THURSDAY, SEPTEMBER 11, 1873

THE ENDOWMENT OF RESEARCH

VI.

AMONG the difficulties which are likely to impede the ready realisation of the object to which attention has been drawn, there remains one which will always be most keenly felt by those who have devoted the most thought to the question. Beneath the word "Science" there lurks a distressing ambiguity, which, though it may not force itself upon the attention of the devoted students of any particular branch, is always arising when the general claims of scientific study come on for discussion. For our present purpose it is particularly important to attach that meaning to the word which, while best justified by usage, is also most calculated to conciliate good will from all quarters.

It will hardly be denied that the name primarily belongs to those sciences called by way of distinction "natural," in the name of which this journal is conducted, and which therefore it is needless to enumerate here; and that the name is thence transferred, by reason of analogies of varying degrees of strength, to those other branches of knowledge which either in their logical methods or positive results approximate to the standard of the physical sciences. Although it would be presumptuous to attempt to lay down with exactness the line which must somewhere exist between scientific and unscientific knowledge, it must yet be always necessary to treat with much suspicion the claims of mere erudition and of social theorising to be admitted to the honoured name. The old-fashioned reputation of the grammarian or the divine, and the modern popularity of practical reformers, are neither of them grounds on which to found a title to national endowment. The unprofitable studies for which the Universities were once famous have for centuries been abundantly rewarded, and the applause of a crowded congress is ever ready to acknowledge the merits of a novel speculation in Sociology. It is not unnatural that those who know by hard experience what Science really is should jealously uphold the dignity of their pursuits, and point with pride to the innumerable advantages which mankind within the last century has reaped from their labours: but, on the other hand, the warning is not unneeded at the present day that the field of the physical sciences is not equal in extent to that which all scientific knowledge can comprehend, and that the appeal to utility may be turned into a fallacious argument. Yet further, it may be urged that those among the sciences which most attract the public attention at the hands of an accomplished experimentalist, and of which the direct practical applications are manifest to all, are least in immediate want of support from national endowments. It is for the languishing departments of Science, which have not been popularised, and of which the results have not yet been turned to commercial value, that the advantages of endowment are most required. As soon as ever the main principle of these articles is publicly recognised, the more advanced and most useful are certain to obtain sufficient care, but it is for the more backward and the least profitable that the need of help is most urgent.

It may be reasonably expected that the Universities, as their traditions become modified under the influence of the public demands, will be disposed to accept the duty of endowing scientific research under the limitations above indicated. They can have no antecedent prejudice in favour of those branches of science which either attract the most spectators, or the greatest number of self-interested students. They have always refrained with anxiety both from bidding for popularity, and from preparing their pupils for the technical business of life. Their historic position also, and the peculiar responsibilities which they cannot but feel, will cause them to interpret Science in a liberal fashion. For these reasons it is confidently hoped that, while they cannot afford to disregard the paramount importance of the physical sciences, they will maintain the position to which other sciences more closely connected with their present curriculum have of late years grown. The former, on account of their rigorous methods, the positive character of their results, and the abundance of their possible applications must always hold the first place, and present a standard for the rest; but these others also, in so far as they are really matters of scientific treatment, are in their proper subordination equally fields for original research and proper objects for endowments. The example of the German Universities has familiarised our own seats of learning with the notion that the study of languages, of antiquities, and of history, are all capable of being pursued in the genuine scientific spirit, and may lead directly to the most important positive results. Abundant evidence has been given within the last few years to show that the primitive condition of mankind and the origin of civilisation are matters which may be revealed by Science. The metaphysical explanations of the last century have given way before the well-ordered facts and regular uniformities which modern inquirers have been able to discover and arrange. The products of the human mind, and the course of human action, when displayed in their simplest and most universal forms, have been proved to be proper subject-matter for Science, no less than the law of man's physical organism or the processes of external nature. The most advanced thinkers have no hesitation in saying that the origin of natural religion is capable of being disclosed by the same methods and with equal certainty as the origin of species, and that philology yields an instrument which can unfold the secrets of an unknown past as surely as the spectroscope reveals the composition of unknown worlds. Just as modern psychology has found it necessary to borrow a large portion of its materials from the kindred science of comparative physiology, so have the nascent sciences alluded to above been under a continual obligation to the methods of physical science, and especially those to which they are linked by means of the recognised science of ethnology.

By thus widely extending the meaning of the word Science, the intention has been to widen the area over which the endowments of original research may be extended, and to give an indication of the number of directions in which scientific investigation should be encouraged. As an indirect consequence it may be suggested that this aspect of the matter shows an easy method by which the don of the last generation, an acute critic merely in longs and shorts, and erudite only in Greek particles, may be changed, without any violent transformation, into the modern scientific student. It is not so much that the subject-matter of his studies will be different, for in this respect the reform must be gradual, and is already being carried out, but that the whole spirit of his pursuit must be altered along with the tenure of his office. The principle throughout advocated is not that money should be abstracted from the Universities for general scientific objects, but that the large funds which they cannot need for the purposes of teaching, should, for the future, be devoted, not as prizes to their successful examinees, but for the support of those engaged in original scientific work: and it is contended that this is the sole means by which they can justify their retention at all of this surplus, and by which also the main objects of the first givers can be carried out. It must also be noticed that this development of our scheme brings out into greater clearness the old position that the obligation of teaching would be merely an incumbrance on our new scientific fellow. Discoveries in Science, especially those of the most important kind, are made in such a tentative fashion, and are proved by such elaborate inferences, as to be quite incapable of being communicated orally to a class; nor indeed would it be desirable that researches, as soon as made, should be forthwith promulgated from the professor's chair. The growth of Science, whether physical or not, must in many cases be militant, and may be left much more profitably to the student, who is ever investigating with a single eye to the truth, than to the teacher, who must be always careful of the form in which his doctrine is to be conveyed.

We trust, therefore, that by this attempt to show that the meaning of scientific research is not so restricted as it has been some times represented; not only has the general thesis of these articles been strengthened, but also that new adherents may be conciliated in favour of a cause which promises to connect together the followers of physical science and those at the Universities who alone worthily maintain the dignity of their ancient studies; and it must always be understood that we have looked upon the Universities as representing local State action, and that the nation must do universally what we think should be done by the University authorities locally at Oxford and Cambridge. C.

EUROPEAN SPIDERS

Remarks on Synonyms of European Spiders. By T. Thorell, Ph.D., Junior Professor of Zoology in the University of Upsala. (Upsala, 1870-73, pp. 1-644.)

FEW branches, perhaps, of entomological science show the effects of independent and isolated labours more strikingly than Arachnology—limiting this term here to the order Araneidea (or Araneæ). The great works of N. Westring on Swedish Spiders, published in 1861, that of Mr. Blackwall on those of Great Britain and Ireland, published 1861-64, and the "Catalogue Synonymique des Araneides d'Europe," by M. Eugène Simon (included in his general work "Histoire Naturelle des Araignées," published 1864), are an instance representing very strongly the fruits of this isolated labour in the same branch of natural history science. These authors appear to have been, and indeed, it is believed, actually were—the two former at all events—totally ignorant of each other's existence. M. Simon, indeed, quotes Mr. Blackwall occasionally in his "Catalogue Synonymique," but his knowledge of that author's works was apparently confined to the scanty and often erroneous quotations in Baron Walckenäers' "Insectes Aptéres." Mr. Blackwall then and M. Westring, each in his own way and with the works of other authors more or less at their common command, plodded on for years in parallel paths. Both worked diligently and laboriously, at, for a very great part, as a glance at the map would suggest, identical objects; their labours at length resulting in the respective volumes above mentioned. So much as this however can hardly be said in regard to the third one of the works noted. The "Catalogue Synonymique" bears few marks of labour at the objects themselves which it enumerates, and is in fact a mere desk work, remarkable chiefly for the limited and often infelicitous use of the materials undoubtedly available at that epoch to any author professing to gather together and to harmonise the independent and scattered morsels of an extensive branch of natural science. The good work, however, done since, and being now daily done in Arachnology by M. Simon, will soon obliterate the remembrance of the comparative failure of the more ambitious efforts of his early years.

Towards these isolated works of Mr. Blackwall and Mr. Westring the minds of Arachnologists in more than one quarter appear soon to have been directed, with a view to bring their parallel lines together. Dr. T. Thorell —of the University of Upsala—a countryman and personal friend of Mr. Westring, and an accomplished scholar, was the first to move publicly in it: and bringing great ability and clearness of head to bear upon the subject, designed an almost exhaustive work on "European Spiders." Of this work, and under that title, was published in 1869-70, Part I. with the special title of "Review of the European Genera of Spiders, preceded by some Observations on Zoological Nomenclature." This portion of the proposed work appeared in the "Nova Acta Regiæ Societatis Scientiarum Upsaliensis," ser. iii. vol. vii. Fasc. i. et ii.; but owing to some unforeseen difficulty, and unfortunately for the external continuity of the two portions of the work, the second part, intended to treat upon the more special division of the subject, was published in 1870-73 as a separate work in a different form and with an independent title, being that given at the head of this notice, viz. "Remarks on Synonyms of European Spiders;" this is, however, as may be at once seen from the respective introductions to the two, although the title of the second does not allude to it, really Part II. of the originally designed work, "On European Spiders." It is thus evident that though the present notice is upon the second work, it could not be adequately considered without first remarking briefly upon the one which preceded.

Dr. Thorell's stated object ("European Spiders," p. 1) being to fix the nomenclature of the spiders described in the works of Blackwall and Westring, it was obviously necessary first to decide upon the genera recognised by them, and by those authors also to whom they refer; and for both this and the subsequent determination of the specific name to which each spider was entitled, it was, above all, important to lay down the rules by which the author proposed to be guided in his decision between perplexed and conflicting synonyms. It is no part of the object of this notice to criticise the rules thus laid down in Part I. pp. 3—18 of Dr. Thorell's work; suffice it to say that they are substantially those laid down by a committee of the British Association, Ann. N. H. 1, vol. xi. p. 239 *et seq.* Their general reasonableness is obvious, though in many cases their rigorous enforcement would savour of pedantry and lead to undesirable results. Another feature in Part I. of "European Spiders," that is the list of works upon Arachnology, with the name of the author and date of publication, is a useful and important one, and cannot fail materially to assist the general student as well as anyone desiring to test the justice of the author's decisions. This list is considerably enlarged in the portion of the work now under consideration, pp. 584—589.

Coming then thus to Dr. Thorell's "Remarks on Synonyms of European Spiders," we soon observe that whatever the difficulties may have been which beset the determination of the generic synonyms in Part I., these difficulties must have been immensely increased when the specific nomenclature came to be decided. The task was not merely to ascertain and fix the names of those species common to Blackwall and Westring, but those of all the authors quoted by them as synonymous, with their species, together with the names of many other species into the bargain, such as those which have been subsequently described by E. Ohlert ("Die Araneiden oder Echten Spinnen der Provinz Preussen," 1867), A. Menge "(Preussiche Spinnen," 1866—1872, not yet complete), and other authors. This laborious task Dr. Thorel has executed with exceeding care and minuteness of investigation; his plan has been first to [take, by way of text, the species described by Mr. Westring, as being, with only four exceptions out of 308, known to himself from examination of the type specimens; then to determine by strict and careful comparison of figures and descriptions what species described by other authors appeared to be more or less certainly identical with those of Westring; and then which of these had priority in regard to time of publication, the date of publication being in each case placed in immediate conjunction with the names of the spider and author, and the usual reference to the name of the publication or where published. Some idea of the labour of comparison and discrimination of descriptions and figures may be obtained by the fact of the number of synonyms given of a single species, being in some instances as many as twenty-two. Each of these would form the subject of a separate investigation, independently of those, often numerous, synonyms quoted by each author cited, and which would frequently have to be investigated in a similar manner; and after all, when the frequent meagreness of descriptions and badness of figures are considered, it will be evident that the determination of synonyms must, without types of the species described for examination, be often little more than approximate guess-work; in fact it is not too much to say that the greatest care and pains bestowed upon figures and descriptions alone would give but very unsatisfactory results.

In the present instance Dr. Thorell has had the advantage (fully acknowledged in his work) of being able to examine and compare, not only the types (as before observed) of all but four of Westring's species, but also, with them, a very large proportion (nearly 250 out of 304) of those described by Mr. Blackwall, and many more described by other authors quoted in the "Synonyms." With these undeniable advantages the various considerations entering into each question of synonymic identity or distinction are detailed in a manner at once full and yet terse; and wherever a doubt has finally rested it is never slurred over or disguised.

It would be in vain to select special examples in proof of this; the details which follow the list of synonyms appended to each species, are, in almost every instance, of this thorough and honest character.

The first section of the work being occupied with the spiders described by Westring, forms by far the largest portion of the whole—pp. 1—407; for the complete determination of all the synonyms involved in this section of necessity cleared off a very large number of the species described by Mr. Blackwall. The consideration of the remainder of these forms section ii., and occupies pp. 414—470. This part ends with an exceedingly useful "List of the Spiders described and figured in Mr. Blackwall's 'History of the Spiders of Great Britain and Ireland,' together with a statement of the names believed to belong to each of them, the year in which the assumed specific name was published, and the work in which this publication took place; or instead of these last-mentioned particulars, a reference to the place in the present work where the species may have been more fully treated of." In this list, those species, about 67 in number, of which Dr. Thorell has not himself seen types, are marked with an asterisk. Section iii. contains "Synonymic remarks on some of the Spiders included in Simon's Catalogue Synonymique des Aranéides d'Europe." For reasons given in the introduction to this section, Dr. Thorell's remarks are confined to a small number of the species contained in Mr. Simon's Catalogue; in fact this catalogue, being a mere list of names, is used only as indicating some European species of general interest not contained in either Mr. Blackwall's or Westring's works.

In a work of the nature of that now under consideration, and occupying nearly three years in its appearance, it was inevitable that some errors should get in, as well as that modifications and additions should be necessitated in consequence of extended research and more accurate information obtained during that time; these, under the head of "Additions and Corrections," occupy pp. 544—582; and the remainder of the volume, pp. 582—607, is taken up with additions and corrections to that portion of the original design (mentioned at first), entitled, "On European Spiders, Part I." This is a very important, as well as interesting, *finale* to the whole, containing, as it does some further observations on nomenclature, with a disquisition on the present state of the question as to the exact functional use of the palpal organs of the male spider. Some remarks are also made upon the *fourth pair of spinners*, or inframammillary organ, discovered by Mr. Blackwall many years ago, and ascertained to be correlated (in the female spider only) with a peculiar comb-like row of bristles—calamistrum—on the metatarsi of the fourth pair of legs. Dr. Thorell appears somewhat to doubt Mr. Blackwall's position, that this organ is in all cases a true spinning apparatus; the better opinion would appear to be that it is so.

The work ends with some very valuable remarks on the general classification of the Araneidea, or (as Dr. Thorell, with good reason, prefers to call the order of spiders) Araneæ, pp. 597—607. Within this compass some recent works and suggestions on the systematic classification of spiders by Dr. Ludwig Koch, Rev. O. P. Cambridge, Anton Ausserer, and others are reviewed and criticised; the conclusion come to being that the new and highly remarkable forms brought to our knowledge by the researches of later years shows more than ever "that a fully satisfactory classification of the order of spiders is a thing not soon to be expected, and that a by no means inconsiderable number of forms cannot without great uncertainty, even if at all, be included under the hitherto received families and higher groups." Undoubtedly, towards this satisfactory classification, by whomsoever it may be finally effected, Dr. Thorell has done good work in the volume on "European Spiders," and that on their "Synonyms." The systematiser hardly exists yet who could say with truth that he had risen from a perusal of these volumes without considerable alteration, or, at least, modification, of his own previous views on the subject.

With so much to commend, in the work under review, it may perhaps appear invidious to notice what seem, to be a defect, at least in point of form. In ths course of the minute and extensive investigation of specimens, descriptions, and figures necessary to arrive at a satisfactory determination of obscure synonyma, species here and there appeared to be new to Science, and others to require separation (under other names, and with a fresh description) from those with which they had before been confounded; these new and separate species Dr. Thorell has described in extended notes, *in loco*, in a smaller type, thus marring the continuity, and breaking in upon the expressed design of the work. Would not these descriptions have come in better, and have been more useful for study and reference, had they formed an appendix to the work?

