and with nature, followed a true and constant impulse to withdraw from the strife and turmoil of the world, and to seek his pleasure and his rest in the silent contemplation of natural truth. No one can look upon that contemplative face without being struck with its expression of philosophic calm. What deep, genuine pleasure these communings brought to the harassed man may be gleaned from his correspondence. In truth, nature intended Watt to be a philosopher of the pattern of Boyle, or Newton, or Dalton; it was destiny that drove him into the world of affairs where, as he said, he was out of his sphere. It is necessary to dwell for a moment on this aspect of Watt, in order to form a just appreciation both of his position and of his merits in regard to the great chemical truth with which his name is associated.

The man of action is apt to regard the contemplative mind with something akin to contempt. I once heard a bustling, busy man, the head of a large engineering establishment, who had enjoyed the good fortune to be a pupil of Thomas Graham, say of that distinguished philosopher that he was the laziest man he had ever met. He did not say he "ever knew"—for how little he really knew of Graham was evident from the fact that at the period to which he referred Graham's thoughts were deeply occupied with some of the most memorable of his investigations.

It was in one of these contemplative moods—in what he himself styled his periods of excessive indolence—and as it happened at the very time that the Soho firm was struggling to protect itself against the unprincipled horde that was seeking to infringe Watt's fundamental patent, that he occupied himself with turning over in his mind the outcome of one of his friend Priestley's multitudinous experiments. Watt had long held the view that air was a modification of water, or, as he expressed it in a letter to his friend Black, under date December 13, 1782, that, "as steam parts with its latent heat as it acquires sensible heat, when it arrives at a certain point it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water": at which point he conceived it would again change its state and become air. As he then relates, he sees a confirmation of this opinion in an experiment of Priestley's made, as he says, "in his usual way of groping about." "As he [Priestley] had succeeded in turning the acids into air by heat only, he wanted to try what water would become in like circumstances. He undersaturated some very caustic lime with an ounce of water, and subjected it to a white heat in an earthen retort. . . . No water or moisture came over, but a quantity of air, equal in weight to the water . . . a very small part of which was fixed air, and the rest of the nature of atmospheric air. . . . He has repeated the experiment with the same result."

About a fortnight later Priestley wrote that he was able to convert water into air "without combining it with lime or anything else, with less than a boiling heat, in the greatest quantity, and with the least possible trouble or expense." He added that "the method will surprise more than the effect," but that he would defer "the communication of the hocus pocus of it" until such time as Watt should give him the pleasure of his company in return for the pleasure he was to give Watt in speculating on the subject.

These experiments, as we shall see in due course, were wholly fallacious; in following them up with his wonted ardour, Priestley quickly found himself in a maze of contradictions, and ultimately discovered that this seeming conversion was absolutely mythical.

It may be useful, however, to make one or two comments on these passages at the present juncture. In the first place Watt's opinion as to the relation of water and air, although founded, as he thought, upon a more philosophical basis, simply embodied the teaching of the schoolmen. The notion that the so-called four elements were mutually convertible, or were in essence identical, ran through the doctrine of twenty centuries of teachers. Despite the onslaughts of the Spagyrists, and the author of the "Sceptical Chymist," it permeated the literature of natural philosophy down to the very beginning of this epoch. Watt was insensibly swayed by a belief which had descended to him, like the undying germ, through the ages, and he could no more shake himself free of it than he could get rid of the influence of heredity. The very mode in which he, in common with men of his time, uses the term "air," is an indication of the manner in which the ancient creed limited and cramped his thought. He knew that there were various "airs," but it is very doubtful if he realised that they were essentially different substances. There is abundant evidence in the few chemical papers that he published, and especially in his letters to Black, Priestley, De Luc, Kirwan and others, that he regarded them all as constituted of the same matter, affected by attributes more or less fortuitous and accidental. Thus, all the varieties of inflammable air were at bottom identical, with properties modified by their origin or their varying content of the hypothetical principle phlogiston—that is the principle that was assumed to make them burn.

From Watt's published correspondence we are able to judge how he regarded Priestley's further work on this so-called conversion of water into air. He admits that the facts are "in some degree contradictory to each other." The apparent conversion would seem to depend upon the material of the vessel in which it was made. In a glass vessel no air was produced, nor was any found in a gun-barrel when the distillation was done slowly; but when confined by a cock, "and let out by puffs, it produces much air; which," says Watt, "agrees with my theory, and also coincides with what I have observed in steam-engines. In some cases I have seen the tenth of the bulk of the water, of air extricated or made from it." Davy once said "the human mind is governed not by what it knows, but by what it believes; not by what it is capable of attaining, but by what it desires." However willing to catch at anything in support of his belief, it is possible that Watt might have been led to doubt the soundness of Priestley's experiment, if an apparent and wholly unlooked for confirmation of it had not arisen.

