THURSDAY, JULY 20, 1871

THE NEWCASTLE-UPON-TYNE COLLEGE OF PHYSICAL SCIENCE

WE have apprised our readers from time to time of the progress of the arrangements for the proposed College of Physical Science in Newcastle. In our report of the meeting held on the 25th of March, at which the scheme was first publicly broached, we expressed an estimate of the administrative ability of the committee appointed to carry into effect the resolutions then adopted, which events have amply justified. A second public meeting was held in Newcastle on Saturday last to receive at the hands of the executive an account of their labours, and the Report now before us shows the energy which has been brought to bear upon a complicated and laborious task. Few who read the announcement of the first meeting—probably few even of those who were there present—supposed that the ship, of which the lines were then but talked of, could be ready at the expiration of little more than three months to launch and make her trial voyage.

We need not reprint the whole Report, as portions of it are merely the official announcement of arrangements already made public; but its contents may be briefly summarised. The Committee state that—

"At their first meeting it was found that the scheme had so far interested the general body of the inhabitants of the district, that very much more support would be forthcoming than was sufficient to provide for the six years' experiment; and as the University of Durham intimated that the promised aid (1,000*l.* per annum) would be made permanent if a capital sum could be obtained sufficient to secure the continuance of the support from the district, it was considered advisable to appeal to the public for 30,000*l.* This sum was mentioned not as being completely adequate to meet the expenses of a collegiate institution, but as providing sufficient funds for the commencement of such an undertaking."

Towards this fund 21,460*l.* has been obtained, together with three subscriptions each of 100*l.* a year, and a hope is expressed that within the next few months between 30,000*l.* and 40,000*l.* may be raised.

The election of Professors in Mathematics, Geology, Chemistry, and Experimental Physics, and the determination of the Committee to open the College in October are announced. It is recommended that the lecture fees should be such as may secure a large attendance of students and it is suggested that five guineas yearly for each course, and one guinea entrance, would be suitable to this end.

It had been agreed by the committee to propose the following Constitution. The governing bodies of the College to be:—firstly, the *Governors*, secondly, the *Council*.

1. The *Governors*; to be limited at first to forty-seven, of whom nine are to be *ex-officio* members of the body. Of the remaining thirty-eight, three are to be north-country Members of Parliament, and two Professors in the College. Nine are to be elected by subscribers to the fund, and the rest in different proportions by the Chapter of Durham, the Senate, the Convocation, the Municipal Councils of twelve northern towns, and the Scientific Societies of the district. Of the elected members one-third are to vacate their seats every two years, but are to be eligible for re-election.

2. The *Council*, which is proposed to "consist of fifteen members, including a Chairman, of whom five shall be members of the Chapter, Senate, or Convocation of the University of Durham; and of these one shall be the Warden, and of the other four at least two shall be members of the Senate; but all members of the Council shall be elected out of and by the Governors themselves. They shall be elected for five years, one-third of the number resigning their places every three years, but being re-eligible. They shall in all cases retain their appointment for two years."

This Council is to meet monthly, and to transact all the ordinary business of the College, arranging the periods of study and fees of the students, and to assume the general direction of affairs.

To a committee of this body. with which all the Professors shall be combined, is entrusted the more strictly academical administration and discipline, including the number and direction of the Professors' Lectures, subject to the general control of the Council.

In conclusion, it is proposed "that while steps are being taken to draw up the necessary documents and to proceed with the election of the Council in the terms of the Constitution, the government of the College shall remain in the hands of the Executive Committee, who sha act as the Council for the period of one year.

"At the invitation of the North of England Institute o Mining and Mechanical Engineers, the Literary and Philosophical Society, the Medical College, and the Natural History Society, occupying a group of buildings in a good situation already dedicated to scientific purposes, a suitable suite of rooms for the lectures, private rooms for the professors, laboratories, and offices, has been secured, which will be sufficient for the accommodation of the College for some years, when it is hoped the success of the Institution will be so secured that a sum of money sufficient to build separate and suitable accommodation will be easily procured."