Another defect (though its rectification might perhaps be said to have been a departure from the strict design of the work) appears to be that Dr. Thorell does not include in his volume *all* the spiders at present known to be indigenous to Europe; it details those described by Westring and Blackwall, with some others given in M. Simon's catalogue, as well as, incidentally, many more described by other authors; but still it leaves unnoticed other described species. It would have given the work a great additional value had there been a general list of all the (at that present time recorded) spiders of Europe in systematic order, or, at least, a supplementary one of all those species mentioned or detailed throughout the work, in addition to those of Blackwall and Westring. This is, however, as before hinted, rather a criticism upon the design than the execution of the work, though it seems to be invited by the author's having so far departed from his own original design as to include descriptions of new species, as well as notices of others besides those included in "Araneæ Suecicæ," "Spiders of Great Britain and Ireland," and the "Catalogue Synonymique."

It would be scarcely proper to conclude this notice of a scientific work written by a native of Sweden, without a remark upon its being written in English, and a well-deserved compliment upon the exceeding clearness and terseness of the style, and its generally happy accuracy of expression.

Dr. Thorell's own opinion—expressed in a note to page 583—and in which most English-writing naturalists will probably acquiesce—is that English will one day become the common scientific language of the world, not only because it "is far more widely diffused over every part of the earth than any other culture-language, and that already two of the greatest nations publish in it the results of their scientific labours, but because English, on account of its simple grammar, and as combining in nearly the same degree Teutonic and Romanic elements, is by most Europeans more easily acquired than any other language." The opinion, however (given in the same note *l. c.*), in regard to works written in little-understood languages, such as Russian, Polish, Bohemian, Finnish, or Magyar, will hardly be endorsed. Dr. Thorell would exclude works written in these or such like languages, from equal scientific weight with others written in French, English, German &c., *i.e.*, he would not apply to the former the rules, as to priority, applied to these latter. Now, however grateful it would be to Western naturalists to have all works on Natural Science published in languages with which they are ordinarily more or less familiar, yet it would be rather too hard upon other nations, to whom the love of natural history has come sooner than a general philological culture, to be excluded from equal scientific rights with their more advanced brethren in the West. It would seem quite as just, if not more so, that if a penalty is to be paid for ignorance of foreign tongues, it should fall rather upon those who, with whatever trouble and inconvenience, certainly might become acquainted with works on Science in any language, than upon those who, preferring to write in that tongue in which they can undoubtedly think most clearly and best express their thoughts, give the results of their scientific labours in the vernacular. By all means let us have, if possible, a common scientific language, but meantime, if it be so, we must put up with the occasional annoyance of finding that a genus or species which we had fondly imagined we were the first to describe, had already, perhaps long, been well described, and possibly figured, in some unheard-of work written in an outlandish tongue not understood of the Western Scientific World.

OUR BOOK SHELF

A History of the Birds of Europe. Parts 18, 19, 20. By H. E. Dresser, F.Z.S., &c. (Published by the Author at 11, Hanover Square.)

THIS fine work continues to appear with commendable regularity every month, and keeps up its high character both for fulness of information and beauty of illustration. In the numbers now noticed are several highly artistic plates, such as those which represent the White-shouldered and Imperial Eagles, the Great Black-headed Gull, the Common Crane, the White Stork, and the Great Bustard, which each form a perfect picture. We find full but not too lengthy articles on all these, as well as on the Black Grouse, the Curlew, and many smaller birds. An excellent plan is adopted, in the more characteristic and difficult European genera, of giving a list of all the known species, with notes of their distinguishing characters and geographical distribution. One of the most rare and interesting species figured (in Part 20) is the Teydean Chaffinch, a bird of a blue colour, and which is confined to the upper limits of the pine forests of the Peak of Teneriffe, and to the desolate plains above them, feeding on the seeds of the Retanca (a broom-like plant) and the *Adenocarpus frankenoides*, which characterise those regions, as well as on the seeds of *Pinus canariensis*.

A. R. W.

Lehrbuch der Physik, von Dr. Paul Reis (Dritte Lieferung). Leipzig: 1873.

THIS forms the concluding part of Dr. Reis's useful handbook of Physics. The subject of physiological optics is continued, followed by a description of optical instruments and the laws of the interference and polarisation of light. Heat is treated in the next part, but hardly so fully nor so well as light; radiant heat, for example, occupying less prominence than it deserves. Considerable space is devoted to the explanation of machines for the conversion of heat into motive power: thus we have some of the various forms of steam-engine described, together with a full account of Ericson's heat-engine and Lenoir's gas-engine. Magnetism follows heat, and then we come to static and dynamic electricity and the practical application of electricity. The book closes with a few chapters devoted to the physics of the heavens, or in other words a brief sketch of popular astronomy and meteorology. The principal defect of this handbook is the want of sufficient woodcuts to illustrate the apparatus referred to. The whole work exhibits the characteristic solidity and thoroughness of the German race, and is a marked contrast to some of the recent French popular text-books on Science, the profuse and beautiful illustrations in which almost supplant the letterpress. Let us flatter ourselves that in our nation these complementary races intermingle.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Tyndall and Forbes

IT will probably be considered necessary that Dr. Tyndall's pamphlet,* which first appeared as an article in the *Contemporary Review*, be answered at full length. That, however, cannot be decided for some time, as several of those concerned are abroad; but it may be well to let Dr. Tyndall know at once that there is no difficulty whatever in answering him, and that the answer will not lose force or point by a little delay. In the meantime I hope you will give me space to briefly notice a few of the more obvious inconsistencies of Dr. Tyndall's article.

1. Dr. Tyndall is astonished that the "blameless advent" of his "innocent" "modest" "unpretending" volume should be looked upon as reiterating charges made against Forbes. An extract or two will settle this point.

a. "Had he (Rendu) added to his other endowments the practical skill of a land-surveyor, he would now be regarded as the prince of glacialists."

"Professor Forbes, when he began his investigations, was acquainted with the labours of Rendu. In his earliest works upon the Alps he refers to those labours in terms of flattering recognition. But though as a matter of fact Rendu's ideas were there to prompt him, it would be too much to say that he needed their inspiration."

Put these two passages into straightforward English, instead of Dr. Tyndall's favourite style of insinuation, and they amount to this: that Forbes, having the accomplishments of a land-surveyor, and being acquainted with Rendu's work, put this and that together and appropriated the discovery.

b. Forbes had, in 1860, minutely informed Dr. Tyndall of the nature and amount of his knowledge of Rendu in 1842. It is not too much to say that Dr. Tyndall's sentence quoted above is utterly inconsistent with the plain statement of Forbes, and so implies a serious personal charge against the latter.

c. A similar serious charge is made, when Dr. Tyndall, knowing that Forbes asserted that it was at his suggestion that Agassiz employed a theodolite or a fixed telescope, and that this had never been denied, carefully states that "the same instrument was employed the same year by the late Principal Forbes upon the Mer de Glace," and that "we are now on the point of seeing such instruments introduced almost simultaneously by M. Agassiz on the glacier of the Unteraar, and by Prof. Forbes on the Mer de Glace."

2. Dr. Tyndall tells us that his work was originally commenced as a boy's book, but that "the incidents of the past year" (*i.e.* his controversy with Forbes) caused him to deviate from this intention. Have boys so altered since 1859 that such controversy has now become suitable for them when supplied in the "International Series"?

3. What I said with reference to the unpublished correspondence of Forbes was said without any special reference to Dr. Tyndall. It was simply my excuse to the reader for the very meagre use I had made of so extensive and valuable a collection.

But, even in this matter Dr. Tyndall's inconsistency is patent. He says that, longing for peace, he abstained from answering Forbes, not from inability to do so, but to avoid making Science the arena of personal controversy. Yet, in the same breath, he not only complains of my not publishing certain letters which he supposes to contain charges against himself, but (see §5 below) insinuates that I am acting from feelings of animosity!!

4. Dr. Tyndall's answer to one of Forbes' charges is certainly to some extent plausible. I can say no more till I have an opportunity of consulting Rendu, for it is quite obvious that it is possible by proper selection of portions of so vaguely-written a book to make him appear to say anything one chooses.

Dr. Tyndall's answer to the other charge is so obviously insufficient that I need not deal with it here.

But more than this:—no ever-so-complete defence of himself on one or two points is any reply to the overwhelming pamphlet of Forbes, every line of which in its calm truthfulness calls for an answer.

5. Dr. Tyndall refers to former controversy between us, and to its happy termination at a personal interview. Why Dr. Tyndall should bring before the public such matters as a private reconciliation, unless with the object of holding me up to scorn as the breaker of a solemn truce, I altogether fail to see. I need scarcely say that no one in his senses would enter into an agreement never in future to differ from another, nor to point out in his writings passages calculated to mislead. But the following, and other passages which I need not cite, are all so many half-mysterious insinuations (of the Tyndall kind) against me, and all tend towards the same implied accusations.

"... the fire was not extinct: the anger of former combats, which I thought spent, was still potential, and my little book was but the finger which pulled the trigger of an already loaded gun."

I shall be obliged by Dr. Tyndall's pointing out to me a single expression, in that part of Forbes' Life which was written by me, which is calculated to give him the slightest offence:—with the one exception of a letter from Forbes, which was specially written for publication; and which, for Forbes' own sake, I would rather not have published.

No doubt he may be annoyed by my saying that little has since been added to the observations made by Forbes on glaciers. This is a matter of opinion. I do not think that Dr. Tyndall has made any addition of consequence to our knowledge of glaciers, and I am supported in this belief by many of the very highest authorities. But this is no charge against Dr. Tyndall.

6. When I saw the "Forms of Water, &c.," I added a brief and excessively temperate statement to what I had already written, and I republished Forbes' own defence of himself against Tyndall and Agassiz. Was I not bound to do something, and could I possibly have done less?

7. The rupture of the truce, or "peace," whatever that may be, was the work of Dr. Tyndall himself—partly by his "Forms of Water, &c." mainly by his article in the *Contemporary Review*. So far as I am personally concerned, the public has no right to know my feelings:—but, whatever they are, they are mingled with the satisfaction I experience in being once more free, as of old, to point out to the public the misleading passages and actual

* Principal Forbes and his Biographers.

errors in Dr. Tyndall's popular works; and to join the too thin ranks of those who, like Mr. Sedley Taylor, are not to be imposed upon by a popular reputation—but venture to think for themselves and to give the public the benefit of the result.

8. Opportunities for such public warning have never been wanting, but now they are so numerous that a long essay would be requisite to do justice to them all.

In the meantime, as an example or two, I may call attention to the way in which Sir Charles Wheatstone, and (by implication) Sir William Thomson, and others, some of whose splendid scientific labours have had the misfortune to become profitable in a pecuniary sense, are treated in Dr. Tyndall's "Lectures on Light," just published. The contrast between the utter contempt for money shown by their censor, and the (implied) opposite which is condemned as unworthy of scientific men, is brought out with all the flow of word-painting and righteous indignation which Dr. Tyndall so abundantly possesses. Besides, the monstrous doctrine is inculcated that men who devote themselves to practical applications are men incapable of original research.

9. But, to conclude for the present, I would simply call attention to the following passage, which comes from an author who in the same work treats of the relative merits of such giants as Young and Fresnel. What confidence can one have in the accuracy of any statement on a scientific matter made by the author of it?:—

"And here we may devote a moment to a question which has often been the subject of public discussion—whether, namely, a rainbow which spans a tranquil sheet of water is ever seen reflected in the water? Supposing you cut an arch out of pasteboard, of the apparent width of the rainbow, and paint upon it the colours of the bow; such a painted arch, spanning still water, would, if not too distant, undoubtedly be seen reflected in the water. The coloured rays from such an arch would be emitted in all directions, those striking the water at the proper angle, and reflected to the eye, giving the image of the arch. But the rays effective in the rainbow are emitted only in the direction fixed by the angle of 41°. Those rays, therefore, which are scattered from the drops upon the water, do not carry along with them the necessary condition of parallelism; and, hence, though the cloud on which the bow is painted may be reflected from the water, we can have no reflection of the bow itself."—"Lectures on Light," p. 25.

If Dr. Tyndall, with the assistance of his scientific advisers, fails to see the justice of my remark on this passage, perhaps you will permit me to make it the text of a little essay in a future number.

I have all along said, and still say, that I cordially recognise the services of Dr. Tyndall in popularising certain parts of Science. But his readers must be cautioned against accepting as correct great parts of what he has written. It is granted to very few men to do this useful work without thereby losing their claim to scientific authority. Dr. Tyndall has, in fact, martyred his scientific authority by deservedly winning distinction in the popular field. One learns too late that he cannot "make the best of both worlds."

I would request Dr. Tyndall for his own sake, not for mine, should he favour me with a reply, not to pick out one or two isolated passages of a letter, which absence from books may possibly have rendered slightly inaccurate—but to answer me, as he has *not* answered Forbes, in the full spirit and not in the partial letter.

P. G. TAIT

St. Andrews, Aug. 20

W. S. J. on Hegel

I RESPECTFULLY request admission, into an early number of NATURE, for an explanatory word or two, in reference to W. S. J.'s review of my poor book on Law, &c., in the valuable publication named, for July 24, 1873.

W. S. J.'s very first sentence speaks of the said little book as containing "a discussion of Hegel's opinions concerning gravitation and the differential calculus." In the first place, Hegel has nothing to say against either gravitation as a fact, or the differential calculus as an established method of indubitable scientific calculation: he would only attempt to philosophise both by placing metaphysical principles under them. Now this is part of Newton's own action, and he certainly would not object to any *attempt*, Hegel's or other, in the same direction. In the second place, I discuss no opinion of Hegel in this reference: I only attempt to expose erroneous opinions of Hegel's relative opinions. To this I strictly confine myself, and this goes much deeper than the reader may, at first, think.

On Law, whatever is said by W. S. J., concerns only the old difficulty of Hegel's *dialectic*; and perhaps the italicising of this word, together with my own intellectual deficiences, may be respectfully offered in explanation of as much. W. S. J. here, then, is evidently misintelligent himself, and, accordingly, only speaks so as to induce misintelligence in others. Nevertheless, it is worth saying that the reader may or may not gain from the particularity of satire in W. S. J.'s hands—so keen is it that it cows, and, again, so kindly that it disconcerts.

Mathematically, according to W. S. J., "the critical statement of the necessary outcome of Hegel's philosophy," reduces itself to this, that the principle in question is placed "in that in which the quantum has disappeared, and there remains the relation only as qualitative relation of quantity." W. S. J. has for this only the mildly-authoritative contempt of a duly-elevated position; and when it is said "What is called infinitely *little* is only qualitative, and is neither little nor great, nor quantitative at all," he at once squelches all by an "*on the contrary!*" Now all this contemned matter comes directly, not from Hegel, but from Newton; for the former, quoting from the latter, says:—

"These (N.'s increments and decrements) are not to be taken as particles of a definite magnitude (*particulæ finitæ*). Such were not themselves moments, but magnitudes, generated out of moments; what is to be understood, rather, is the principles or beginnings (elements) of finite magnitude:" that is, plainly, what is concerned lies "in that in which the quantum has disappeared as quantum, and there remains the relation only as qualitative relation of quantity."

What concerns comets is naively amusing. We have not had to wait in their regard (as W. S. J. seems to think) for the information of "Chambers' Handbook." The astronomers of the last century, as it appears, were able to speak better than even the "Handbook." Comets that return, they say, though after a great many years, travel in ellipses of enormous axes; whereas those that do not return may describe parabolas or hyperbolas. Such is the opinion of Science yet, though it may talk of many other explanations of non-return, as dissipation, interception, &c. This, I say, is how Science looks yet; but W. S. J., for his part, is under the belief that Science has actually within its ken comets that (so to speak) *revolve* in hyperbolas, as well as others that revolve in ellipses. (Positively such seems his idea. Now, Hegel is never once at fault here—in his own way, I mean; for whether in ellipse, in parabola, or hyperbola, Hegel's assignation of the moment of singularity to the comet is, on his own principles, justifiable. May not a non-returning comet, too, be attributed to that contingency which is, and must be, inherent in externality as externality? On the whole, it may be well for us all to let comets alone yet. Our greatest living authority can only philosophise them into stone-rattles which the sun (for his amusement?) whirls about his head.