To make the account exact, and in view of what is to follow, it is necessary to go back a little, in point of time. In the spring of 1781, Priestley performed what he styled "a mere random experiment made to entertain a few philosophical friends." It was practically a repetition of Volta's experiment of firing a mixture of the inflammable air from metals, that is, hydrogen, with common air in a closed glass vessel by means of the electric spark. After the deflagration the vessel was found to be hot, and on cooling its sides were observed to be bedewed. Neither Priestley nor any of his philosophical friends seem to have paid particular attention to the deposit of moisture, or, at all events, if they did they failed to perceive its significance. One of them, however, Mr. John Warltire, a lecturer in natural philosophy in Birmingham, imagined that the experiment might afford the means of showing whether heat was ponderable or not; and accordingly he repeated it, using for greater safety a copper globe, weighed before and after the passage of the spark. A minute loss of weight was always noticed, "but not constantly the same; upon the average it was about 2 grains."[1]

Priestley, who, with Withering, was present when the experiments were made, confirmed the apparent loss of weight; but he added, with a caution that was not characteristic, that he did not think "that so very bold an opinion as that of the latent heat of bodies contributing to their weight should be received without more experiments, and made upon a still larger scale."

Priestley's volume—the sixth in the series—was published in 1781, and was certainly known to Watt; indeed, in the Appendix are printed a number of observations made by him apparently as the work was passing through the press. Although, therefore, he must have had his attention drawn about this time to the formation of the dew in Priestley and Warltire's experiment, there is nothing to show that he attached any importance to the circumstance, or that, if he did, he dissented from Warltire's conclusion that common air deposits its moisture when it is phlogisticated.

For some time previous to the publication of Priestley's book, Mr. Cavendish was engaged upon an inquiry "to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." In other words, it was an investigation to determine the changes experienced by air when bodies were made to burn in confined portions of it. On the appearance of Priestley's book he repeated Warltire's experiment, thinking "it worth while to examine more closely, as it seemed likely to throw great light on the subject I had in view." He confirmed the observation on the formation of dew; but although he made the experiment on a larger scale, and with varying proportions of the two airs, he was unable to satisfy himself as to the loss of weight after the explosion. As the result of a number of trials, made both with the inflammable air from zinc and from iron—that is, hydrogen—and mixed with common air in the proportion of 423 measures of the inflammable air to 1000 of common air, he says, "we may safely conclude that when they are mixed in this proportion, and exploded, almost all the inflammable air and about one-fifth part of the common air lose their elasticity, and are condensed into the dew which lines the glass." In order to examine the nature of this dew, large quantities of the hydrogen were burnt with two and a half times its volume of common air, and the product of the combustion was caused to pass through a long glass tube whereby it was condensed. "By this means 135 grains of water were condensed in the cylinder [*i.e.* the tube], which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it leave any pungent smell during the evaporation; in short, it seemed pure water. . . . By the experiments with the globe, it appeared that when the inflammable and common air are exploded in a proper proportion, almost all the inflammable air and nearly one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

The idea that common air was for the most part a mixture of two gases—oxygen or the dephlogisticated air of Scheele and Priestley, and nitrogen or the mephitic air of Rutherford, the azote of Lavoisier—was familiar to chemists at this period as the result of the teaching of Scheele and Lavoisier, and there is reason to suppose that this opinion was shared by Cavendish. He had been engaged for some time past in an elaborate inquiry into the constitution of atmospheric air, the results of which admitted of no other interpretation than that common air was composed of two different gases, mixed or combined in constant relative proportions. It is true that in the memoir containing the results of his inquiry he nowhere directly gives his estimate of these relative quantities, but, from the data he affords, it is easy to deduce the amount and the constancy of the proportion. Cavendish's papers are characterised by remarkable conciseness and brevity; an experiment which must have involved the putting together of elaborate and complicated apparatus, and which must have occupied considerable time in its performance, is described in a few lines, and hence it is not always possible to gather with certainty the precise disposition of the arrangements. He never sets out his reasons or his conclusions with any great amount of detail, and his published words occasionally give little indication of his line of thought. But that he clearly recognised that only one portion of common air was concerned in the formation of water, and that this portion was the dephlogisticated air, or oxygen, is obvious from the next series of experiments in which he fired a mixture of about two measures of hydrogen and one measure of oxygen in a previously exhausted glass globe furnished with an apparatus for firing air by electricity. When the included air was fired, almost all of it lost its elasticity, so that fresh quantities of the explosive mixture could be introduced and the process repeated until a sufficient quantity of the moisture was obtained for examination. In these experiments Cavendish clearly and definitely demonstrated that the weight of the water was practically equal to the weight of the mixed gases which had combined to form it. In some cases the water was perfectly neutral in its reaction; in others it was slightly acid, and the cause of this acidity caused Cavendish much experimenting, but he is never in any doubt as to the main result; he says distinctly, "if those airs could be obtained perfectly pure, the whole would be condensed." Now if Cavendish had published this main result at the time he obtained it, namely in the summer of 1781, or even if he had formally communicated it to one of the meetings of the Royal Society during the ensuing session, there would have been no Water Controversy. But even if he were ready, it was characteristic of him to delay, not from inertia or indolence, but from a morbid shyness, an unconquerable reticence, which constantly led him to postpone any public announcement of his work. He had the additional, and to him all-sufficient, reason that he had not yet worked out the cause of the occasional acidity of the water. What he did, however, was to communicate the facts of his experiments to Priestley, as Priestley himself states in a subsequent paper published in the *Philosophical Transactions* for 1783. When or how he communicated them to Priestley does not appear, nor have we any means of knowing precisely what was said. Something, however, on this point may be inferred from what