It is almost needless to say that this report was well received and unanimously adopted, and that the meeting willingly accorded to the Executive Committee the proposed continuation of their powers for a year. In the discussion some remarks were made by gentlemen taking an active part in the labours of the Committee, which are worthy of note.

Mr. Lowthian Bell alluded to the present available accommodation for the College as insufficient, except for temporary purposes, and suggested the probable necessity for building at any rate new laboratories. He also bade the meeting to regard the four professorships already established as but a commencement, there being many other departments of Physical Science which must ere long be provided for, and made special allusion to the claims of Biology to representation.

It is true that another member of the committee with very pronounced political views, suggested that a chair of Political Economy should be the next subject adopted, but as he did not attempt to show the position of Political Economy in his scheme of physical science, it was scarcely thought necessary to argue the point.

In far wider spirit were the remarks of a subsequent speaker, who looked hopefully forward to the time when

success in teaching physical science should enable them to enlarge the basis of the Institution, so as to include all the higher branches of a liberal education.

We have, on more than one occasion, advocated the addition of a Biological chair to the four already agreed upon, for it has seemed to us an anomaly that a School of Physical, or, to use the correlative term, Natural Science, should be without teaching in general Natural History, especially in a locality in which excellent facilities exist. But there is another view which has been prominently in our mind. If the College were intended to be a mere mining and engineering school, established to enable engineers and coal-viewers to educate their pupils with less labour and cost, its claim upon the general public would be small. It is due to the public that Science for its own sake,—Science with less direct reference to considerations of pounds, shillings, and pence, should be recognised; and in no way could this be so readily done, under existing circumstances, as by the establishment of a Chair in Biology.

We readily admit the pre-eminent importance of the subjects selected to commence with, and as willingly record our confidence that the Executive Committee will approach this as all other subjects with the single desire to do what is right.

Finally, we would make one comment on the attitude of Durham University. For many years past it has been regarded as almost hopeless to expect any active assistance in educational matters from that hitherto somewhat sleepy body. But with the new Dean seems to commence a new *régime*, and facts appear to bear out the testimony of many of the speakers at Saturday's meeting, that in all arrangements in connection with the new College, the University authorities have shown the widest liberality and unselfishness.

The vast importance of schools of this sort, and the prospect of a movement with a similar object in the West Riding of Yorkshire, renders needless any apology for reviewing with some detail this last addition to our scientific institutions.

PERCY'S METALLURGY OF LEAD

The Metallurgy of Lead, including Desilverisation and Cupellation. By John Percy, M.D., F.R.S. (London: J. Murray, 1870.)

THE preparation of metallic lead from its several ores, amongst which galena stands foremost, presents to us processes and circumstances which, though generally simple, are amongst the most interesting and delicate in the whole range of productive metallurgy. It is therefore with even more expectation than attached to his former volumes on Copper, Zinc, and Iron that we opened Dr. Percy's present volume; and, in finding a copious and well-arranged compilation, we have not been disappointed, although we might have anticipated something more of original research.

It would, indeed, be improbable, with the great power of obtaining information directly from manufacturers necessarily belonging to the influence and position of a Professor at the Government School of Mines, that the result should be any other. Accordingly, the reader who desires to obtain a distinct and tolerably detailed though "bird's-eye" view of all the various forms of commercial metallurgy of lead (in humbler phrase of lead smelting) will here find a classified survey of it as practised in Great Britain, all over Europe, and in North America, with some notices of attempts made in South America. Of the very ancient lead-smelting processes of Asia, probably the earliest practised on a large scale in the world, and still believed to be in use in China and Japan, we do not find a word. Of recent methods in use in Japan there is a brief notice from Mr. Pumpelly at p. 384, and in China at p. 479.