One has only to consider these things and others the like—the exquisite little gibes, not forgotten, about a secret in two volumes and a secret in fifteen pages, &c.—to perceive that what we have here is only once again the blind rush of prejudice from its usual dark corner of relative ignorance—an ignorance which it will persist in, and not (through study) convert into the light of day. There is that approbative allusion to Mr. Smith, too; W. S. J. will yet be ashamed of that.

On the whole, however, I hope I have not spoken disrespectfully, for I cannot fancy who W. S. J. may be. He talks of law and logic, and is possibly a lawyer; he certainly has a profound contempt for "Hegel and his satellite Stirling;" but were he (what his initials may indicate) "the eminent Jaggers" himself, I cannot, whatever his power of *practice*, admire his capacity for *principles*.

Edinburgh, July 28 J. HUTCHISON STIRLING

Lakes with two Outfalls

IN NATURE, Aug. 14, a paper under this heading concludes thus:—"Colonel George Greenwood, who is, I presume, the same as the former active correspondent about this subject, visited this lake (Lesjeskaugen) last summer, as appears from the entry of his name in the day books. I am not aware that he has since published any opinion, but the lake seems, so far as I can judge, to support his view of the matter.—W. Stanley Jevons." I sent an account of my visit to Lesjeskaugen Lake to the *Geological Magazine* in July 1872, but it was not so fortunate as to meet with acceptance from the editor. The following extracts coincide singularly with the opinions of Prof. Stanley Jevons:—

The river Rauma, at the western end, which gives its name to Romsdal, is the *natural* outlet. The outlet to the river Logen, at the eastern end, is entirely *artificial*. The water-parting there, between Romsdal and the Dovre Feld, is an ancient ridge of drift. A cut has been made by man through this ridge. The stream through this cut now works a saw mill, but was formerly connected with the old iron works. The one outlet from the lake enters the mill pool, from which there are two outlets, one to serve the mill the other for the waste water. All these three outlets are kept each at its required level *artificially*, that is, with piles, logs, boulders, and rubble, so that the quantity of water which is let out of the lake is regulated by 'the miller and his men.' The case is precisely equivalent to the Black Loch, in Dumfriesshire, whose *natural* (!) outlet is an iron sluice in a stone dam opening to a mill lead cut through the water-parting to Lord Bute's mill. (See *Athenæum*, Aug. 6, 1864, and 25 in. Ordnance maps.) If such lakes as these are lakes with two outlets, then the new conduit for the water supply of Glasgow makes Loch Katrine a lake with two outlets. An old dry channel is in direct continuation of the present mill lead. It passes so close to the old iron work as actually to touch its base. If, as I imagine (as does also Prof. Stanley Jevons), the two are connected with each other in origin, the artificial outlet to the lake may be of very great antiquity.

A notice in NATURE, of a new work by Capt. Burton (1872), quotes these words of his: "The northern and north-western portions of the so-called 'Victoria Nyanza' must be divided into three independent broads or lakes . . . *in order to account for the three effluents*, within a little more than sixty miles." Here, then, the great traveller adopts my dictum, that "a lake can only have one outlet." I first published this dictum in the *Athenæum*, July 4, 1863, when the late lamented Capt. Speke, in his "Sketch Map," gave four outlets to Lake Victoria Nyanza, three on "native information;" and in the *Athenæum*, July 25, I said, "I think that the native information will prove to be erroneous." GEORGE GREENWOOD

Brookwood Park, Alresford, Aug. 15

As Prof. Jevons has revived the question of the existence of lakes possessing more than one outlet, I would invite the attention of your readers to what appears to me an unequivocal instance of the kind, though on a small scale, in the neighbourhood of the place whence I write.

On the high and very broken ground between the old mountain road from Dolgelly to Towyn (which runs at the foot of Cader Idris) and the south shore of the estuary of the Mawddach is a watershed, which separates streams flowing directly into the estuary by Capel Arthog from others which, after joining the stream that descends from Llyn y Gader in the hollow immediately under the summit of Cader Idris, find their way into the estuary some three miles higher up. On this watershed lies a lake about half-a-mile long, named Llyn Creigenen, which occupies a rock basin with two lips at exactly the same level, one at its western, the other at its eastern extremity. By the western lip a small stream issues which descends rapidly and at one part of its course forms one of the branches of the Falls of Arthog, well known to visitors at Barmouth. By the eastern lip also, a stream, diminutive, it is true (at any rate in the summer months), but still quite distinct, issues and descends into a boggy tract, along which it wanders for some two miles, until it joins the stream before mentioned from Llyn y Gader. These facts are distinctly recorded on the Ordnance map, and I have frequently verified them myself and pointed them out to others. I think there can be no doubt but that in this instance both of the outlets are *natural*, and that a stream must issue from one if a stream issues from the other, at any rate at the ordinary level of the water in the lake. It is perhaps, impossible to say that both outlets are *permanent* in that *secular* sense which Prof. Jevons seems to attribute to the word, as circumstances are easily conceivable under which the flow through the smaller easterly outlet might cease; but at any rate for many years, supposing the average supply of water to the lake to remain the same, and no artificial barrier to be erected, the two streams will continue to issue from the lake at all seasons.

Prof. Jevons remarks that "on *à priori* grounds it seems very unlikely that there should exist any lake with two distinct outflows." I would reply that, while it is undoubtedly improbable that any particular lake named at random should possess this characteristic, it can hardly be regarded as *à priori* very unlikely that among all the lakes on the earth's surface there should be found here and there one with more than a single outlet. At any rate, I would recommend anyone who is sceptical in the matter to visit Llyn Creigenen, which is but an easy hour's walk from the Arthog Station on the railway between Barmouth Junction and Dolgelly. ROBERT B. HAYWARD

Capel Arthog, Aug. 16

Cranes in the Gardens of the Zoological Society of London

IN NATURE of June 26 (*antea*, p. 164), Mr. W. A. Forbes points out an error in the report of the meeting of the Zoological Society for June 15, in a statement that no example of *Grus vipio* (*sive leucauchen*) had been brought to Europe previously to those lately received by that Society. Instead of "Europe" the word "England" should have stood in the paragraph in question, which would then have been correct.

It is quite true (as stated by Mr. Forbes) that the collection of living cranes in the Gardens of the Zoological Society of Amsterdam is the finest in the world. At the same time the series of these birds in the Regent's Park is also at the present moment very nearly perfect, embracing, as it does, examples of all the usually recognised species, with the exception of *Grus leucogeranus*, and *G. monacha*.

Of the former of these the Society once possessed a living specimen, but the rare *G. monacha* of Japan has, I believe, never yet reached Europe alive.

The following is a list of the Zoological Society's present series of the Gruidæ:—2 Common Cranes (*Grus cinerea*), 1 Brown Crane (*G. canadensis*), 2 White-necked Cranes (*G. leucauchen*), 1 Sarus Crane (*G. torquata*), 1 Australian Crane (*G. australasiana*), 1 White American Crane (*G. americana*), 1 Mantchurian Crane (*G. montignesia*), 2 Wattled Cranes (*G. carunculata*), 1 Balearic Crowned Crane (*Balearica pavonina*), 4 Cape crowned Cranes (*B. regulorum*), 3 Demoiselle Cranes (*Anthropoides virgo*).

August 27 P.L.S.

Colour of Lightning

I SHOULD be much obliged to any of your readers who would give me any information as to the cause of the colour of lightning.

In one of two storms which passed over here yesterday evening the lightning was decidedly pink in tint; later in the night it had regained its normal yellow or bluish colour.

Odrey, Aug. 25 H. GEORGE FORDHAM

Harmonic Causation and Harmonic Echoes

IN reference to the question of "Harmonic Echoes," allow me to suggest to those who may have the opportunity of observation, how desirable it is that these echo-tones should be investigated in a manner to determine whether they are truly harmonic or not. There would be no difficulty in testing the sounds given in response to the notes of a closed organ-pipe and an open one, or the notes of representative musical instruments, clarionet and flute. It might be found that the echo at Bedgebury Park would give the octave always, irrespective of the particular instrument provoking it; or, on the other hand, that it refused to answer to a closed pipe, or gave only the twelfth, its proper reply. We should then know whether the echo-tone was that of the harmonic or a new fragmental tone consequent on the breaking up of the wave of the fundamental or ground-tone, by "breakers ahead."

Now that we are called upon to recognise several varieties or classes of musical tone, it is time that the leaders in Science came to a general agreemeut upon the use of the term "harmonic." Is it to be applied indifferently to "over-tones," otherwise "partial-tones," to "combination-tones," to "concussion-tones," arising from the violence of the shock of sound-waves in collision, to "fragmental-tones" produced out of the wave of the ground-tone broken up by obstacles encountered in its course or in reflection, and to "echo-tones" which may be affiliated to either variety? It seems to me that we risk much confusion unless "harmonic" is restricted to its earlier usage, and applied solely to the "harmonic series,"—the tones which are the direct offshoots of the fundamental. These tones have but one order of succession, and will bear no interpolation: the octave, twice the velocity of the fundamental; the twelfth, which is three times; the fifteenth, or double octave, four times; the seventeenth, five times, and so on, always an acceleration by uniform addition. In the examples taken from the compilation of Dr. Brewer, the echo-tones go beyond all law of harmonic progression, and must be accounted for as belonging to other classes of tones, if the data can be relied upon, seeing that some instances are questionable as to authority, and others are beyond proof. The Bedgebury Park instance may be taken as proved; it is simple, and attested by living authority. Who will vouch for the other instances as evidence? The question is not put to cavil, but because of the dubiousness of the possession by the several recorders of the necessary qualification for an accurate estimate of the phenomena recorded.

Musical people of any pretension to critical power in these matters are generally "self-centered," each individual considers himself competent to pronounce judgment on "pitch," yet the delusiveness of this belief would be testified to by none more readily than by men who are daily engaged in tuning and in estimating minute relations of the invisible geometry denominated "pitch." Notwithstanding long experience and daily practice, no sooner does any question arise out of ordinary routine than they hesitate to depend on judgment alone, but resort to comparison with some fixed pitch already ascertained, and by this means prove themselves to be frequently at fault when least expecting it. Harmonic sounds are difficult to judge of, they lie at wide intervals, are frequently sharp, and if pure and faint, the ear is as liable to be deceived by an apparent lowness as it is with pure ground-tones. Fineness of ear for perception of niceties of pitch is by no means a common endowment, and where it exists, does not certify a fine musical organisation. Pitch bears relation to musical tune and to quality of tone similar to that which geometry bears to figure drawing and to painting. In rare instances only are the faculties for each associated in fair proportion, and frequently the possession of one power seems to exclude or override the others. Some men are gifted in this respect, and will tell you the pitchnote of a button, or a pencil, or a pin, as accurately as they will the notes of a song; or will discriminate, without hesitation, every note in a series of complicated chords with a skill almost as sure as instinct.

Professor Tyndall introduced the term "over-tones" in connection with "harmonic;" more recently, in Helmholtz' Lectures, Mr. Ellis has substituted "partial tones;" and Mr. Sedley Taylor adopts the same. This is a pity, for there is something incongruous in the idea of "partial tones" which yet are complete whilst component, and Tyndall's term "over-tones" is far more expressive.

The question of harmonic force, in which probably lies the explanation of the Bedgebury echo, came before me a few days since in experiments made to obviate, if possible, the wavy unsteadiness common to stopped pipes with high-cut mouths. Many variations were made without useful results. On withdrawing to some few yards' distance from the pipe into a recessed doorway, it was observable that the fundamental tone completely vanished, and the first harmonic, the twelfth, came into prominence instead of it, although you had only to step a yard forward to become again aware of the continued co-existence of the fundamental. On comparing this segregated twelfth with a corresponding note in the scale of the standard pitch of the organ, it was found to be decidedly too sharp, and thus the real cause of the waviness of tone was discovered, thereby saving many experiments in a false direction.

Several works now give elaborate analyses of harmonic tones; Mr. Sedley Taylor's "Sound and Music" is the last most useful addition, and supplies much previously wanting. In no work, however, do we meet with any definite statement as to the causation of harmonic tones; yet it seems necessary for the full understanding of their nature and of the relation they bear to the instruments producing them that the mode of their origination should not be left unheeded. The conclusion derived from my own investigations is that the harmonics of musical instruments have their origin solely in the *surplus energy* of the generating force over and above that necessary to produce the fundamental tone; this superabundant vigour finds its outlet in accessory vibrations, and the harmonics are the escape valves for securing to the fundamental tone freedom from fluctuations to which otherwise it would succumb. When the vibrating force is inadequate to waken the ground-tone of an organ pipe it settles down into the harmonic nearest related to its power; the tone may be considered as surplus energy, since it is disproportionate to its work, and only becomes harmonic because it falls short of the fundamental after which it is striving. Except in this relation we should regard it as ground-tone. When a pipe is overblown, the harmonics maintain themselves through the excess of energy to the complete exclusion of the fundamental, and they are sharp to the regulated pitch of the pipe. Harmonic tones when thus produced independently have considerably more intensity than the normal tones of pipes of corresponding pitch. In all the orchestral wind instruments it is the higher notes that require greatest wind-force for their production; the clarionet alone differs as respects a certain range of its high notes, where the reverse is the case, the force being considerably less than for the lower range, but the structural conditions of the instrument sufficiently account for the peculiarity.

The experiment with the stopped pipe previously described clearly shows the penetrating power of accessory tones, and that whilst the fundamental occupies the ear by its volume, the harmonic has the strongest vitality even in its associated condition. In view of these facts we may reasonably infer that the "octave echo" in Bedgebury Park is the reflected harmonic heard alone; still it would be well to prove it in the manner suggested. That the voice was returned from a plantation "across a valley," gives intimation of a distance favourable to the loss of the fundamental tone in the depths of the valley; and that "the original sound required to be loud and rather high" is an additional assurance of the presence of harmonic vigour in the vocal tone.

A remarkable instance of echo freaks within my own experience is well timed to be spoken of here. At the bottom of my garden there is a meadow, then a double row of houses with a high railway embankment at the end, and a wall rising beyond that. About two months ago, whilst looking over the meadow at the clouds of sunset, the sound of a band in the distance came upon me, and, immediately following, the sound of another and more demonstrative band from an opposite direction, giving prediction of horrible discord. Strange to say, although the two bands were playing most noticeably different melodic phrases, there was no conflict; one band seemed to be the symphonic accompaniment of the other, and there was a peculiar charm in the effect, causing regret that the music should come to a natural end. Knowing that the *first* band was echo-music, there was at once a singularity to attract attention, how to account for the precedence of that which should be secondary? but the greater puzzle was to understand how it came to pass that the music was *different*, so that whilst listening the illusion of a distinct band was difficult to dispel, doubts arose about Echo having any voice in it at all, only that from time to time the pauses between the phrases showed the *following* of the form upon the shadow. Reflection upon the matter afterwards furnished the probable explanation. The distance of the place of echo was approximately between six and seven hundred feet from the source of the sounds, my standing place being at about one-fourth of the distance; between me and the band three houses intervened, over which the music came to me, whilst the terrace on which the band was stationed opened freely on to the meadow; thus Echo received the music earliest by reason of the unobstructed passage, and her rendering was that of natural selection, the most vigorous tones, and the penetrating harmonics, whatever had most living power, infused by the players and sustained by the characteristics of the instruments, all these reached her and rose again in perfect accord with the original harmony, whilst all the other notes, those of low vibrating power and of inferior stamina, were lost by the way. It should also be noted that observation afterwards of the angle of incidence and positions of the band and of the listener showed that the course of the sound waves on their way to Echo was in *front* of a detached line of cottage buildings, then passing into the enclosed space between the double row of houses up to the embankment, the recourse being by the *back* of the cottage buildings, across gardens and the meadow to the listener. Doubtless the singular vividness of the phenomena was due in great measure to the state of the atmosphere, which at the time was peculiar, the western sky heavy with gorgeous clouds, and the air silent and sultry. The relation which the organ-pipe experiment first detailed has to the theoretical solution here offered will be readily perceived; and but for the support afforded by it one could hardly have ventured on the statement and the explanation, which else would have appeared to be, the one unreal, and the other fanciful.

August 25 HERMANN SMITH

The Oreodon Remains in the Woodwardian Museum

MY attention has just been accidentally called to some notes on "Oreodon Remains in the Woodwardian Museum, Cambridge" in your number of August 14.