[1] The account of these experiments is given in a letter to Priestley, and constitutes No. v. of the "Appendix to Priestley's Experiments and Observations relating to various branches of Natural Philosophy, &c.," vol. ii. (Birmingham, 1781).

Priestley proceeded to do. It appears from a letter to Wedgwood that he repeated Cavendish's experiment during the March of 1783. It will be remembered that he was at this period engaged on his experiments on the seeming conversion of water into air. He had obtained a number of contradictory results which had led Wedgwood, as far back as the previous January, to put certain sagacious queries, which doubtless in the end had their effect in opening Priestley's eyes to the origin of his mistake. But at the time both he and Watt were seeking for fresh evidence to substantiate the possibility of this conversion. Now just as Cavendish thought that Warltire's experiment might throw light upon the particular matter on which he was engaged, so Priestley considered that Cavendish's work might afford evidence, indirect it is true, but still evidence, of the intimate connection between water and air. Cavendish had, he thought, established the converse of the proposition which he and Watt were seeking to prove in showing that "air," or rather certain kinds of "air," could be converted into water weight for weight. It was no longer the original Warltire experiment of exploding common air and hydrogen. Cavendish had indicated the particular kinds which were really concerned in the phenomena, and it was the Cavendish experiment, pure and simple, which he proceeded to repeat. This is obvious from what he says: "Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the *reconversion* of air into water by *decomposing* it in conjunction with inflammable air." Priestley here used the word "decomposing" in a sense contrary to that which the context implies; but that he is consistent in so using it is evident from what follows, and also from similar expressions to be found in his correspondence. But although he professed to repeat Cavendish's experiment, he neglected to do so in Cavendish's manner. He says: "In order to be sure that the water I might *find in the air was really a constituent part of it*, and not what it might have imbibed after its formation [*i.e.* by contact with the water of the pneumatic trough], I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallisation was come over), and the latter from perfectly made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water.

"In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering-paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was reconverted into water, *and therefore that the origin of it had been water*."

These passages, when compared with the accounts given of his own work by Cavendish, strikingly exemplify the difference in the character of the two experimentalists. It would be difficult to pack a greater number of errors into a couple of paragraphs than are contained in these sentences. The expressions in italics show that Priestley wholly failed to comprehend the true origin of the water. In his laudable anxiety to free the two gases from extraneous moisture, he committed blunder after blunder. His method of obtaining the oxygen was bad; that of procuring the inflammable air was worse. Both the gases must have been highly impure, and it was a physical impossibility that they should have given their aggregate weight in water, even after making every allowance for Priestley's crude and imperfect method of determining it.

Bad, however, as the experimental work was, what it appeared to teach was not lost on Watt: it clearly proved to him that water and air were mutually convertible. How the theory took shape in his mind is evident from the terms in which the two series of Priestley's experiments are coupled together in his letters to Gilbert Hamilton, to De Luc and to Black. Each set is regarded as complementary to the other, and, both taken together, are held to prove that air and water are mutually convertible, and are therefore essentially the same. Under date April 21, 1783, he tells Black that "Dr. Priestley has made more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have put in the form of a letter to him, which will be read at the Royal Society with his paper on the subject." He then proceeds to give Black a summary of the three sets of facts, or supposed facts, on which he bases his generalisation, and he makes use of these significant words: "In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence—become red-hot—and on cooling, totally disappear. The only fixed matter which remains is *water*; and *water*, *light* and *heat* are all the products. Are we not, then, authorised to conclude that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat, and that dephlogisticated, or pure air, is composed of air deprived of its phlogiston, and united to heat and light; and if light be only a modification of heat or a component part of phlogiston, then pure air consists of water deprived of its phlogiston and its latent heat." Very similar turns of expression and trains of reasoning are to be met with in other letters to his friends, written at about the same period. In all it is abundantly clear that, whatever may have been his surmises as to the real nature of water, it was the conception of the mutual convertibility of air and water that was uppermost in his mind. These passages, however, constitute Watt's claim to be regarded as the true and first discoverer of the compound nature of water.