The first one hundred pages are occupied with the physical and chemical properties of lead viewed from the metallurgical stand-point, one which we cannot but think is always essentially misty and unsatisfactory. The physics and chemistry of any metal ought to be the same to everybody, and it seems to us ought to be fully and accurately known before ever the student opens a metallurgical book. If that be admissible, then metallurgy proper has its limits advantageously defined and narrowed, and its treatises ought to be then divided into two distinct classes—the one like the small octavo volume of Rammelsberg (that most elegant and classic work, now several years published, but yet as true and valuable in almost every page as when it was wet from the press), which teaches the *principles* of metallurgy, that is to say, the principles of those reactions which occur in the established and fully-adopted processes of commercial metallurgy, without going into any details as to apparatus, furnaces, or criticism, as to whether this or that method or construction of plant be better or worse. The other, consisting *not* of any attempt to aggregate in one volume the details of manufacturing apparatus, of trying to tell all about the minutiæ, of all the diversities of all the commercial metallurgies in the world—which, we are compelled to say, is impossible within even the very diffuse limits taken by Dr. Percy—will best consist, we think, of *monographs*, such as those of M. Grüner, in the *Annales des Mines* of a year or two back, on this subject of lead. Each one of these monographs, with the necessary plates of illustration, should really, and in a genuinely practical way, exhaust one single national or special system of smelting of lead, or of some one other metal.

Such has been the plan almost universally adopted in Germany and France, and with results at once far more comprehensive, clear, and exact, than are practicable from the hand of any one man, however able, or in any volume though bulky, illustrated only by woodcuts however excellent, and those of Dr. Percy's present volume are remarkably clear and good.

For the practical and exhaustive description, in fact, of any single smelting process largely in commercial use, an atlas of folio copper plates, forming a volume in itself, is indispensable. The result of the contrary view of the metallurgist's descriptive task, is inevitably that want of balance, and yet incompleteness here and there, which characterise all these metallurgic volumes of Dr. Percy. Thus, for we feel bound to give an example to sustain our criticism, in his volume on Iron, Dr. Percy goes into the question of blowing machines, blast cylinders, and the like—a thing really as foreign to the metallurgy of iron as the theory and practice of building chimney stalks would be to that of lead;

and Dr. Pole, it appears, wrote for him the rather jejune algebraic investigation of the principles of such machines, which, when we come to examine it, we find is merely what we may find in any elementary book on pneumatics; and owing to the omission of all the *structural conditions producing loss of effect* in blowing machines, exists, in fact, as a mere parade of useless symbols, of no value to the constructor or the purchaser or the user of such apparatus.

Now we are wholly unable to see the necessity for thus cumbering with a needlessly hooked-on subject a book on Iron Metallurgy at all; but if otherwise, then it should have been gone into thoroughly, and in a way to be of real value to the constructor. To have done this, however, would have required some fifty pages or more, so that a far better mode, in our judgment, would have been to have simply confined the point to a reference to the great monographs which exist on this special subject, both theoretic and practical. Neither Dr. Percy nor Dr. Pole seems to have been aware of the fact that a quite exhaustive investigation of the theory of blowing machines (omitting none of the conditions of practice) and of high merit, was published as long ago as 1805, by Herr J. Baader, Counsellor of Mines of the Kingdom of Bavaria, and which was specially and by the authority of Napoleon I. translated into French and published in the *Annales des Mines* in 1809. There may be such a thing as apparent completeness, which yet is only the piling together of incongruity or of incompleteness.

But this want of the sense of balance and of relative importance is not confined to such collateral subjects of practice. Dr. Percy, in the volume here noticed, devotes nine pages to the physical properties of lead, in commencing, and of these we find four (under the head of Resistance to Pressure) are occupied with details of Coriolé's fruitless attempts in 1829 to construct weighing machines, whose indications were to be derived from the compression suffered by known volumes and forms of lead pieces—a subject as indirect and foreign to the physical properties as it is far away from the metallurgy of lead.