I hasten to correct an error into which your correspondent has fallen as to the locality in which the remains to which he refers were obtained. I did not visit the Mauvaises Terres of Nebraska, but collected all my specimens in the valley of the John Day River, in Upper Oregon, about long. 120° 10′ W., lat. 44° 40′ N.

Most of the specimens are from near the head of a small stream called Bridge Creek, a locality well known to Prof. Marsh, whose new species of Oreodon described in the *American Journal of Science and Art* was possibly obtained there. A few, however, are from the Great Cañon higher up on John Day's River, nearly opposite Old Camp Watson, where I passed the winter of 1871-72.

I was informed by a gentleman who accompanied Prof. Marsh's Yale College Expedition, in October 1871, that they had on that occasion found a skull of a new and unusually large species of Oreodon in one of the places above mentioned. But your correspondent is probably acquainted with all the descriptions that have been published in America, and will know whether the *Oreodon superbus* of my informant has or has not yet been christened in print.

I have regretted much since my return that I only devoted parts of three days to a search for these interesting remains.

WALSINGHAM

Merton Hall, Thetford, Sept. 5

Bright Shooting Stars

I BEG to send you the following particulars of the observed paths of nine bright shooting stars recently seen here.

Ref. No.	Date.	Time.	Apparent Mag.	Began R.A.	Began Dec. N.	Ended R.A.	Ended Dec. N.	Length of path.	Approx. Radiant point.
		h. m.							
1	July 28	11.32	= ♃	210°	49°	200°	38°	14°	Pegasus
2	,, 28	11.48	1st mag. *	202	44	193½	36	10	Pegasus
3	,, 30	10.45	1st mag. *	42	43½	45	36	8½	χ Persei
4	Aug. 2	11.40	= ♃	43	54	62	56	12	Pegasus
5	,, 7	9.33	2 × ♀	190	59	195	30	30	Polaris
6	,, 9	10.12	= ♀	41	75	196	73	30	χ Persei
7	,, 9	10.29	1st mag. *	37	45	50	42	10	Andromeda
8	,, 9	11.25	1st mag. *	337	59	304	50	20	χ Persei
9	,, 9	11 29	1st mag. *	28	41	12	46	12	Andromeda

No. 5 in the above list was the brightest, and left a very perceptible train just N. of Cor. Caroli for 7^s. No. 9 also left a train, visible for 3^s, N. of γ Andromedæ.

The evening of August 9 was clear, and two observers counted thirty-five meteors in the interval between 10h. 15m. and 11h. 45m., after which time clouds obscured the sky. During the night of August 10 it remained overcast. Of the thirty-five shooting stars seen on August 9, the great majority were Perseids, but the radiant region is diffusely extended from the star group at χ Persei to B Camelopardali. There were also indications of radiation from Pegasus and Andromeda. The August meteors of this year appear to have been larger than those seen in former years; at any rate bright meteors have been exceptionally abundant during the dates included in the above list.

WILLIAM F. DENNING

Bristol, August 11

November Meteor Shower of 1872

MR. E. D. JONES, of San Paulo, Brazil, has sent me the enclosed extract from his diary, referring to the meteor shower of November last, which he observed whilst crossing the Atlantic.

HENRY C. BEASLEY

Gateacre, Liverpool, Sept. 3

"Nov. 27, 1872, s.s. *Halley*, N. lat. 11° 30′, W. long. 26° 50′. —There was a splendid shower of meteors this evening. I saw them shooting in profusion as soon as it was dark (about half-past six). I sat in a chair on deck facing the west, where Jupiter was flaring in the tropic sky, and watched the flying messengers from other worlds. I counted no less than 400 in half an hour, that is at the rate of about 14 per minute. They came in shoals, as it were. There would be a long pause, and then five or six would fly across together, reminding me forcibly of the flying-fish we had seen in the daytime. Every now and then a much brighter one than usual would flash into existence, and leave a trail of beautiful reddish light behind. Generally speaking, they were as bright as a star of the second magnitude. But the brighter ones I speak of were quite equal to stars of the first magnitude. One splendid one at about eight o'clock (local time) was so bright that it lit up the sails of the ship; it was of a red colour, and burst in two before disappearing. One later on left a trail which I could distinguish for half a minute. I was able to trace the point in the heavens from which the meteors emanated, viz., a point near the northern extremity of Perseus, between that constellation and Andromeda. About this point I often saw them come into view, and die away with scarcely any apparent motion, on account of their coming in a straight line towards the observer; below this point they fell towards the horizon, above it they fell across the zenith, and so on. Those with the longest path were in the western sky (opposite Perseus), as the view was the least fore-shortened there. The position of the *Halley* was that given at the heading of this extract. The following table shows that we probably did not see the thick of the shower, having passed it by daylight:—

G.M.T.	Time in which 100 were seen.	Number per minute.
8.30 P.M.	8 minutes	12·5
8.38 ,,	7 ,,	14·3
8.45 ,,	7 ,,	14·3
10.5 ,,	17 ,,	5·9
10.22 ,,	17 ,,	5·9
10.49 ,,	22 ,,	4·6
12.15 A.M.	36 ,,	2·8

"The reasons that the first observation gives fewer than the second, may be that the twilight did not allow of the less brilliant meteors being seen; that the eye of the observer was not so well practised in detecting them; and the light clouds flying through the air may have obscured some of them. The other observations show a regular decrease in the numbers from 8·45 P.M.

"I counted 750 meteors in my observations, and saw quantities more besides. Of course I could only see about one-third of the sky at a time, but I was looking in the direction of the thickest fall most of the time, so that I daresay I saw half the number that actually fell; taking this for granted, there must have been 3,500 between 8.30 P.M. and 12.15 A.M., Greenwich mean time."

EXPLORATIONS IN THE GREAT WEST

WE are now in possession of facts which will supplement our last reference to this subject. The following names may be added to the list of scientific men accompanying the Wheeler Expedition engaged on surveys west of the 100th meridian:—Mr. Severance, ethnologist; Drs. H. C. Garrow and J. L. Rothock, naturalists; Mr. H. Stewart Brown, meteorologist; Messrs. Klett and Louis Mell, topographers. The entire force numbers 175 men.

The surveying party of Mr. Clarence King, geologist, designated as the Geological Survey of the 40th parallel, has just finished its work and is recently disbanded. Among the scientific men accompanying it were Messrs. J. G. Gardner (astronomer and geographer), Wilson (topographer), J. D. Hague (mining geologist), Emmons (assistant geologist), Arnold Hague (chemist and mineralogist), Robert Ridgway (zoologist), and S. Watson (botanist). The force is largely absorbed by other expeditions now in the field. The results of this expedition are expected to fill five quarto volumes and accompanying atlases; of which one on mining in Nevada and adjacent territories with folio atlas will be by Mr. Hague, and one on botany is already published. The remaining volumes are well under way and will, it is expected, be completed during the present year.

There is an expedition known as the International

Northern Boundary Commission, engaged in the survey of the 49th parallel from the Lake of the Woods to the crest of the Rocky Mountains. Archibald Campbell, of Washington, is the commissioner in charge; Major Twining is the chief of engineers on the part of the United States, and Dr. Elliott Coues of the U.S. army is the naturalist of the expedition. The British Government details its proportion of the party, which is thoroughly equipped for this service. The operations of the present year extend westward from Pembina.

The expedition under Major J. Powell, to the cañons of Colorado, is still in the field. Major Powell has spent several years in explorations in this region, and has constructed a map of great interest and accuracy. His ethnological researches among the Piute and other Indian tribes have resulted in a large and exceedingly valuable collection.

Mr. Wm. H. Dall, well known by his elaborate work on the Territory of Alaska, founded on his former three years' residence in that region, is now actively employed in continuing his survey and hydrography in the Aleutian Islands, under the direction of the U.S. Coast Survey, a work on which he has been engaged during the past two years. His labours have been principally in Alaska and the adjacent islands, from which he returned last September, having gone there in the summer of 1871. He spent last winter in San Francisco, in preparing for the expedition of the present year, which included fitting out a vessel expressly for this service. Among other objects contemplated is the selection of an island on the western extremity of the Aleutian chain, to serve for the landing of the Japan cable, for laying which the U.S. steamer *Narragansett* has been detailed to make deep-sea soundings. Mr. Dall is assisted by Prof. Baker, of Ann Arbor, Mich., astronomer.

Mr. Henry W. Elliott is at the head of a private expedition to St. Paul and St. George, the fur-bearing seal islands of Bering's Sea. He has the assistance of Captain Bryant, who is in charge of the U.S. Revenue and other Government interests on these islands. Mr. Elliott is making exhaustive collections in natural history, which he sends to the National Museum at Washington, his investigations respecting seals and walruses being especially valuable and complete. His labours during 1872 were demonstrated by twenty large boxes of collections. He is a very skilful draughtsman, and his drawings of natural subjects are remarkable alike for accuracy and vigour.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE *

V.

IV.—*The Metric System*

AS a system of weights and measures, constructed on strictly scientific principles, the metric system may justly claim pre-eminence over all others. It was established upon the fundamental basis of the *metre*, its primary unit of length bearing a determinate decimal ratio to one of the largest natural constants, that is to say, the ten-millionth part of the earth's meridian-quadrant. It includes a fixed relation between the units of weight and capacity, the *kilogramme* and the *litre*, and the unit of length, the *metre*, from which both are derived; and it comprehends a uniform decimal scale of multiples and parts of these units. It must, however, be admitted that the more recent progress of modern science has demonstrated that the actual standards of metric length, weight, and capacity do not exactly correspond with their scientific definition; and apart from the insuperable difficulties which have been found to exist in the precise determination of material standards from any natural constant, the unanimous opinion of several of the highest scientific authorities in this country has been deliberately expressed that there is no practical advantage in adopting a unit founded in nature over one of an arbitrary character. In truth, the great advantage of the metric system consists in the simplicity and uniformity of its decimal scale, and the great convenience of this scale for all purposes of account as agreeing with the decimal system of notation, and more especially when combined with a decimal coinage which formed part of the original scheme. These undoubted advantages have proved the chief recommendations to the adoption of the metric system, first by France, and afterwards by so many other countries, and generally in the scientific world. There is now every prospect of the metric system being generally adopted in all countries of the civilised world, thus greatly enhancing its value as a common international system of weights and measures, and constituting, as it were, a universal language for expressing all quantities weighed or measured.

The original steps which led to the establishment of the metric system in France were taken with a view of reforming the old French system of weights and measures, which had become intolerable from their defective state and want of uniformity. In 1790, on the motion of M. Talleyrand, in the National Assembly, the question o the formation of an improved system to be based upon a natural constant, was referred to the French Academy of Sciences. A request was also made at the same time to the British Government that the Royal Society should act jointly with the French Academy, but no response was given to the invitation, in consequence of the distrust then entertained in this country at the progress of the revolutionary party in France. The preliminary work was consequently entrusted to five of the most eminent members of the French Academy, Lagrange, Laplace, Borda, Monge, and Condorcet. The important Report of this Committee, which bears also the signature of a sixth member, Lalande, gave rise to the metric system. It was presented to the Academy on March 19, 1791, and is printed at length in their Memoirs. The choice of the fundamental unit of the new system lay in its derivation either from the length of the seconds-pendulum, of the earth's equator, or of the earth's meridian. The Committee rejected the length of the pendulum beating seconds as the basis of the new standard unit of length, because it involved a heterogeneous element, that of time, as well as an arbitrary element, the division of the day into 86,400 seconds. They proposed a unit of length taken from the dimensions of the earth itself, and not dependent upon any other quantity; and they did not hesitate to select as its basis the quadrant of the meridian in preference to a quadrant of the equator, from its being a universal measure applicable to all countries, as every country was placed under one of the meridians of the earth, whilst only few countries are under the equator. They considered also that no greater dependence could be placed upon the regularity of the equator, than upon the equality or regularity of the several meridians. They recommended the ten-millionth part of the quadrant of the meridian as the definition of the new fundamental unit of length. Renouncing the ordinary subdivision of the meridian-quadrant into degrees, minutes, and seconds, they proposed a uniform decimal scale as practically the best, from its agreeing with the scale of arithmetical notation. In order that no other arbitrary principle should be introduced into the new system of weights and measures, they recommended for the basis of the unit of weight a measured quantity of distilled water, being a homogeneous substance, always to be easily found in the same degree of purity and density; and that such quantity should be weighed in a vacuum at its temperature when passing from a solid to a liquid state.

* Continued from p. 370.

For the practical purpose of ascertaining the length of the meridian quadrant, they proposed to measure an arc of the meridian from Dunkirk to Barcelona, a distance of nearly 9½°, and comprehending about 6° to the north and 3½° to the south of the mean parallel of latitude. These extreme points had also the advantage of being both at the sea level. The actual operations required were stated to be as follows :—

1. To determine the difference of latitude between Dunkirk and Barcelona.

2. To re-measure the ancient bases which had served for the measurement of a degree at the latitude of Paris, and for making the map of France.

3. To verify by new observations the series of triangles employed for measuring the meridian, and to prolong them as far as Barcelona.

4. To make observations in lat. 45° for determining the number of vibrations in a day, and in a vacuum at the sea level, of a simple pendulum equal in length when at the temperature of melting ice, to the ten-millionth part of the meridian quadrant, with a view to the possibility of restoring the length of the new standard unit, at any future time, by pendulum observations.

5. To verify carefully and by new experiments the weight in a vacuum of a given volume of distilled water, at the temperature of melting ice.

6. To draw up tables of existing measures of length, surface, and capacity, and of the different weights in use, in order to ascertain their equivalents in the measures and weights of the new system, as soon as they should be determined.

In pursuance of the recommendations of this Report, the law of March 26, 1791, was passed by the National Assembly for constructing the new system upon the proposed basis; and the Academy of Sciences was charged with the direction of the necessary operations. They entrusted the measurement of the arc of the meridian from Dunkirk to Barcelona to two of their members, Méchain and Delambre, who carried on the work during seven years, from 1791 to 1798, notwithstanding many great difficulties and dangers.

The unit of measure adopted for the actual measurement was the existing French standard of length, the Toise of the Academy, better known as the *Toise de Perou*, a measure of 6 French feet (*Pieds du Roi*). This standard is now deposited at the Observatoire at Paris. It is a bar of polished iron, about 1½ inch in breadth, and ⅓ inch in thickness, and a little longer than a toise. The length of a toise is marked by a rectangular step near each end of the bar, leaving the remaining portion at the ends half the thickness of the measuring part of the bar.

The true length of the toise was taken about a line (or $\frac{1}{12}$ inch) above the re-entering angles of the bar, at the temperature of 13° Réaumur, or 16°·25 C. It has been declared to be equal to 76·7563 English inches, the old French foot (which was divided into 12 inches and the inch into 12 lines), being equal to 12·792 English inches. The toise was afterwards found to be equal to 1·94904 metre.

This standard had been originally constructed as the unit for measuring an arc of the meridian in Peru, and for verifying the meridian of Paris, in 1740; and it was substituted in 1766 for the more ancient French standard of length, the *Toise du Grand Chatelet*, from which it had been originally derived. This older toise was deemed wanting in the scientific precision requisite for a standard of length. It had been constructed in 1668, and is said to have been 5 lines shorter than the toise measure then ordinarily used, for which no authoritative standard could be found; and to have been actually derived from the width of the inner gate of the entrance to the Louvre, which, according to the original plan, was made 12 feet wide, and one half of this width was taken for the length of the standard toise.

The measures actually used for the survey operations are known as the *Règles de Borda*. They were four in number, each consisting of a bar of platinum two toises, or 12 French feet, in length, about ½ inch broad, and $\frac{1}{12}$ inch thick. Each platinum bar was fixed at one end only to a bar of brass about 11½ feet long, the other end of the platinum bar being free and extending about 6 inches beyond the corresponding end of the brass bar. The object of this second bar was that it should form, together with the first bar, a metallic thermometer, indicating the temperature of the two bars by their difference of dilatation, which could be measured by a fine vernier. The four measuring bars were accurately verified, and found, when placed together, end to end, not sensibly to differ from eight times the length of the Toise of Peru at the temperature of 12°·5 C.

The base for the measurement of the northern portion of the work was measured at Melun, and found to be 6075·90 toises. The base for the southern portion was measured at Perpignan, and found to be 6006·25 toises.