Three days after the letter to the Royal Society was written, or rather dated, there came a bolt from the blue in the form of a letter from Priestley to Watt. "Behold," it said, "with surprise and with indignation the figure of an apparatus that has utterly ruined your beautiful hypothesis, and has rendered some weeks of my labour in working, thinking, and writing almost useless." The doubts of Wedgwood, certainly no mean authority on the properties of baked clay, had, in fact, led Priestley to devise an experiment by which it was proved beyond all doubt that this seeming conversion of water into air was really due to an interchange of steam and air, effected by diffusion through the porous material of the retort. Well might Priestley cry to De Luc, "We are undone!" Watt's faith in the "beautiful hypothesis" was no doubt rudely shaken, but it was not shattered. In his answer to Priestley he denied that it was ruined: "It is not founded," said he, "on so brittle a basis as an earthen retort." Priestley, however, would have none of it: theories with him—always excepting the all-comprehensive one of phlogiston, which was the head and front of his creed, as, indeed, of his subsequent offending—had at no time much value, for, as Marat said of Lavoisier, he abandoned them as readily as he adopted them, changing his systems as he did his shoes. Indeed, he rather prided himself on his capacity for quick change. "We are, at all ages," he once said, "but too much in haste to *understand*, as we think, the appearances that present themselves to us. If we could content ourselves with the bare knowledge of new facts, and suspend our judgment with respect to their causes, till by their analogy we were led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge." With a candour all his own, he immediately added: "I do not pretend to be perfectly innocent in this respect myself; but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them. . . . I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as discoveries, and mere *deductions* from these facts, as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves."

Watt's mind was of a very different cast. He did not lightly adopt opinions; his convictions were slowly and deliberately formed, and were retained with a corresponding tenacity. But, all the same, he eventually thought it prudent to withdraw his letter; and three days prior to the reading of Priestley's paper, which accompanied it, Priestley informed Sir Joseph Banks of Watt's desire that the letter should not be publicly read. That it was withdrawn on account of what Watt calls Priestley's "ugly experiment," is stated by him in a letter to Black, on the ground that this experiment rendered "the theory useless in so far as relates to the change of water into air. . . . I have not given up my theory [that is, as to the mutual convertibility of water into air], though neither it nor any other known one will account for this experiment."

In the meantime Cavendish had been pursuing his inquiries, and towards the end of this year (1783) he was prepared to give the explanation of the cause of the disturbing factor in his proof of the real nature of water—that is, the origin of the occasional and apparently haphazard presence of small quantities of nitric acid. This he demonstrated to be due to the difficulty of excluding a greater or less quantity of atmospheric nitrogen from the gases employed; and he determined the conditions under which this nitrogen led to the formation of the acid, the true nature of which he thus for the first time established. The account of his labours was read to the Royal Society on January 15, 1784.

In the previous autumn, however, disquieting rumours reached this country that the French philosophers, and chief among them Lavoisier, were poaching upon the English preserves. The circumstance is alluded to in a letter from Watt to De Luc, dated November 30, 1783. "I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the spring, has run away with the idea and made up a memoir hastily, without any satisfactory proofs. . . . I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed; for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints from my letter, which may enable him to make experiments, and to improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance and sink it into utter oblivion; nay, worse, I may be condemned as a plagiary, for I certainly cannot be heard in opposition to an Academician and a financier. . . . But, after all, I may be doing Mr. Lavoisier injustice."

That Lavoisier did get some hints, and possibly even through the medium of Watt's letter, is beyond all question. The fact that he was informed of Cavendish's work is specifically stated in Cavendish's memoir in a passage interpolated by Blagden, the Secretary of the Royal Society and Cavendish's assistant and amanuensis, who himself told Lavoisier. The whole of the circumstances are set out in detail in a subsequent letter which Blagden addressed to the editor of the *Chemische Annalen* in 1786. That it was known to be Cavendish's experiment that was being thus repeated, is confirmed by a letter from La Place to De Luc, dated June 28, 1783, in which we read: "Nous avons répété, ces jours derniers Mr. Lavoisier et moi, devant Mr. Blagden, et plusieurs autres personnes, l'expérience de Mr. Cavendish sur la conversion en eau des airs dephlogistiqués et inflammables, par leur combustion. . . . Nous avons obtenu de cette manière plus de 2½ gros d'eau pure, ou au monis qui n'avoit aucun caractère d'acidité, et qui étoit insipide au goût; mais nous ne savons pas encore si cette quantité d'eau représente le poids des airs consumes; c'est une expérience à recommencer avec toutes l'attention possible et qui me paroit de la plus grande importance." The phrase "qui n'avoit aucun caractère d'acidité" is of special significance. The French philosophers, and Lavoisier in particular, could with difficulty, as Blagden relates, be brought to credit the statement that when inflammable air was burnt, water only was formed; their preconceptions concerning the part played by oxygen in such a case, led them to suppose that an acid would be produced. Cavendish was familiar with Lavoisier's doctrine, which is connoted in the very word oxygen, which we owe to the French chemists; and it may be that this circumstance was, amongst others, one cause of the pains he took to understand the origin of the acid he occasionally met with. Lavoisier was led to repeat Cavendish's experiment on June 24, 1783; and on the following day he announced to the Academy that by the combustion of inflammable air with oxygen "very pure water" was formed. It is this statement that has been said to constitute Lavoisier's claim to be considered as the true and first discoverer of the composition of water. That he has no valid claim has been implicitly admitted by Lavoisier himself. The eminent Perpetual Secretary of the French Academy, M. Berthelot, is no doubt accurate in regarding June 25, 1783, as the first certain date of publication of the discovery that can be established by authentic, *i.e.* official, documents; but, as I have elsewhere attempted to show, the circumstances under which that priority of publication was secured give Lavoisier no moral right to the title of the discoverer.[1]