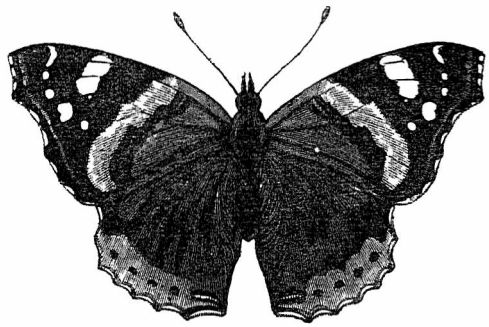
One statement made in this part of the volume is undoubtedly incorrect, where it is said, "by hammering lead becomes harder, but acquires its original softness by annealing." The actual fact is, that lead cannot be made harder by hammering, for its annealing temperature is so low (that of every metal being a function of its fusing point), and it suffers so large a deformation by reason of its softness when hammered, that enough heat is evolved by internal work to cause the metal to anneal itself,—in other words, never to become harder. This has been fully ascertained, and the fact has even been taken practical advantage of by those engaged in "drawing lead pipe" by the older methods, who are well aware that a hard pinch at first or rapid reduction in diameter in passing through the holes of the draw-plate, heating the lead, enables it to be drawn into finished pipe with a less total expenditure of power than if drawn slowly and with so gradual a reduction in diameter as that the lead should remain always nearly cold. Were the lead hardened here by a compression quite the same in effect as hammering, the very reverse must be the case. This volume comprises a very good account of the Pattinson process for separation of silver, and also of Parkes's zinc process. What can have induced Dr. Percy (who is, we believe, fond of scholarship) to employ such barbarous compounds as 'lithargefication," "desilverisation," and "decopperisation," in place of "disargentation," "decuperation"? What would be thought of "desugarification" as a substitute for "desaccharisation"?—but these are matters of taste and no more.

The chapter on the ores of lead and that on the assay of lead ores are amongst the very best in the volume, which is beautifully printed with the clearest of type and paper, and with good indices. There are nine pages near the end devoted to poisoning by lead, which, though certainly not the metallurgy of lead, may prove of some use to those employing work-people in lead smelting or manufacturing operations; though we think here, perhaps, the wisest instructions might have been simply, "send the patient to the doctor." We have little confidence in amateur or improvised medicine on the part of "laymen," in such cases as lead-poisoning, at any rate. On the whole, though, as we have had to point out, this work of Dr. Percy's is not free from faults, it is, we think, in several ways the best of all those on Metallurgy which have appeared from under his pen, and in the collection and discussion of a vast array of facts is a noble volume, the very best that yet exists in English on its subject.

NEWMAN'S BRITISH BUTTERFLIES

An Illustrated Natural History of British Butterflies. By Edward Newman, F.L.S., F.Z.S., &c. 8vo. (London: W. Tweedie, 1871.)

THE British Butterflies form a small but striking group of insects, and hence not only are they as a general rule the first objects on which the collecting spirit of the

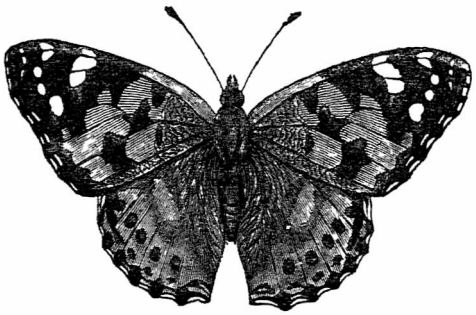


RED ADMIRAL (*Pyrameis Atalanta*). Upper side.

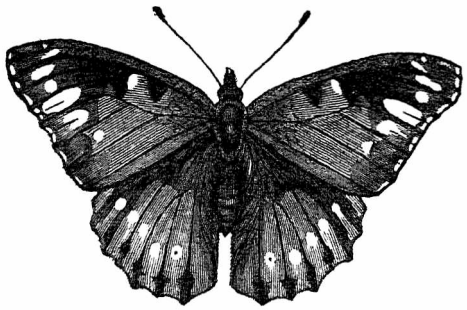
Under side.

young entomologist is exerted, but they also offer one of the best means of commencing the study of entomology. Thus they are easily collected and preserved, their appear-

ance is pleasing and often beautiful, their characters are generally very clear and distinct, so that the discrimination of the species is by no means difficult, and their



PAINTED LADY (*Pyrameis Cardui*).



Painted Lady. Var. 1.

Natural History is easily studied; whilst the small number of the species renders it an easy matter for the beginner to procure in a season or two by far the greater proportion of the known forms.