Meanwhile the Academy of Sciences was abolished in 1793, by a decree of the National Convention, and a Commission of eleven scientific men, consisting principally of those who had been previously engaged in the proceedings, was appointed, in 1795, to carry out all the arrangements for the definitive establishment of the Metric System. In 1798, towards the close of the operations, an equal number of scientific men, representatives of foreign countries, were added to the Commission, which was then composed as follows :—

French Members: MM. Borda, Brisson, Coulomb, Darcet, Delambre, Lagrange, Laplace, Lefevre-Gineau, Legendre, Méchain, de Prony.
From the Batavian Republic: Aeneae, Van Swinden.
Sardinia: Balbo, afterwards replaced by Vassali, from the Provisional Government of Piedmont.
Denmark: Bugge.
Spain: Pédrayés, Ciscar.
Tuscany: Fabbroni.
Roman Republic: Franchini.
Cisalpine Republic: Mascheroni.
Ligurian Republic: Multedo.
Helvetian Republic: Trallès.

The final results of all the operations for determining the new metric unit of length, were stated by the Commission in their Report, dated April 30, 1799. They found :—

1. That the length of the arc of the meridian comprehended between Dunkirk and Barcelona, was 9·6738° (or 9° 40′ 45″), and measured 551,584·72 toises.

2. Assuming, from the previous measurements in France and Peru, that the mean ellipticity of the earth was $\frac{1}{334}$, they computed the length of the meridian-quadrant to be 5,130,740 toises.

3. That the length of the new unit of length, the ten-millionth part of the meridian-quadrant, was equal to 0·5130740740 toise, or 3 feet and 11·296 lines; being 443·296 lines of the Toise of Peru (which contained 864 lines), at its standard temperature of 16°·25 C. In terms of the new standard unit, the Toise of Peru was equal to 1·949036591 metre.

4. That the length of the pendulum at the temperature of melting ice, beating seconds in a vacuum at the sea level at Paris, was equal to 0·99385 metre.

The actual construction of the new standard measure of length had been entrusted to the mechanician Lenoir. As a preliminary proceeding, he made four end-standard metres of brass, differing in length very slightly from each other, and each about equal to 443·242 lines of the Toise of Peru. This was the computed length of one ten-millionth part of the meridian-quadrant, as deduced from the previous measurements of an arc of the meridian in France made in 1740. The length of these four brass metres, when placed end to end, was nearly 1,773 lines, thus exceding double the length of the Toise of Peru, by about 45 lines. Lenoir constructed a supplementary measure of this excess of length, and its exact relation to the toise was ascertained by numerous comparisons, for which other intermediate measures were employed, and their exact length determined. The actual comparisons of the four brass metres were made not with the Toise of Peru itself, but with two standard toises constructed by Lenoir, the length of each of which in relation to the Toise of Peru had been carefully determined. In these comparisons the additional length of the measure of 45 lines was also employed. The comparing instrument was a *comparateur* made by Lenoir, which enabled very minute differences in measuring bars under comparison to be read off on a subdivided scale by means of a contact lever. One division of this scale was equal to 0·00001 toise, and one-tenth part of one of these divisions (= 0·001949 mm.) could be read off with the aid of a vernier. It appears from the Report of MM. Borda and Brisson, dated July 17, 1795, that the result of a number of comparisons, including those of the four metres with each other, showed metre No. 2 to be nearest to the required length, being 443·4519 lines of the Toise of Peru at the mean temperature during the observations of 12°·96 Réaumur, thus very closely approaching its standard temperature of 13° Réaumur, and exceeding the required length at this temperature by only 0·0119 line. It was accordingly selected as the provisional Standard Metre. But they considered that its standard temperature would more conveniently be fixed at 10° C., and as, according to Borda's determination, the coefficient of dilatation of brass between 0° and 32° C, was 0·00001783 for 1° C. they determined its length at 10° C. to be 443·401 lines of the Toise of Peru.

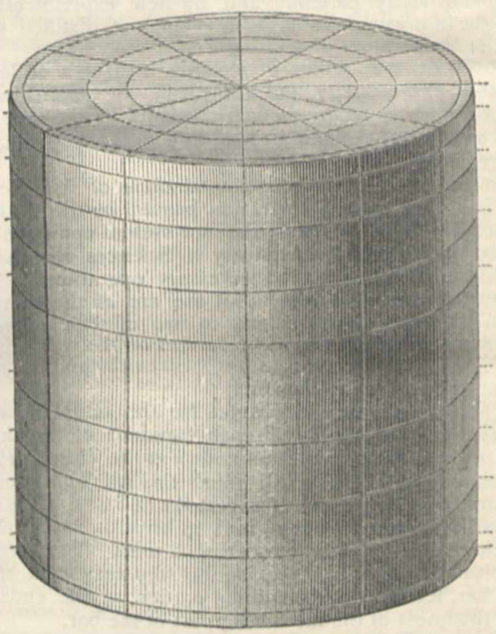
For obtaining the definitive standard, which was to be the length of 443·296 lines of the Toise of Peru at 16°·25 C., which was thus so nearly indicated by the provisional metre, two standard metres of platinum, and twelve metres of iron, were constructed by Lenoir, his comparing apparatus having been improved so as to show differences of 0·001 line. The Commission were not satisfied with making numerous comparisons of these metres and the provisional metre of brass among themselves, but they also compared them repeatedly with the four *Règles de Borda* and a new supplementary measure of above 45 lines, so as to determine not only their relative and absolute length, but also the rates of expansion of the three metals of which they were composed. The rates of expansion definitively adopted by the Commission, from observations made by Borda between 0° and 32° C., were as follows:—

		In a metre.
Coefficient of linear expansion of platinum for 1° C.	= 0·00000856,	or 0·0031 mm.
,, brass	,, = 0·00001783,	or 0·0092 ,,
,, iron	,, = 0·00001156,	or 0.0063 ,,

The comparisons and corrections of the several metres were continued until no difference amounting to 0·000001 toise, or 0·001 millimetre, could be found at the temperature of melting ice, either in their desired absolute length of 443·296 lines of the Toise of Peru or in relation to each other. They were consequently all determined to be perfectly exact. One of the platinum metres, subsequently known as the *Metre des Archives*, from its place of deposit, was reserved as the new prototype measure of length; the other was kept at the Observatoire at Paris, as its accessible representative. The twelve iron standard metres were distributed amongst the several countries represented at the Commission.

The primary *Metre des Archives* is a rectangular platinum bar, bearing no mark or description. Its breadth is 25 mm. (0·984 in.), its height 3·5 mm. (0.138 in.). Its ends are planes perpendicular to its axis of length, and the straight line between them in this axis denotes the true length of the metre at 0° C., or the temperature of melting ice. It thus constitutes what is termed a *Mètre-à-bouts*, or end-standard metre.

The unit of metric weight was defined to be the weight in a vacuum of a cubic decimetre of distilled water at its maximum density, or the temperature of 4° C. Distilled water was selected as the best material in nature for thus determining the unit of weight, from its being obtainable everywhere and at all hours in the greatest purity, its being perfectly homogeneous, and its density being invariable at any given temperature. It was required first accurately to ascertain the weight of this volume of water, and then to construct a metallic standard of equivalent weight. The necessary operations for effecting both these objects were entrusted to M. Lefevre-Gineau in 1795. He had to decide between two modes of proceeding for accurately determining the volume of water to be weighed; one, by measuring the internal capacity of a vessel to contain this volume of water; the other, by measuring externally a solid or hollow body, in order to ascertain the weight of the volume of water displaced by it. He chose this last method, considering that the accurate external measurement of a metallic body was much less difficult than that of the internal capacity of a metallic vessel; and it was determined that the best form of this body was a cylinder of a height equal to the diameter of the base, this form



IG. 10.—Cylinder for determining cubic centimetre.

being capable of being made and measured with the greatest precision.

It was not thought requisite that the cylinder should be of the specified volume of a cubic decimetre, but only of the most convenient size for arriving at the desired result by computation. The cylinder actually used was made of brass, and hollow, being only so much heavier than the same bulk of water as to enable it to sink by its own weight when plunged in water. It was intended to be 2·435 decimetres (about 9½ inches) in diameter and height.

To facilitate the accurate measurement of the cylinder, 12 radial lines or 6 diameters were drawn on its base plane, dividing it into twelve equal parts; and corresponding lines were drawn on its upper plane. The ends of these two series of lines at the circumference were joined by vertical lines on the cylinder, thus dividing it vertically into twelve equal parts. Circular lines were also traced on the two plane surfaces at about 11 mm. from the circumference, and at half and two-thirds of the radius from the centre; and eight horizontal lines were drawn around the cylinder at the following distances from the base :—13, 35, 67, 95, 148·5, 176·5, 208·5, 230·5 millimetres. The height of the cylinder was determined from the ascertained mean distance of the corresponding 37 points of intersection of the lines on the upper and lower surfaces, including the centres. The diameter of the cylinder was determined from the ascertained mean length of the 48 diameters, included between the corresponding points of intersection on its cylindrical portion.

The measurement was effected by means of an apparatus specially constructed for the purpose by Fortin, and it indicated minute differences of length of $\frac{1}{4000}$ line, or $\frac{1}{1700}$ mm.. The standard measures used for determining the absolute length measured were 16 brass measures specially constructed for the purpose, each very nearly equivalent to the height of the cylinder, and 16 other measures, each nearly equivalent to its diameter. The length of each of these two series of measures in relation to each other was ascertained by numerous observations with the new apparatus ; and the total length of each set of 16 measures in relation to the new standard unit was obtained by comparing the sum of their length with Borda's *règle* of 2 toises, No. 1, to which they very nearly corresponded in length, by means of the *comparateur* used for the comparison of these large measuring rules.

The final result of the measuring operations was that the mean height of the cylinder was determined to be 2·437672 decimetres, and its mean diameter 2·428368 decimeters, at the temperature of 17°·6 C. According to Borda's determination of the coefficient of the linear expansion of brass, the volume of the cylinder was determined by computation to be nearly 11·28 cubic decimetres, when at the temperature of melting ice.

For ascertaining the weight of water displaced by this cylinder, a series of brass weights was specially constructed, consisting of a unit or provisional kilogram, made as nearly as possible of the estimated weight of a cubic decimetre of water, together with 11 exact copies and smaller weights in decimal subdivision down to the millionth part, all carefully verified and deemed to be accurate within less than half of one-millionth part.

The mean weight of the cylinder in ordinary air was taken, no reduction to a vacuum being deemed requisite, as the weights used were of similar metal to the cylinder, the interior of which communicated with the external air. For this purpose a metallic tube, 1·285 mm. in diameter, was screwed to the top of the cylinder, its end being out of the water when the cylinder was immersed. The top of the cylinder was 43 mm. from the surface of the water during the weighings, and the volume of the tube immersed was therefore 55·77 cubic mm. Taking the volume of the cylinder to be 11·28 cubic decimetres, the volume of the metallic part of the cylinder was computed to be 1·506 cub. decim., and of the hollow part filled with air 9·774 cub. decim. During the weighings the cylinder was surrounded with ice, but the temperature of the water was never below 0°·2 C. and the mean temperature was 0°·3. The final results of the weighings were declared to be as follows :—

	Prov. kilo.
Weight of the cylinder in air, in terms of the unit employed	= 11·4660055
Its mean weight in distilled water, after deducting the weight of air in the cylinder, and of the air displaced by the weights used	= 0·1967668
Hence weight of the volume of distilled water equal to the volume of the cylinder	= 11·2692387

H. W. CHISHOLM

(*To be continued.*)

NOTES

It is announced that the Transatlantic Balloon will leave New York to-day. It will carry four passengers—Prof. Wise and Mr. Donaldson, the aëronauts ; an officer of the United States Signal Service, and an agent of the *Daily Graphic*. They hope to reach some point on the English or Continental coast in about sixty hours from their departure from New York. They have with them six very powerful and experienced carrier-pigeons, purchased in Belgium, which, if liberated from the balloon within "pigeon flight" of the coast, are expected to fly directly to their old homes. Each of these has painted on his breast, in indelible ink, the outline of a balloon, and on his wings the words, "Send news attached to the nearest newspaper." Despatches received by these pigeons should be sent to the nearest newspaper for publication. We wish these daring men a safe landing ; but while we do this we regard the enterprise as one needlessly hazardous, so far as the settlement of the scientific problem is concerned.

Mr. Campbell, the Chief Secretary of the Inspectorate-General of Customs in China, is now in Europe with a view of obtaining instruments for a complete chain of meteorological stations in that country. It is also proposed to transmit weather information all along the east coast of Asia. This is great news, and we shall return to this important matter, giving full details of the proposals.

Miss Elizabeth Thompson, of New York, has made a donation to the American Association for the Advancement of Science of 1,000 dols., for the purpose of advancing scientific original research ; and she intends repeating the donation annually during her life.

M. Stephan has succeeded in finding Faye's Comet. The correction of the Jahrbuch Ephemeris is almost *nil*.

Mr. Froude, who is now with the *Devastation*, informs us that it is Mr. W. Barlow, not himself, who is president of Section G at the ensuing meeting of the British Association. Mr. Froude will, indeed, probably not even be able to attend the Bradford meeting at all.

We learn from the *Monthly Microscopical Journal* that Prof. Gegenbauer, of Jena, the well-known comparative anatomist, has been nominated Professor of Anatomy and Director of the Anatomical Institute in the University of Heidelberg.

The arrangement made by Prof. Henry, of the Smithsonian Institution, a few months ago, for the interchange between America and Europe, by Atlantic cable, of important astronomical discoveries and announcements, appears to have borne excellent fruit. One great object of this movement was to enable astronomers in all parts of the world to concentrate attention upon any celestial phenomenon before too great a change of place had occurred, or before the intervention of a long period of moonlight after the first discovery. On the 26th of May last Prof. Henry announced a new planet, discovered by Prof. Peters, to the Observatory of Paris, among other institutions, and on the following night it was looked for by the director of the Observatory of Marseilles, who at once detected it, and subjected it to a careful criticism. The announcement of three planets has thus far been made from the Smithsonian Institution to Europe ; the only return communication being that of a telescopic comet, discovered at Vienna on July 5. On being notified of the fact, Prof. Hough, of the Dudley Observatory at Albany, made search for it, and succeeded in finding the object without any difficulty.

Biology is flourishing at the Antipodes. The last mail has brought us "Australian Vertebrata, fossil and recent," by G. Krefft, curator and secretary of the Australian Museum, Sidney ; a list of Australian Longicorns, chiefly described and arranged by Francis P. Pascoe, with additional remarks by George Masters, assistant curator of the Australian Museum ; Guide to the

Australian Fossil Remains exhibited by the trustees of the Australian Museum, by G. Krefft, curator and secretary; a Catalogue of the Marine Mollusca of New Zealand, by Capt. F. W. Hutton, assistant geologist; and a paper on the Geographical Relations of the New Zealand Fauna, by the same.

WE have received from the Science and Art Department the following list of Queen's Medallists in the Science Examination, May 1873; we regret that want of space compels us to give only the gold and silver medallists.—Practical Plane and Solid Geometry: Atkinson, Roger, Crewe Mech. Inst., gold; Millington, F. H., Patricoft Mech. In., silver.—Machine Construction and Drawing: Daltry, Thomas L., Newcastle, Elswick Mech. In., gold; Atkinson, Roger, Crewe Mech. In., silver.—Mathematics: McAlister, Donald, Liverpool In., gold; Edwards, Harry H., Liverpool In., silver. — Theoretical Mechanics: McAlister, Donald, Liverpool In., gold; Sisson, William, Newcastle Mech. In., silver.—Applied Mechanics: Millington, Fred. H., Paticroft Mech. In., gold (obtained gold medal in 1872); Dixon, Samuel, Manchester Mech. In., gold; Daltry, Thomas L., Newcastle, Elswick Mech. In., silver.—Acoustics, Light, and Heat: Martin, T. W., Newton Abbott, gold; McAlister, D., Liverpool Inst., silver.—Magnetism and Electricity: McAlister, Donald, Liverpool Inst., gold; Louis, Henry, Islington Sci. and Art Sch., silver.—Organic Chemistry: Whiteley, John, Halifax W. M. Coll., gold; Taylor, William D., Belfast, W. M. Inst., silver.—Geology: Dowlen, Ethelbert, Woking, St. John's, gold; Southeran, Arthur, Marske Inst., silver.—Vegetable Anatomy and Physiology: Dowlen, E., Guildford Science, silver.—Navigation: Windass, John T., Hull Nav. Sch., gold; Daws, Thomas, Plymouth, Courtenay Street Sch., silver.—Nautical Astronomy: Lawson, Henry, Hull Nav. Sch., silver (obtained silver medal in 1872); Ashford, Joseph, Hull Nav. Sch., silver.—Steam: Fairweather, James, Glasgow, Anderson Univ., gold; Daltry, Thomas L., Newcastle, Elswick Mech. Inst., silver.—Physical Geography: Forbes, James L., Torphins Sci. Sch., gold; Armstrong, J. W., Blackburn School of Science and Art, silver.