Shortly after the reading of Cavendish's memoir to the Royal Society (January 15, 1784), De Luc wrote to Watt, giving an account of its contents, and insinuating that its conclusions had been formed in the light of knowledge obtained from Watt's letter to the Royal Society, which although, as we have seen, not publicly read, had, there is no doubt, been perused by others than Priestley, to whom it was originally addressed. De Luc was, no doubt, a zealous friend, but in this letter his zeal outran his discretion. The letter was, indeed, unworthy of him. He hastens to exculpate Lavoisier and La Place, but makes a charge against the honour and integrity of Cavendish, for which there was absolutely no justification. He stirs up Watt's suspicions, and then seeks to appease them; he rouses his anger, and then counsels him to silence by an argument which shows how wholly he misunderstood Watt. Watt's reply was characteristic: "On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine; which of the two is the right I cannot say: his is more likely to be so, as he has made many more experiments, and consequently has more facts to argue upon. . . .

"As to what you say of making myself *des jaloux*, that idea would weigh little; for were I convinced I had had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory: from a conviction in my own mind that I was their superior: or from an indolence, that makes it easier for me to bear wrongs than to seek redress. In point of interest, in so far as connected with money, that would be no bar; for though I am dependent on the favour of the public, I am not on Mr. C. and his friends; and could despise the united power of *the illustrious house of Cavendish*, as Mr. Fox calls them.

"You may, perhaps, be surprised to find so much pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right, and because I pay respect to the opinion of others, where I think they may merit it. At present *je me seus un peu blessé*; it seems hard that in the first attempt I have made to lay anything before the public, I should be thus anticipated."

There was no desire on the part of anybody connected with the management of the Royal Society to withhold from Watt his just due; and it was eventually arranged that his letter to Priestley, together with one he subsequently addressed to De Luc, should be publicly read to the Fellows, and they were subsequently ordered to be printed in the *Philosophical Transactions* in such manner as their author might desire. By his directions the two letters were merged together, and they appear as having been read on April 29, 1784, under the title, "Thoughts on the constituent parts of water, and of Dephlogisticated air: with an account of some experiments on that subject. In a letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S." The greater part of the "thoughts" are concerned with the dephlogisticated air. What relate to water have already been given in the extracts from his correspondence. The terms in the letter to De Luc, as printed in the *Philosophical Transactions*, are substantially identical with those of the letters to Black, Hamilton, Smeaton and Fry.

I have now given all the essential facts which led to the recognition of the true chemical nature of water, and I have stated, as accurately and as impartially as I could, the relative share of Watt, Cavendish and Lavoisier in their discovery and interpretation. As regards Lavoisier, it cannot be claimed that he was the first to obtain the facts. To Cavendish belongs the merit of having supplied the true experimental basis upon which accurate knowledge could alone be founded. Watt, on the other hand, although reasoning from imperfect and, indeed, altogether erroneous data, was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight, of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air. It would be a mistake, however, to suppose that Watt taught precisely the same doctrine of the true nature of water that we hold to-day. Nor did Cavendish utter a more certain sound. What we regard to-day as the expression of the truth we owe to Lavoisier, who stated it with a directness and a precision that ultimately swept all doubt and hesitation aside—except to the mind of Priestley, whose "random experiment" gave the first glimmer of the truth.

In this respect the conclusion of Lord Brougham is most just. It was a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply-

[1] Priestley, Cavendish, Lavoisier, and "La Révolution Chimique": the Presidential Address to the Chemical Section of the British Association, 1890; see also "Essays in Historical Chemistry" (Macmillan, 1894).

rooted opinion that prevented Watt and Cavendish from doing full justice to their own theory; while Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.

We thus see that each of these eminent men played an independent and, we may say, an equally important share in the establishment of one of the greatest scientific truths that the eighteenth century brought to light.

As regards Watt, the history of this incident serves to bring out only more clearly what we know to be the true character of the man. It illustrates the vigour of his intellectual grasp, the keenness of his mental vision. At the same time it exhibits his love of truth for truth's sake; his unaffected modesty, and the sense of humility that was not the less real because accompanied by a sense of what his inherent love of rectitude taught was due also to himself. The voice of envy and detraction has not been unheard amongst the strife of partisans in the Water Controversy, but throughout it no syllable has been breathed that reflected even remotely upon his honour and integrity.

## SCIENTIFIC SERIALS.

SEVERAL contributions of anthropological interest appear in the January and February issues of *Globus*.—An old Mexican terra-cotta figure in the American Museum of Natural History is described and figured. It was discovered near Texcoco, and represents a warrior in a padded coat of mail. The figure is of life-size, and its workmanship is peculiar to Mexican antiquities.—A description of the temple-pyramid of Tepoztlan, by Dr. E. Seler, contains not only interesting details, but several very good illustrations of the plan and construction of the temple. Tepoztlan is the place where the Mexican kings had their famous pleasure gardens, and the inhabitants have preserved their ancient language and many of their old customs in their mountain home. The temple lies 2000 feet above the town on a cliff. The ruins consist of several buildings of all kinds and sizes, which are suggested to have been the dwellings of the priests. The temple itself has massive walls built of black and red volcanic stone. The inner space is divided into two rooms by a door let in a thick wall. In the inner room was found a rectangular cavity containing coal and two pieces of copal, showing probably that here was the place where the holy fire was burnt. The door leading to the inner room is flanked by two pillars, richly carved, but the most interesting feature of the room is its benches of sculptured stone. In this room stands an idol, and there were found two pieces of sculpture: one a bas-relief painted in dark red, the other a relief of a Mexican king's crown. Altogether, this is a notable discovery; and if it is really the fact that these people have preserved their ancient culture, it is greatly to be hoped that a scientific exploration will be undertaken before it is too late.—Another people of South America is noted in a paper by Dr. Ehrenreich on the Guayaki in Paraguay. Their territory is bounded on the east and south by Parana, on the north by the rivers Acaray and Monday, and on the west by well-wooded hills. Very little is known about them, and only few ethnographical specimens have found their way into museums. The personal possessions of the people consist of a conical-shaped cap made out of a jaguar skin, chains made of pierced teeth and bones of animals, stone axes, bows and arrows, lances made out of the bark of the palm, and a sharp instrument made out of animal bones. Their vessels are particularly remarkable. Some are egg-shaped, and obviously intended to fix in the ground, and most of them belong to the so-called basket pottery. Several illustrations accompany the paper, including three photographs of a Guayaki man. He is very short, with strikingly short legs, long arms, broad shoulders, short neck and large head. They live entirely as huntsmen, without any tillage, and the very primitive character of the race suggests that they, and possibly other tribes on the boundary line of Brazil, would reveal much information of value to the anthropologist.—An account of the Moplahs of the coast of Malabar, by Dr. Emil Schmidt, is exceedingly useful. They are partly of Hindoo and partly of Arabian origin, and the mixture is shown in their customs. In the north the young husband settles in his wife's house, and the woman's right of succession is admitted; in the south, male succession is the rule. A careful study of these mixed peoples is much needed.—Dr. Nehring gives an account of the worship of the ringed snake among the old Lithuanians, Samoyitians and Prussians.—A paper by Mr. C. G. Hoffman, on the Niggers of Washington, contains some notes on the curious superstitious practices of the Voodoo, said to be a survival of the old religion.—Mr. Christian Jensen's paper on the grave mounds and giants' graves in the islands of North Friesland, contains information of special interest to English folk-lorists who have followed Mr. MacRitchie's ingenious explanation of some fairy beliefs.

## SOCIETIES AND ACADEMIES

### LONDON.

**Royal Society**, March 10.—"On the Relative Retardation between the components of a Stream of Light produced by the passage of the Stream through a Crystalline Plate cut in any direction with respect to the Faces of the Crystal." By James Walker.

If the surface of the plate be the plane of $xy$, the positive axis of $z$ being directed inwards, the relative retardation is $T(n_1 - n_2)$, where the velocity of light in air is unity, T is the thickness of the plate, and $n_1$, $n_2$ are the positive roots of a biquadratic in $n$ obtained by expressing that $lx + my + nz = 1$ is a tangent plane to the wave-surface. Writing the roots of the biquadratic as series proceeding by powers of sin $i$, and expressing the coefficients (which are linear functions of sin $i$) as symmetrical functions of the roots, the terms of the series may in general be determined in succession by means of linear equations, and have the form $\pm \alpha' + \gamma$, $\pm \alpha'' - \gamma$, where

$$\alpha = \alpha_0 + \alpha_1 \sin i + \alpha_2 \sin^2 i + a_4 \sin^4 i + \ldots,$$

and

$$\gamma = \gamma_3 \sin^3 i + \gamma_5 \sin^5 i + \ldots,$$

while the relative retardation is

$$T(\alpha' - \alpha'' + 2\gamma).$$

This method fails when the plate is perpendicular to an optic axis, in which case the biquadratic may be written

$$n^4 + (c_0 + c_2 \sin^2 i)n^2 + b_3 \sin^3 i \, n + a_0 + a_2 \sin^2 i + a_4 \sin^4 i = 0.$$