MR. J. WOOD-MASON, of Queen's College, Oxford, is to officiate as Professor of Comparative Anatomy and Curator of the Comparative Anatomy Section of the Medical College Museum, Calcutta, during the absence, on furlough, of Dr. J. Anderson.

MESSRS. LONGMANS announce the following among their forthcoming scientific publications:—A new volume of Transatlantic Travel, entitled "The Atlantic to the Pacific; What to See, and How to See it," by John Erastus Lester, M.A., author of "The Yo-Semite, its History, Scenery, and Development." A study of Asiatic savage life, entitled "A Phrenologist amongst the Todas, or the Study of a Primitive Tribe in South India—History, Character, Customs, Religion, Infanticide, Polyandry, Language," by William E. Marshall, Lieut.-Col. of H.M. Bengal Staff Corps. A second Supplement to Watts's "Dictionary of Chemistry." The first Supplement, bringing the record of chemical discovery down to the end of the year 1869, was published in 1871. The second Supplement, now in course of preparation, is intended to bring the record of discovery down to the end of 1872, including also the more important additions to the science published in the early part of 1873. This Supplement will form a volume of about 800 pages, and is expected to be ready in the year 1874. The author has been fortunate in securing the co-operation of several of his former contributors. A new work on "Sidereal Astronomy," by R. A. Proctor. "Introduction to Experimental Physics, Theoretical and Practical, including Directions for Constructing Physical Apparatus and for Making Experiments," by Adolf F. Weinhold, Professor in the Royal Technical School at Chemnitz, translated and edited (with the author's sanction) by Benjamin Loëwy, F.R.A.S., with a Preface by G. C. Foster, F.R.S., Professor of Physics in University College, London. "A Treatise on Practical, Solid, or Descriptive Geometry, embracing Orthographic Projection and Perspective or Radial Projection," by W. T. Pierce, Architect, late Lecturer on Geometrical Drawing at King's College, London, and at Harrow School. "On the Sensations of Tone, as a Physiological Basis for the Theory of Music," by H. Helmholtz, Professor of Physiology, formerly in the University of Heidelberg, and now in the University of Berlin, translated from the third German Edition by Alexander J. Ellis, F.R.S., formerly Scholar of Trinity College, Cambridge. "Organic Chemistry," by H. E. Armstrong, Ph.D., Professor of Chemistry in the London Institution; "A Manual of Qualitative Analysis and Laboratory Practice," by T. E. Thorpe, F.R.S.E., Professor of Chemistry in the Andersonian University, Glasgow, and M. M. Pattison Muir; "Telegraphy," by W. H. Preece, C.E., Divisional Engineer Post Office Telegraphs, and J. Sivewright, MA., Superintendent (Engineering Department) Post Office Telegraphs; "Elements of Machine Design, with Rules and Tables for designing and drawing the Details of Machinery," adapted to the use of Mechanical Draughtsmen and Teachers of Machine Drawing, by W. Cawthorne Unwin, B.Sc. Assoc. Inst., C.E., Professor of Hydraulic and Mechanical Engineering at Cooper's Hill College; "Principles of Mechanics," by T. M. Goodeve, M.A., Lecturer on Applied Mechanics at the Royal School of Mines, and formerly Professor of Natural Philosophy in King's College, London. These five works form part of the series of text-books now being published by the Messrs. Longmans.

AMONG Messrs. Macmillan's announcements of forthcoming works are—"On the Theory of Sound," by Lord Rayleigh, F.R.S.; "Contributions to Solar Physics," by J. Norman Lockyer, F.R.S., with numerous illustrations; "Cave Hunting," by W. Boyd Dawkins, F.R.S., being researches on the evidence of caves respecting the early inhabitants of Europe; "The Origin and Metamorphoses of Insects," by Sir John Lubbock, F.R.S. (vol. ii. NATURE Series); and a new edition of Canon Kingsley's "Glaucus."

DURING the ensuing season Messrs. H. S. King and Co. will publish the following new volumes of their "International Scientific Series":—"Mind and Body," by Alex. Bain, LL.D.; "Animal Mechanics," by J. Bell Pettigrew, M.D., F.R.S.; "Principles of Mental Physiology," by W. B. Carpenter, LL.D., F.R.S.; "On the Conservation of Energy," by Prof. Balfour Stewart; "The Animal Machine, or, Aërial and Terrestrial Locomotion," by Prof. C. J. Marey; "The Study of Sociology," by Herbert Spencer. With the exception of the last-named work, the whole of the above will be illustrated.—Messrs. H. S. King and Co. also announce the following books of interest to scientific men:—"Studies of Blast-furnace Phenomena," by M. L. Gruner, translated by L. D. B. Gordon; "The Norman People and their Existing Descendants in the British Dominions and the United States of America," and "The History of the Natural Creation," a series of popular Scientific Lectures on the Theories of Progression of Species, by Prof. Ernst Hæckel.

MR. VAN VOORST has recently published new editions of "Blackwall's Researches in Zoology, illustrative of the Structure, Habits, and Economy of Animals," and Salvin and Brodrick's "Falconry in the British Isles."

PROF. E. D. COPE has been bold enough, in the August number of the *Penn Monthly* (Philadelphia), to portray his conception of the general external appearance of the new gigantic mammal from Wyoming, named *Tinoceras anceps* by Marsh, and *Loxolophodon cornutus* by himself. The result is an elephantine form, with elephantine knees, feet, ears, and tail; bovine preputial sheath; and a head with two pairs of somewhat cervine horns, and an anterior pair of simple but diverging processes. A proboscis about half as long as the head is made to project forwards in a Tapir-like manner, below the base of which the upper canines descend in a way which shows that it would be impossible to use them for defence or obtaining food, without doing great injury to the sensitive trunk which overshadows them. Nothing seems more illogical than the assumption, that because an animal has elephantine proportions and feet, it should possess a proboscis, especially when all arguments from the skull tend in a different direction.

THE Quarterly Weather Report, from July to September, contains, in addition to the usual tabular results, a discussion of four years anemometrical results for Bermuda.

WE have received the Report on the Freshwater Fish and Fisheries of India and Burmah, by Surgeon-Major Francis Day, Inspector-General of Fisheries in India.

WE have received from Prof. Edward Morse an excellent paper, read by him before the Boston Society of Natural History, on the Systematic Position of the Brachiopoda, in which, from a careful study of the anatomy and development of those animals, he has been led to endorse and substantiate Steenstrup's opinion as to their affinities being with the Annelids instead of with the Mollusca, as generally believed. The following is his concise summary :—"Ancient Chaetopod worms culminated in two parallel lines—on the one hand in the Brachiopodæ, and on the other in the fixed and highly cephalized Chaetopods. The divergence of the Brachiopodæ, having been attained in more ancient times, a few degraded features are yet retained, whose relationships we find in the lower Vermes ; while from their later divergence the fixed and cephalized Annelids are more closely allied to present free Chaetopods." The author lays stress on the certainly soft and uncalcified condition of the earliest forms of life causing great imperfection in the earliest geological record.

IN the death of Mr. William S. Sullivant, which is recorded in the scientific columns of *Harper's Weekly*, and which took place at Columbus, Ohio, on April 30 last, the United States has lost one of its most accomplished botanists, especially in the department of the mosses, in which he was the recognised head for many years. From a biographical notice published by Professor Gray in the *American Journal of Science*, we learn that Mr. Sullivant was born in 1803, near Columbus, in the vicinity of which place he resided the greater part of his life. His first publication appeared under the title of *Musci Alleghanienses*, a work on the mosses and liverworts of the Alleghany Mountains, illustrated by prepared specimens of the plants themselves. This was shortly after 1843, and a few years later a work on the same subject was published in successive numbers as a memoir of the American Academy. The section of Mosses and Hepaticæ in Prof. Gray's *Botany of the Northern United States* was prepared by Mr. Sullivant, and credited to his pen. A separate edition was subsequently published by the author. A work on the mosses of Cuba was prepared by him, illustrated by specimens collected by Mr. Charles Wright. He also published, in 1859, the account of the mosses collected by the Wilkes expedition. The most important of Mr. Sullivant's publications, however, consists of his *Icones Muscorum*, being "figures and descriptions of most of those mosses peculiar to Eastern North America which have not been heretofore figured"—this forming an imperial octavo volume with 129 copper-plates. It is stated by Prof. Gray that a second or supplementary volume of *Icones* was in preparation by Mr. Sullivant, and nearly completed at the time of his death.

THE additions to the Zoological Society's Gardens during the past week include two Mouflons (*Ovis musimon*) from Sardinia, presented by Mr. H. E. Holloway ; two Barbel (*Barbus vulgaris*) and a Bream (*Abramis brama*) from British seas, presented by Mr. E. S. Wilson ; two Sacred Ibises (*Geronticus æthiopicus*) from Gough's Island ; a Black-handed Spider Monkey (*Ateles melanochir*) from Central America, purchased ; five Horned Lizards (*Phrynosoma cornutum*) from California, deposited.

SPÖRER'S OBSERVATIONS ON THE SUN*

THE author gives chiefly the results of his spectrum observations, and simultaneous spot observations, recorded in the Transactions of the Berlin Academy of Sciences for November 1871, and May 1872. To the two earlier instances of striking changes observed in the protuberances, there is added an interesting observation of August 8, 1872. It was estimated that the prolongation of the upper part of the protuberance had a velocity of forty-two kilometres per second, parallel to the sun's surface. In the case of many protuberances, it will be readily allowed that they are not only subject to cyclones, but also owe their origin to them. Protuberances of similar form, observed on several successive days, in the same heliographic latitude, Spörer has accounted for, by the supposition of volcanic eruptions, owing to the smaller rate of linear rotation of the deeper strata ; if, however, we regard these protuberances as the results of cyclones, the explanation of the changes of position would rest upon the impelling power of the storms, and their tendency to create new forms ; and the velocity of the advancing cyclone would, in several instances, average 1·4 kilometre.

Spörer, in this work, adheres to his division of protuberances into two classes. Secchi, in his work on the Sun, has distinguished four classes of protuberances, but afterwards accepted Spörer's twofold division. Both observers are at one in this, that the protuberances, which Spörer has named "flame" and Secchi "ray" protuberances, give different spectral lines, and stand in intimate connection with the spots. But with regard to the proper hydrogen protuberances, Secchi says they are not in the condition to give rise to a spot, against which Spörer adduces examples of their influence in neighbouring spot formation, especially prominent in the intervals between considerable protuberances of hydrogen.

The observation of the protuberance, which Secchi also noticed, on July 7, 1872, and which gave a well-marked image with the line 6543, is particularly described, and drawings are appended.

With regard to observations of spots, interesting comparisons are given, showing the difference between the two hemispheres in respect to the frequency of spots, and the mean heliographic latitudes. In this connection, Carrington's observations, from November 1853 to the beginning of 1861, are gone into, so that the comparisons embrace a period extending from November 1853 to the end of 1871. With regard to frequency of spots, it appears that the southern hemisphere exceeds the northern both in maximum and minimum. The curves also show distinctly the rapid passage from minimum to maximum, and the slow decrease after the maximum.

The mean heliographic latitudes are obtained through assigning to each group of spots, a factor of value (*Werthfactor*). The union of five-rotation periods gave a point of the curve for the northern as well as for the southern hemispheres. Carrington had obtained from his observations the striking result, that the spots at the time of the minimum approach the equator, thereafter veered off to higher latitudes, and that then the more numerously spotted zones gradually approached the equator. Spörer, by his observations since 1861, has confirmed this result.

* Translated from a review in *Der Naturforscher*, No. 29, of Beobachthungen der Sonne, von. Prof. Dr. Spörer, Abhandlung zum Programm des Gymnasiums. Separat-Abdruck. Anklam. Verlag und Druck von Richard Poettcke ; 1873.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE meeting of the American Association for the Advancement of Science was held this year at Portland, in the State of Maine, during the fourth week of August; there was a large attendance of well-known scientific celebrities and members. The following account, for which we are indebted to the *New York Tribune*, will give an idea of some of the most important papers and discussions.

A discussion on the Darwinian hypothesis, which was started by Prof. Swallow, who is a vigorous opponent, was continued by Dr. Dawson, who began by stating with some fullness of detail the demands upon our credence made by the advocates of the evolution theory. Among other requirements of the theory, he said, it must provide an explanation of the origin of life. To accomplish this the experiments of Bastian were brought forward. Referring to these, he stated that no less an authority than Prof. Huxley, though an evolutionist, had denied their conclusive character and disputed the alleged results. We are expected to admit, in every department to which scientific inquiry relates, that in all things there has been a successive progress from the lower to the higher. Why should we make this admission? What proof is there of it? The recent discoveries of embryology, showing the likeness of early forms of the embryo to other animals of the same families, furnished to the advocates of evolution no real argument in its favour. They proved nothing. Admit if you will the close resemblance of similar bones and general physical structure in the ape and man, it is not the slightest evidence of identity. While it may be true that there is bone for bone in monkey and in man, still it remains that the bones of one are different from those of the other. The making of monkey and of man is explicable quite as readily, to say the least, on the theory of plan as on that of evolution. The history of the growth of an animal has been cited as the evidence of a development from a lower to a higher form. But what are the facts in the case? The egg grows into the animal, and that organism produces an egg again. This is revolution, not evolution.

We are told to accept as a postulate that mind too is a result of development; that the moral as well as the material being is simply a consequence of the evolving process. I do not grudge the naturalists who have adopted such theories the intellectual exercise which is involved, but I regret that much of their labour is wasted, and the results will be burnt when the fires of truth are applied to the chaff they are accumulating. This is not a question of physics that they are arguing, it is one of metaphysics, and it would be well for our children as well as growing scientists if they were taught more of mental and moral philosophy as a basis for such inquiry.

But I thank the students who are thus engaged for some good results of their exertions. They have thereby succeeded in reducing the superfluous numbers of species, and have obtained far better views in respect to classification. Good results will also flow from the profound embryological researches of the day. But I am sorry for the investigators, for their reputations are at stake, and they have chosen a mistaken path.

We are, however, approaching in our studies a correct theory. After its appearance in geological history, every species has a plastic tendency to spread to its utmost limits of form. Then ensues a period of decadence until it may become extinct. This has been set forth in some of my printed memoirs on the plants of the carboniferous series. I believe that a similar process is true of the human race. He referred to the skull of Mentone and its finally developed character—a grandly developed man cerebrally and bodily. The burial of his dead testified to his religious belief. The people of the Cromagno skull age were of a similarly elevated character. The only point of difference from men to-day was in the flattening of one of the leg-bones. This was perhaps a result of the habits of the tribe, running through forests in pursuit of game. It begins to be admitted that the man of Western Europe came in with the modern mammalia at the close of the glacial period. This was a period of decadence, and when the pliocene fauna were dying out and new forms were taking their places. The most ancient form of man is beyond the average standard of modern humanity. If the man of Cromagno or Mentone had been sent to Harvard, he would have been graduated with the full honours of an average American student.

Prof. Morse stated that Dr. Dawson and Prof. Swallow had both misquoted Prof. Huxley, who had said, in respect to the ancient skull referred to, that it might have held the brains of a thoughtless savage, or it might have contained those of a philosopher. Dr. Dawson had referred to only the differences in those remains from those of the man of to-day in respect to the flattened tibia. There were, however, several other characters of a similar nature which Dr. Dawson had not referred to, some of which had been discovered by Prof. Wyman, and had not yet been published. In the existing races of man the *foramen magnum* (the large opening at the base of the skull through which the brain communicates with the spinal cord) exhibited very little change of position in its relation to the rest of the skull, while with the higher primates (apes) this opening is very near the posterior portion of the skull. In eleven ancient skulls from the shell heaps of Tennessee, the *foramen magnum* in every case was nearly an inch further back than in those of present existing races. The powerful muscles on the sides of the head that move the jaws leave a distinct line at their upper points of attachment. These lines are called temporal ridges. In all present existing races a space occurs on the top of the skull, between these lines, of from three-and-a-half to four inches. In the apes these muscles meet in the median line which rises into a bony crest so characteristic of the gorilla. There was a remarkable skull discovered by Prof. Wyman in the lowest beds of the ancient shell heaps of Florida. This has the temporal ridges approaching each other within a half inch at the top of the skull. If the high development of the skull referred to by Mr. Dawson was such as he states, it only carries man further back. Similarly, in the light thrown upon the history of man by the wonderful discoveries in archæology, where we meet with traces of an ancient civilisation, with complicated language and manners, we can surely believe in savage hordes pre-existing from which this ancient civilisation has been evolved.

As to the early traces of man, we must fully appreciate the rare possibility of their occurrence. Wherever you dredge in the waters of the present day the traces of man are among the rarest discoveries. The Lake of Haarlem, upon whose waters naval battles have been fought, and on whose shores a dense population has existed, was drained, and on its bottom not the slightest traces of man's existence were found. Prof. Morse dredged repeatedly for years off the shores of Maine, and no trace of man was ever brought up, except a single spike. When we consider how abundant the material for such remains must be now compared with those furnished by the simple methods of life and the sparse population of earlier days, the indications of man's existence in geological eras must be of the rarest occurrence. In fact, in such rocks as the drift, only the rude stone implements could be preserved.

The evolution theory as compared with that of special creation presented similar features to the undulatory theory of light as compared with the emission theory. Newton's theory required a new modification with every discovery in optics, until, as a writer said at that time, the emission theory is a mob of hypotheses. The undulatory theory of Young not only explained all that was difficult to Newton, but gave physicists the power of prevision. So with evolution. It not only accounts for existing phenomena, from the modification of a flower or the spot on a butterfly's wing to the genesis of the solar system, but it has endowed naturalists with the gift of prophecy and enabled them to predict the intermediate forms afterwards discovered in the records of the rocks.

On Calvert's Supposed Relics of Man in the Miocene of the Dardanelles. By G. Washburn.—The author reports, in view of the facts to which the paper refers, as to the flints, the split bones, and the marks upon the fossil bone, that he believes that Mr. Calvert and Sir John Lubbock (who had never seen the specimens) are mistaken in the conclusions to which they have come, and that they have not been able to find any evidence whatever at the Dardanelles in reference to the antiquity of man.

The Rotation of the Planets as a Result of the Nebular Theory. By Prof. Benjamin Peirce.—Prof. Peirce's paper set forth an explanation of the actual rotation of the planets on the supposition of their being formed according to the nebular hypothesis, from rings thrown off from the rotating main body in the process of condensation. He instanced more particularly the planets Jupiter and Saturn. The inner portions of such a ring having a less velocity than the outer ones, axial rotation in the same direction as that of the primary would be determined in the breaking up and running together of the ring into a planetary body. He showed, by a mathematical analysis of the movements of the particles composing the ring, that the velocity of the resulting rotation must be such as is actually observed in the case of the planets referred to, whose mass represents nine-tenths of the whole planetary system.

In Jupiter and Saturn, the velocity of a particle in the planet is very nearly the velocity of the planet itself. Then Jupiter and Saturn must have derived their material from the whole mass of the planetary system. The best theories of the earth make it of uniformly decreasing density from the surface to the centre. Suppose that after Jupiter were formed it were condensed, that might otherwise explain its velocity. He showed that, in the case of the planets, the velocity, had it been one-half what it actually was, would have resulted in their having no rotation. This theory was applied to the absence of rotation in the case of our satellite. He showed the probability that the original nebular ring from which the planets were formed may have been of twice the size of their present orbits. The nebular theory, to meet the requirements of the mere mathematician, would have placed all the planets at regular distances, and given them exactly similar motion. But not such was the method of nature.

In the discussion which followed he stated that we have never seen anything of Jupiter or Saturn but the clouds which cover them. He thought that those planets were yet at a white heat, and we simply saw the clouds that are raining down upon them. The present state of the satellites may be a result of their tides, and not the index of their original velocity. Jupiter and Saturn took so large a proportion of all the planet-forming material that the laws impressed upon them may serve best to tell the whole history of the solar system. There may be, however, a rotation of the inner mass of those planets of which we know nothing.

Geology of Southern New Brunswick. By Prof. T. Sterry Hunt.—The recent labours under the Geological Survey of Canada, by Messrs. Bailey Matthew and the author, were sketched. They show south and west of the coal basin various uncrystalline formations, all resting upon ancient crystalline rocks. These latter are by the author regarded as for the most part the equivalents of the Green Mountain and the White Mountain series, or what he calls Hurordan and Montalban. These are penetrated by granites, and associated in one part with Norian rocks, but the presence of Lamentran is somewhat doubtful. While the author recognises thus, at least, four distinct series of pre-Cambrian crystalline rocks in Eastern North America, he does not question the possible existence of yet other series in this region. The analogies offered by the more recent rocks of this region are very suggestive.

On the Possibility that the Sun, while mainly Gaseous, may have a Liquid Crust. By Prof. Charles A. Young.—There can be very little doubt that Secchi and others, who hold that the sun is mainly gaseous, are correct in this: the smallness of density cannot possibly be explained on any other supposition. At the same time the eruptional phenomena which are all the time occurring on the surface, almost compel the supposition that there is a crust of some kind which restrains the imprisoned gases, and through which they force their way in jets with great violence.

Prof. Young suggests that this crust may consist of a more or less continuous sheet of descending rain, not of water, of course, but of the materials whose vapours exist in the solar atmosphere, and whose condensation and combinations are supposed to furnish the solar heat. As this tremendous rain descends, the velocity of the falling drops would be retarded by the resistance of the denser gases underneath; the drops would coalesce until a continuous sheet would be formed; and these sheets would unite and form a sort of bottomless ocean resting upon the compressed vapours beneath, and pierced by innumerable ascending jets and bubbles. It would have an approximately constant depth in thickness, because it would re-evaporate at the bottom nearly as rapidly as it would grow by the descending rains above, though probably the thickness of this sheet would continually increase at some slow rate, and its whole diameter diminish.

Prof. Young added an explanation of the narrow disc fringes seen at the moment of totality in a total eclipse, showing them to be optical interference effects caused by the sudden changes of the temperature of the air at the edge of the shadow. The twinkling of stars is analogous in many respects.

The Existence of Live Mammoths. By Prof. Feuchtwanger.—The discovery of the mammoths in Siberia in the deep gorges of the mountains near the Lena Viner, which was lately published as having been made by a scientific Russian convict, who had five living animals, twelve feet in height and eighteen feet in length, with projecting tusks four feet long, excites some discussion in Europe. I think it worthy of inquiry whether the mammoth of the past tertiary period, discovered during this century in Siberia, near the same river, can have any relation to the convict's discovery. Thousands of these animals have been found buried in the ice, with their well-preserved skins, and thousands of tusks are brought to England to this day for the use of the turner. These are of nearly the same dimensions as those seen by the Russian. The convict has received an unconditional pardon, on the recommendation of scientific men who have investigated his statements and believe them to be true.

Prof. E. S. Morse, of Salem, Mass., read a paper on the subject of *Variations in Wave Lengths.* Prof. Morse first called attention to the interesting discoveries of Lockyer, Huggins, and others in accounting for the displacement of lines in the spectrum in observations of celestial objects. It is well known that when a star is approaching the observer the luminiferous waves emitted by it are crowded together, and on the contrary are separated when the star is receding.

Mr. Morse brought forward an instrument by which this phenomenon in the case of light may be easily and plainly illustrated before a large audience. The instrument consists of a tank filled with water and set on wheels. On the top of this is a compartment containing compressed air. From one end of the tank a pipe protrudes, which is moved up and down at a fixed rate by simple clockwork. When the cock is opened, allowing the water to escape from the pipe, the stream assumes a sinuous line, which may be shown, if brilliantly lighted, across a large audience hall. This undulatory stream, when the tank is at rest, illustrates a luminiferous wave from a stationary source. To exhibit the shortening or lengthening of the waves of light by the approach or recession of the luminiferous body, Mr. Morse simply moves the apparatus rapidly back and forth on the table. As the apparatus moves with the direction of the stream its undulations are crowded together, and the waves are consequently shortened. On the other hand, when the motion of the apparatus is in an opposite direction, the waves are proportionably lengthened. The advantage of this illustration is that it exhibits precisely what takes place in the luminiferous waves approaching or receding from the observer of celestial bodies, producing the displacement of spectrum lines.

Concerning Hyalonema. By Dr. Samuel Lockwood.—The recent deep-sea dredgings have done much toward clearing up the singularly anomalous history of the Japanese glass-rope sponge. Prof. Lockwood, however, thinks that, either from inappreciation or otherwise, the knowledge thus obtained has not been applied to the elucidation of certain mooted points connected with Hyalonema. With regard to the mistakes in representing Hyalonema "wrong end up," my opinion is that the error was led off by the Japanese themselves. The drawings by the native artists represented these curious objects as attached to the sea bottom by the sponge mass, thus making the fascicle to be erect and uppermost. Obtained by the net, or some such means, from the bottom at great depths, it is supposable that the fishermen at Enoserma were entirely ignorant of the matter. Their theory, however, as represented by the native artists, has wrongly represented the Hyalonema. These ropes attached to the sponge and sand are some distance from the main or upper portion encrusted with parasites. After removing portions of the encrusting case from the fascicle, he could not detect any structural evidence that Polython owed anything for food to the object which had given it local support. It, however, "chums" with the sponge for a purpose of its own. Prof. Lockwood thinks that it draws sustenance from the fishing process of its radiating tentacles.

Both Polyps and sponge provide for themselves. In his view the zoöphyte is what we must call a compensal, and could not exist without that sort of support from Hyalonema which the oak affords the vine: and Hyalonema, too, is a compensal; for how long would it endure without the support of Polython? The stem, without this support, would not be able to hold itself erect. Other varieties are supported by stems consisting of sheaves of short spicules, bound together by bony cement. These have and need no supporting Polython. He combated the view that Hyalonema was sunk in the mud up to the neck, arguing that the polyps surrounding the stem could not so live; that it could not use its tentacles to obtain food, and that the position of the egg-cases of the deep-sea shark, the oldest egg being attached, and the most recent at the bottom, sustained this view. Some account was then given of the material structure of the encoating Polython. The essayist spoke of the deep-sea sharks off Setubol, making that place their feeding-ground, because of the facility afforded them to secure these egg-cases by the abundance of the Hyalonemas there.

The Co-efficient of Safety in Navigation : an attempt to ascertain within what Limits a Ship can be located at Sea by Astronomical Observations. By Prof. Wm. A. Rogers.—This was an attempt to ascertain mathematically the average number of miles that a ship may be out of her reckoning. It was a paper of length, indicating long and careful research. It stated that in the case of British vessels there is a continual increase in the proportion of wrecks, as shown in the following :—

British vessels.	Wrecks.
Inc. 1858 over 1848......38 per cent.	Inc. 1862 over 1852......59 per cent.
Inc. 1868 over 1858......44 per cent.	Inc. 1867 over 1857......57 per cent.

For 1869 we have a decrease in the number of vessels of 4 per cent., and an increase in the number of wrecks of 21 per cent. The confidence in reckoning by instruments had increased the danger. He considered separately (1) wrecks by causes beyond control ; (2) wrecks to obtain insurance ; (3) wrecks by deviation of compass ; (4) wrecks by errors of observation. He concluded that 70 per cent. of wrecks were from preventible causes. There are 3·3 times as many insured vessels wrecked as uninsured. The ratio of errors in chronometers wasillustrated in an elaborate series of tables showing that the navigator must expect from this source an error of 3·6 miles, must be on the look-out for one of 11·5, and must not be surprised at one of 21 miles, all on the supposition that he has an average chronometer. One serious source of error is varying temperature during a voyage. The conclusion was that the navigator who assumes that he can get the place of his ship certainly within five miles, or probably within fifteen, exhibits an over-confidence which may lead to his ruin.

There were other papers of interest, by Prof. Elliott, on International Coinage ; by Prof. Wheildon, on the Arctic Regions ; by Gen. Barnard, on the Relation of Internal Fluidity to the Precession of the Equinoxes ; by Prof. Hilgard, on Transatlantic Longitudes, and on Meridional Arcs ; by Col. Whittlesy, on Rivers in the Mississippi Valley ; by Prof. Hunt, on Breaks in the American Palæozoic Series ; by A. E. Dolbear, on a new method of measuring the velocity of light.

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*MR. HARTNUP ON DETERMINING THE RATES OF CHRONOMETERS**

THE difficulty in predicting the rate of a chronometer for a voyage arises from the imperfect state of the instrument ; and by a well-arranged and carefully conducted test, these imperfections may be so exhibited as to enable the mariner to avoid the danger which must frequently follow from the neglect of such precautions. The Greenwich mean time is now so easily obtained in most seaports, that there can be no difficulty in ascertaining the daily gain or loss of a chronometer, if the rate so found could be depended on. The communication of time to the port of Liverpool, by the firing of the gun which is placed on the Morpeth Dock Pier Head, has been so successful that the difference between the flash of the gun and 1 P.M. Greenwich mean time has not, on any occasion during the past year, been such as could lead to an error in a ship's longitude to the extent of the width of the Mersey opposite the point on which the gun is placed ; and by observing the flash of the gun on two occasions at an interval of a few days, the rate of a chronometer may be obtained with sufficient accuracy for most practical purposes. The rate so obtained might, however, differ very much from the rate at sea, if the temperature in which the rate was obtained in port differed much from that to which the instrument was exposed on the voyage.

Imperfect thermal adjustment is a defect so well known, that during the past thirty years the attempts made to improve the quality of marine timekeepers have been mainly confined to the compensation balance. The ordinary balance does not perfectly compensate for the change in the elasticity of the balance-spring, caused by change of temperature, and various forms have been given to balances with the view of attaining greater perfection. Balances have, without doubt, been made to compensate for change of elasticity in the spring throughout long ranges of temperature, but there is evidently some objection to their general adoption for the merchant navy. It is possible that the thinness of the laminæ, and peculiarity in the construction of balances which are made with the view of removing the defect above named, may render them less permanent in their action, and more liable to injury in the hands of a less skilful mechanic than the original maker ; but however this may be, the ordinary balance seems to be almost universally used in the merchant navy. This having been found to be the case, about four years ago arrangements were made at the New Observatory for the trial of chronometers in three definite temperatures with the view of showing the amount of change in their rates due to error of thermal adjustment, and more than one thousand marine timekeepers have now been tested in 55°, 70°, and 85° of Fahrenheit. From a careful examination of the records of these tests there appears to be a definite temperature peculiar to each chronometer in which the instrument goes faster than in any other temperature, and as the number of degrees above or below this temperature of maximum gaining rate increases the chronometer loses in a rapidly increasing ratio. If we assume this law of variation to be that the change of rate is directly as the square of the number of degrees from the maximum gaining rate, the rates calculated on that assumption are found sensibly to agree with those obtained from observation ; therefore, if we have the rate from observation for each of three definite temperatures, as given in my last two Reports, we can find, by computation, the correction for error of thermal adjustment due to any other temperature. In order to do this it is necessary to find—

T . . the temperature in which the chronometer has its maximum gaining rate,
R . . the rate at the temperature T, and
C . . the factor, or constant number, which multiplied by the square of any given number of degrees from T shows the amount of loss for that number of degrees.