Neglecting the coefficient of $n$, the roots are

$$\pm(\pi + \rho), \pm(\pi - \rho),$$

$\pi$ and $\rho$ being series proceeding by even and odd powers of sin $i$ respectively. Assuming that the actual roots are

$$\pi + \rho + \alpha, - \pi - \rho + \beta, \ldots$$

the successive terms of the series $\alpha$, $\beta$, $\gamma$, $\delta$ are determined as in the former method, and, as for terms of the fourth order, have the form

$$\alpha = - \gamma = a_2 \sin^2 i + a_3 \sin^3 i + a_4 \sin^4 i,$$

$$\beta = - \delta = a_2 \sin^2 i - a_3 \sin^3 i + a_4 \sin^4 i,$$

so that

$$\Delta = 2T(\rho + \alpha).$$

**Geological Society**, March 23.—W. Whitaker, F.R.S., President, in the chair.—The Eocene deposits of Devon, by Clement Reid. A re-examination of the area around Bovey has led the author to think that Mr. Starkie Gardner is probably right in referring the supposed Miocene strata to the Bagshot period. Lithologically as well as botanically the deposits in Devon and Dorset agree closely. The gravelly deposits beneath the Bovey pipeclays are also shown to belong to the same period, and not to be of Cretaceous date. This correction has already been applied by Mr. H. B. Woodward to a large part of the area. The plateau gravels capping Haldon are also considered to belong to the Bagshot period, for they correspond closely with the Bagshot gravels of Dorset to the east, and of the Bovey Basin to the west, and possess peculiarities which distinguish them from any Pleistocene Drift. Several speakers took part in a discussion upon the paper, some agreeing with the author's views, and some were opposed to them.—On an outlier of Cenomanian and Turonian near Honiton, with a note on *Holaster altus*, Ag., by A. J. Jukes-Browne. Although an outlying patch of chalk in the parish of Widworthy was mentioned by Fitton and marked on De La Beche's map, it has not hitherto been described. The tract is about 4½ miles south-west of Membury, 3½ miles east of Honiton, and about 7 miles from the coast at Beer Head.—Cone-in-cone: additional facts from various countries, by W. S. Gresley. Examples of flinty stone in the "fire-clay series" of the Ashby coalfield exhibit "areas of conic structure lying unconformably." In the same stratum of shale are large masses of the same flinty rock, more or less coated with conic structures, which appear to have been formed out of layers of shale and ironstone. The bending-up of the shale above the nodules and down below them, the close but unconformable covering of Permian breccia, and the staining of the whole section suggests, if indeed it does not demonstrate, to the author that the growth of the cone-in-cone took place subsequently to the deposit of the Permian breccia. Several American and other examples are described, and a series of conclusions are appended to the paper.

Paris.