The following example shows the method of calculating C, T, and R from the observed rates in 55°, 70°, and 85° :—

Chronometer, No. 727.

$$\text{Rate in } 55^\circ = -\overset{s}{2{\cdot}92} \ldots r \qquad r - r' = -1{\cdot}04 \ldots d$$
$$\text{,,} \quad 70 = -1{\cdot}88 \ldots r' \qquad r' - r'' = +1{\cdot}25 \ldots d'$$
$$\text{,,} \quad 85 = -3{\cdot}13 \ldots r''$$
$$d - d' = -2{\cdot}29$$
$$d + d' = +0{\cdot}21$$
$$C = \frac{2(d-d')}{30^2} = \frac{-4{\cdot}58}{900} = -0{\cdot}00509$$
$$T - 70 = \frac{d+d'}{C \times 60} = \frac{+0{\cdot}21}{-0{\cdot}3054} = -0{\cdot}69$$
$$T = 70 - 0{\cdot}69 = 69{\cdot}31$$
$$R = r' - (T - 70)\frac{d+d'}{60} = -1{\cdot}88 + 0{\cdot}69 \times 0{\cdot}0035 = -1{\cdot}878$$

From the preceding Examples

	Mean Daily Rate in 55°	in 70°	in 85°	C.	T.	R.
	s	s	s			
No. 727	−2·92	−1·88	−3·13	−0·00509	69·31	−1·88

Let N = any number of degrees from T, then the Rate at $T \pm N = R + C \times N^2$.

Required the Rate of No. 727 at 40°
Here $N = 29{\cdot}31$ and $N^2 = 859{\cdot}08$

Therefore the Rate at 40° = − 1·88 + (− 0·00509 × 859·08) = − 6·25.

The values of C and T remain the same for long periods ; as a rule, they do not sensibly change so long as the adjustments are not altered, and the instrument remains in good condition ; but R is more changeable, and should be redetermined on all favourable occasions. To find the change in R the rate must be first carefully found in some definite temperature. Suppose, for example, that at some subsequent time the rate of No. 727 was found to be — 2·13, instead of — 3·13, in 85°, then the rate at T would be — 0·88 instead of — 1·88 ; but it might not be convenient to obtain the rate in either of the temperatures in which the rates are given in the test, and then it may be found as follows :—Suppose the rate has been found to be — 1·55 in 81·5, then the rate must be computed for 81·5, on the assumption that R has not changed, and the difference between the rate observed and the rate computed will be the correction to be applied to R.

The computation is as follows :—81·5 — 69·3 or N = 12·2 and $12{\cdot}2^2 = 148{\cdot}84$.

* Extracted from the Report of the Astronomer to the Marine Committee, Mersey Docks and Harbour Board, for the year 1872.

Therefore, the rate at 81·5 = − 1.88 + (−0·00509 × 148·84 = −2·64.

Observed rate in 81·5 = −1·55. Computed rate in 81·5 = −2·64. The losing rate at T must therefore be diminished by 1·09, making the newly found R = −0·79 instead of − 1·88.

For any chronometer which has been allowed to remain at the Observatory for a period of five weeks the certificate of test issued with the instrument contains the necessary data for calculating the correction due to imperfect thermal adjustment.

THE WHITWORTH SCHOLARSHIPS

THE following Memorandum on the Whitworth Scholarships, prepared by Sir Joseph Whitworth, has been approved by the Lords of the Committee of Council on Education, South Kensington :—

1. The experience of the past competitions for my scholarships has proved to me the necessity of establishing rules which shall insure that the holders of scholarships shall devote themselves to the studies and practice necessary for mechanical engineering during the tenure of the scholarships.

2. To effect this I propose to the Lords of the Committee of Council on Education that as soon as possible, *i.e.* in the competition of 1875, every candidate for a scholarship should produce a certificate that he has worked in a mechanical engineer's shop, or in the drawing office of a mechanical engineer's shop, for two years consecutively. In 1874 six months' consecutive work only in the engineer's shop will be required. The candidate must be under 22 years of age.

3. The candidate for the scholarship will be examined in the appointed sciences ; in smith's work, turning, filing, and fitting, pattern making and moulding, as already established, and the same marks will be awarded as at present.

4. In 1875 and the following years each holder of a scholarship appointed under these new rules will be required to produce satisfactory evidence at the termination of every year that he has made proper advances in the sciences and practice of mechanical engineering by coming up for an examination similar to that which is prescribed for the competition both in theory and practice.

5. The scholarships may be held for three years, but may be withdrawn at the end of each year if the scholar has not made satisfactory progress.

6. The number of scholarships in the competition of 1874 will be reduced from ten to six. Each scholarship will be of a fixed annual value of 100*l.*, together with an additional annual sum determined by the results of the progress made in the preceding year.

7. At the end of each year's tenure of the scholarship, the scholars appointed under these new rules will, as before stated, be examined in theory and in practice in the same manner as in the competition for the scholarships. On the results of this examination the following payments, in addition to the 100*l.* before mentioned, will be made among each year's set or batch of scholars :—To the scholar who does best in the examination, 100*l.* ; to the second, 60*l.* ; to the third, 50*l.* ; to the fourth, 40*l.* ; to the fifth, 30*l.* ; and to the sixth, 20*l.* ; provided that each scholar has made such a progress as is satisfactory to the Department of Science and Art, which will determine if the sum named, or any other sum, shall be awarded.

8. At the expiration of the three years' tenure of the scholarships under these new regulations a further sum of 300*l.* will be awarded in sums of 200*l.* and 100*l.* to the two scholars of each year's set or batch who have done best during their tenure of scholarship.

In this way it will be possible for the best of the scholars at the end of his period of tenure of the scholarship to have obtained 800*l.*, and the others in proportion.

9. The prizes under paragraph 7 will be awarded according to the total number of marks obtained by the students in practice and theory in the examination at the end of the year. The prizes under paragraph 8 will be awarded by adding together the marks obtained by the students at the end of each of the three years.

SCIENTIFIC SERIALS

THE current number of the *Zoologist* commences with a notice by the editor, of Mr. Lloyd's "Official Handbook to the Crystal Palace Aquarium." In an interesting historical sketch of the growth of aquaria, he divides its development during the last forty years into three eras, the earliest being the instructive, the second the poetic and fashionable, and the present the commercial. The early development of the aquarium is then entered into, the work done by Bowerbank, Daubeny, and Warington being fully described. This is followed by a review of Mr. T. J. Moggridge's work on Harvest-idea that these insects do accumulate seeds in store-houses for winter consumption is correct, contrary to the assertions of Kirby, Latreille, and other high authorities. What is very peculiar is that these seeds scarcely ever show any tendency to gering Ants and Trapdoor Spiders, in which the author, from a careful and painstaking series of excellent observations on the habits of ants, which are described in detail, shows that the old minate, though under apparently very favourable circumstances. —Mr. Cornish notes the occurrence of the following fish at Penzance :—The Black Fish (*Centrolophus pompilus*), the Solenette (*Monochirus linguatulus*), the Braize (*Pagrus vulgaris*), Bloch's Gurnard (*Trigla blochii*), and the Torpedo (*Raia torpedo*). —Mr. F. H. Balkwill, in reply to a critical note which appeared in this journal (NATURE, July 24, p: 252) on a paper by him in the *Zoologist* for July last, objects to his remarks being thrown into the general form ; the fact that the forms and arrangements of teeth in vertebrates is practically infinite, being assumed by him. But that such is very far from being the case will be agreed to by all zoologists ; the types and arrangements of teeth being extremely few in comparison to what they might be. The argument does not require, as Mr. Balkwill thinks, the proof of the statement that the teeth of the wombat, dog, &c., should be of low type and simple development, which they are not ; and he may be assured that all "genuine Darwinists" are of opinion that when two not distant types of animal life are in a position to occupy new and separate regions, the fact that their food can only be obtained from two sources, namely, animal and vegetable tissues, invariably leads to their divergence in two directions only, that is, towards a carnivorous and a herbivorous conformation. Therefore the non-placental type, on occupying Australasia, as well as the placentalia in the rest of the world, have differentiated into flesh-eaters and vegetable-eaters, each having developed, by natural selection, organs suitable for procuring their accustomed diet. It is not therefore to be wondered at that these organs should present many points of similarity in the two main divisions of the Mammalia.

BARON VON MALTZAN gives in the second number of the *Zeitschrift für Ethnologie* for 1873, an account of his travels in Arabia, and points out the various causes which have opposed the advance of our knowledge of its interior. Amongst these religion has acted as the most powerful obstacle, the exclusiveness of the Islam faith having, in fact, so effectually closed the country to modern research, that there are still many spots of which nothing is known beyond what Ptolemy was able to tell us. Baron von Maltzan selected the most southern extremity of the peninsula, which is as yet a *tabula rasa* on our maps, for the scene of his explorations. He draws attention to the artistic skill exhibited by these people in statuary and carving, before they fell under the rule of their Mahomedan conquerors from Central Arabia, when all their earlier civilisation was rudely checked and their language superseded, while they were then also first driven to adopt a monadic mode of life. In spite, however, of amalgamation with central Arabian elements, the population of South Arabia still admits of division into two distinct peoples, the Sabäer and the Himyarites, the former of whom have light yellow skins, while the latter, whose name he derives from *Hamr*, red, are so dark-skinned as to be genrally classed amongst the black races. Baron Maltzan observed a curious physical character in the family of the Himyarite rulers of the Fodli, or Ozmani-State, many of whom, both males and females, had six fingers and six toes on both hands and feet. This peculiarity is looked upon by the people at large as a special mark of blue blood, and prized accordingly by the possessors. It would seem that the practice of forming consanguineous marriages, which prevails in the Fodli, as in other ruling houses, may of itself explain, as a mere case of hereditary recurrence, the appearance of this physiological character in numerous and remote members of the family. The author concludes his paper with an appeal to men of Science to turn their attention to a region which is at once so little known and so rich in materials of interest for physiologists, ethnologists, and geographers.—Herr von Martens, in a critique on Prof. Strobel's paper on the appearance of *Unio* shells in the pile-dwellings of Upper Italy and in the Paraderos Patagonians, draws attention to the diversity of opinion to which the occurrence of this bivalve has given rise, Dr. Boni deducing from it the theory that the Emilian Terremare are the sites of human habitations on artificially constructed water basins, whilst Dr. Coppi regards them as the remains of sacrificial or other slaughter places. Dr. von Martens has ascertained by personal observation that the Paraderos of Patagonia resemble in very many respects the Danish Kjökkenmöddings. It is worthy of note in reference to this subject that shells of the Adriatic form (*Aporrhais pes pelicani* and *Venus verrucosa*) occur in the Moravian pile-dwellings near Olmütz, while Mediterranean shells (*Cypræa pyram* and *hirida*) have been found on the Dordogne. These facts, which afford incontrovertible evidence of the extension of commerce in prehistoric ages, are corroborated by the appearance of Red Sea if not Indian Ocean forms of shells, as *Eburna spirata* in a Mariera at Reggio, and of *Cypræa pantherina*, in the Allemannic tumuli of Würtemberg. It has been suggested by Dr. E. Friedel that the *Unio pictorum* L., and the *Alasmodonta compressa*, which are so abundant in Italian Lacustrine deposits, may be connected with the presence of domestic swine, as these bivalves constitute in the present day a very important element in the food of these animals in the poorer districts of the Oder and the Brandenburg Mark.—In conclusion we would draw attention to a curious paper read by Herr von Meyer before the Anthropological Society of Berlin on the origin of "Right and Left," and the causes which have led mankind to give the preference to one over the other, in using the hands and feet. The superior estimation of right over left is shown alike in the most ancient forms of Egyptian sculpture, in Jewish ordinances, in Hellenic poetry, and in language generally, whether of Turanian, Scythic, or Aryan origin. In these tongues the right hand is synonymous with what is good, straight, and right, while the left is identical with what is awkward, evil and abnormal. The author attempted to explain the universally diffused preference for the right hand on the ground of instinctive religious veneration in primæval man, who raised the right hand in adoration as he traced the course of the sun from its rising to its setting, while Prof. Virchow was inclined to refer it to a primary physical principle of the human organisation. The subject gave rise to an animated discussion in the Society, and led to the consideration of several questions of interest to the student of ethnology.

Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden. Oct.—Dec. 1872.—The principal paper in this number is one by M. Ackermann, giving a comprehensive account of recent deep-sea researches.—Dr. Hoffmann furnishes a critique of Zöllner's work on comets; and among the shorter notices will be found information on Phylloxera, the physical features, climate, and products of Venezuela, silkworm-cultivation, the Zoological Garden at Dresden, and other topics.—The succeeding number (Jan.—Mar. 1873) consists, in great part, of zoological lists.—M. Rostock enumerating the Neuroptera of Saxony, and Dr. Köhler the Gasteropoda and Conchifera of Schneeberg.—In the botanical section, M. Wilhelmi gives a list of plants found on the Murray river in Australia.—M. von Kiesenwetter communicates a paper on the history of zoology to the time of Linnæus, being chiefly an abstract of Carus's work on the subject in a voluminous "History of the Science in Germany," now in course of publication.

THE *American Journal of Science and Arts*, Sept. 1873.—In a fifth paper on some results of the earth's contraction from cooling, Prof. Dana treats of the formation of continental plateaux and oceanic depressions, thus concluding the reconsideration of the views he brought out in 1847. Besides the admission of a solid nucleus and the present partial union of the crust to the nucleus, these views have been modified in some points connected with mountain-making and metamorphism, in accordance with ideas developed by Le Conte and Mallet, and the results of personal study. The author gives a valuable summary of his progress.—Prof. O. Rood has a paper on the residual or secondary spectra which Brewster studied, and which are obtained when white light is passed through two prisms of different substances, so arranged as to compensate each other for colour. The Professor has obtained a large dispersion in such spectra by using as one of the constituents the spectrum furnished by oil of cassia, bisulphide of carbon, or flint glass, the other being the *normal spectrum* from a diffraction grating. Some curious experiments with these are described.—A paper on the explorations last year, by the Snake River Division of the U.S. Geological Survey of the Territories, is furnished by Prof. Bradley; and another geological paper, by Mr. Washburn, treats of the Bosphorus region. There are also notes on the Corundum of North Carolina, Georgia, and Montana; on minerals found at the Tidley Foster Iron Mines, New York; on an apparatus for rapid filtrations; and on the discovery of a new double star β Delphini.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 1.—M. Bertrand in the chair.—The following papers were read:—On the Aurora Borealis, by M. Faye. The author's paper related to Donati's late memoir on the same subject, in which he suggests that the passage of electro-magnetic currents from the sun to the planets is the cause of this phenomenon. M. Faye, on the other hand, deprecated the introduction of such a theory, and suggested that the effect of gravity as an agent in producing these effects may at least be probable. He suggested that motions such as are observed in the tails of comets might occur in the upper regions of our atmosphere, *i.e.* that excessively attenuated air might be constantly rushing from the side of the earth turned towards the sun to that turned from it, and that this motion might cause incandescence of the air, visible at the poles as auroræ.—On the Carpellary Theory as regards the *Amygdalaceæ*, by M. A. Trécul.—Gnomonic projection, &c., of a portion of the Sahara, by M. A. Pomel.—Study of the metallic veins of Cornwall; structure of the rich veins, and their relation to the stratigraphical arrangement of the country, by M. Moissenet.—On the Siemens coil, by M. A. Pellerin.—Observations of Planet 133 and of Borrelly's comet, by M. Stephan.—On the changes of form of Comet IV., 1873, and on its spectrum, by MM. G. Rayet and André. The comet has developed a tail and become brighter; it has no nucleus. Its spectrum at first consisted of three bands, one between D and E, another very close to *b*, and a third beyond F. After the tail had developed the same bands appeared, but they were larger and brighter and accompanied by a faint continuous spectrum.—On the form of the Martial seas as compared with the terrestrial oceans, by M. Stan. Meunier. The author considers that the long narrow straits on Mars are an additional proof of its greater age as compared with the earth. Taking the soundings of the Atlantic, he observed that if its level were reduced 4,000 metres (by absorption), it would then present a similar aspect to the Martial seas.

BOOKS RECEIVED

ENGLISH.—The Sea and its Wonders: Hartwig (Longmans & Co.).—Centrifugal Force and Gravitation: John Harris (Trübner & Co.).—Quantitative Chemical Analysis: Thorpe (Longmans & Co.).—What a House should be: William Bardwell (Dean).—The Convolutions of the Human Brain: Ecker (Smith, Elder & Co.).—Scripture Manual (Murby).—Mechanics: Skertchley (Murby).—Report of Freshwater Fish and Fisheries of India and Burmah: Surg.-Maj. Francis Day, Government of Calcutta.

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