**Academy of Sciences**, March 28.—M. Wolf in the chair.—Preliminary study of a method of estimating carbon monoxide diluted with air, by M. Armand Gautier. It has been shown in previous papers on the same subject, that carbon monoxide is completely oxidised by passing over iodic anhydride at 60°-65°. The present study is concerned with the dilution at which this action ceases. Known volumes of carbon monoxide were mixed with large quantities of air, and the resulting mixture passed over iodic anhydride; the carbon dioxide product was measured by the method of Müntz. It was found that even at dilutions of 1 in 30,000, the quantity of CO present could be accurately determined. Both acetylene and ethylene are oxidised under the same conditions, but only partially, experiments showing that some 10 to 24 per cent. of the former, and 40 to 60 per cent. of the latter were converted into carbon dioxide.—On the use of palladium chloride as a reagent for the detection of minimal quantities of carbon monoxide in the air, and on the transformation of this gas into carbonic acid at the ordinary temperature, by MM. Potain and Drouin. One part of carbon monoxide in 10,000 of air can be detected by this reagent, if it be assumed that no other reducing gas is present, but the method does not yield quantitative results. Atmospheric air containing $\frac{1}{1000}$th part of carbonic oxide, after remaining in sealed flasks for forty-two days, showed no trace of the monoxide, but a nearly equal volume of carbon dioxide. From this it would appear that the monoxide can be slowly oxidised by air at ordinary temperatures.—Observations of Perrine's comet (1898 March 19) made at the Observatory of Paris, by MM. G. Bigourdan and G. Fayet.—Observations of the same comet, made with the large equatorial at the University of Bordeaux, by M. L. Picart.—Observations of Perrine's comet, made at the Toulouse Observatory with the Brunner equatorial, by M. F. Rossard.—Elements of Perrine's comet, by M. J. Lagarde.—Fundamental theorem on the birational transformations with complete coefficients, by M. S. Kantor.—On certain linear functional equations, by M. Lémeray.—Researches of precision on the infra-red dispersion of Iceland spar, by M. E. Carvallo. The measurements agree well with the results of earlier researches, but are accurate to another decimal place.—On the rigorous determination of molecular weights of gases, starting from their densities, and the deviations which they exhibit from Boyle's law, by M. Daniel Berthelot.—Gas engines with high compression, by M. A. Witz. A discussion of the theory of the Diesel engine.—On the Hertzian field, by M. Albert Turpain.—On an iodide of tungsten, by M. Ed. Defacqz. The hexachloride is first prepared by the action of chlorine upon the metal, and this heated to about 400° C. in a current of hydriodic acid. The iodide has the composition $WI_2$.—Quinolinic bases, by M. Marcel Delépine. Heats of combustion and formation of quinoline, tetrahydroquinoline, quinaldine, and tetrahydroquinaldine. Combination of organic bases with certain oxygen salts. Double salts are described of aniline and toluidine with cadmium, zinc, magnesium, nickel, cobalt, and copper sulphates.—New observations on the evolution of the *Urnes*, by MM. J. Kunstler and A. Gruvel.—On the encephalon of the Glyceræ, by M. Ch. Gravier. In spite of certain peculiarities which are related to the considerable length of the prostomium, the encephalon of the Glyceræ present the same fundamental characters as those of other allied Annelids of which the nervous system has been specially studied.—On the relation between centrosomes and vibratile cilia, by M. L. F. Henneguy.—On the structure of the mycorhizia, by M. Louis Mangin.—On the replacement of a principal stem by one of its ramifications, by M. Auguste Boirivant. When a lateral branch replaces a portion of a principal stem which has been destroyed, it undergoes modifications so profound as to finally more nearly resemble, both in its structure and external appearance, the axis which it replaces, rather than the branch to which it is homologous.—Biochemical preparation of crystallised dioxyacetone, by M. Gabriel Bertrand. By the action of the sorbose bacteria upon glycerine under suitable conditions laid down in this paper, excellent yields of crystallised dioxyacetone are obtained (25 gr. of the latter from 100 gr. of glycerine).—On the treatment of mania by the injection of normal nerve substance, by M. V. Babes.

## BOOKS, PAMPHLET, and SERIALS RECEIVED.

Books.—Outlines of Descriptive Psychology: Prof. G. T. Ladd (Longmans).—The Diseases of the Lungs: Dr. J. K. Fowler and Prof. R. J. Godlee (Longmans).—Nippur, or Explorations and Adventures on the Euphrates: Dr. J. P. Peters, 2 Vols. (Putnam).—Simple Lessons in Cookery: M. Harrison (Macmillan).—A Text-Book of Botany: Strasburger, Noll, Schenck, and Schimper, translated by Dr. H. C. Porter (Macmillan).—The Process of Creation discovered: J. Dunbar (Watts).—Respiratory Exercises in the Treatment of Disease: Dr H. Campbell (Baillière).—Biomechanik erschlossen aus dem Principe der Organogenese: Dr. E. Mehnert (Jena, Fischer).—Fossil Plants for Students of Botany and Geology: A. C. Seward, Vol. 1 (Cambridge University Press).—Bibliography of the Metals of the Platinum Group (Washington).—Philip's Artistic Animal Studies (Outline and Coloured Series), ditto, Fruit Studies, (Philip).

Pamphlet.—Report of S. P. Langley, Secretary of the Smithsonian Institution, for the Year ending June 30, 1897 (Washington).

Serials.—Natural Science, April (Dent).—The Atoll of Funafuti, Part 6 (Sydney).—Sunday Magazine, April (Isbister).—Good Words, April (Isbister).—An Illustrated Manual of British Birds: H. Saunders, 2nd edition, March and April (Gurney).—Contemporary Review, April (Isbister).—National Review, April (Arnold).—Transactions of the Edinburgh Geological Society, Vol. vii. Part 3 (Edinburgh).—Fortnightly Review, April (Chapman).—Psychological Review, Index for 1897 (Macmillan).—Century Magazine, April (Macmillan).—L'Anthropologie, Tome ix. No 1 (Paris, Masson).—Zeitschrift für Physikalische Chemie, xxv. Band, 3 Heft (Leipzig, Engelmann).—Journal of the Royal Agricultural Society of England, Vol. ix. Part 1 (Murray).—Bulletin of the American Museum of Natural History. Vol. ix, 1897 (New York).—Proceedings and Transactions of the N.S. Institute of Science, Halifax, N.S., Vol. ix. Part 3 (Halifax).—Journal of Botany, April (West).

## CONTENTS.