Although there are already many books treating specially of the British butterflies, some of them expensive, and others so cheap as to come within the reach of every one, we cannot blame Mr. Newman for adding one more to the number, especially as his work is distinguished by the great prominence given in it to the Natural History of the species. Upon this subject, as also upon the distribution of the species in Britain, Mr. Newman has long been publishing details from his own observations and those of other naturalists, in his periodicals the "Zoologist" and "Entomologist;" and the whole of the information thus accumulated is here summarised and supplemented with observations derived from other sources. Another useful feature in the present work is the insertion of notices, and frequently of figures of the more important varieties of each species, which will often relieve the young student from a state of puzzled suspense in the determination of his specimens. The classification adopted is founded, in its broad outlines, upon the preparatory states of the insects, but it leads pretty nearly to the same results as the system more generally followed.

The illustrations are very numerous, including figures of all the species and of both sexes when there is any difference either in the upper or lower surface. They are all woodcuts, and are generally well executed, as may be seen from the examples which we are enabled to give.

W. S. DALLAS

OUR BOOK SHELF

The Western Chronicle of Science. Edited by J. H. Collins, F.G.S., Secretary to the Royal Cornwall Polytechnic Society. January to June, 1871. (Falmouth. Pp. 96.)

WE are glad to afford space for a short notice of this cheap scientific journal, which, although specially intended for the benefit of the mining population of Cornwall and West Devon, deserves a wide circulation in all our mining districts. Each monthly number contains one or two original articles, either on general subjects, as "The Practical Value of Scientific Knowledge," or giving descriptions of various forms of machinery, followed by notices of books, and a monthly chronicle of science. From one of the editorial articles on "The Practical Value of Scientific Knowledge," we learn that a good stoker may effect an annual saving of nearly 35*l.* per annum over a bad one, and that it is a common Cornish habit to hang heavy jackets, great coats, &c., on the lever of the safety valve of engines devoid of a pressure guage; while the farmers, with the view of giving their ground two good things at once, mix lime with their guano some days before spreading the manure. A very remarkable natural-history statement is made by Mr. Williams, of Hayle, in his paper on "Scientific Nursing." "I have (he says) in my possession a double chick, the produce of an egg laid by a barn-door fowl one half being the natural species, the other half composed of the sparrow-hawk!" Until this remarkable chick appears *in propriâ personâ* at the office of NATURE, or, at all events sends us its photograph, we must, with much regret, decline to accept the fact.

Medizinische Jahrbücher. Herausgegeben von der K. K. Gesellschaft der Aerzte, redigirt von S. Stricker. Jahrgang, 1871, Heft I. und II. Mit 26 Holzschnitten und 2 lithographirten Tafeln. (Vienna: Braumüller; London: Williams and Norgate, 1871.)

THE two parts before us are the continuation, in a new form, of an old and valuable periodical, and, under Prof. Stricker's able editorship, its tendency, instead of being, as heretofore, chiefly clinical, will be so far modified as to embrace all the results obtained in the physiological laboratory. A glance at the table of contents is sufficient to prove the truth of this statement. Thus putting aside the first paper by Prof. Stricker, entitled "Pathology and Clinical Observation;" the rest, nine in number, are nearly all devoted to the results of microscopic research. Thus, Dr. Genersich contributes a paper on the Serous Canals of the Cornea; Dr. Heiberg one on the Regeneration of the Corneal Epithelium; Dr. Güterbock one on Inflammation of Tendons; Dr. G. F. Yeo one on the Structure of Inflamed Lymphatic Glands; Dr. Lang one on the First Stages of Inflammation in Bone; Dr. Albert and Dr. Stricker one on Surgical Fever, and the latter author another on the nature of the Poison of Pus, and so on. The journal leads off with a good start, and if it continues as it has commenced, will probably take up a leading position. We notice one or two of the papers that appear to be of general interest. H. P.

LETTERS TO THE EDITOR

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

Cotteau's "Echinides de la Sarthe"

A NOTICE of Cotteau et Triger's *Echinides de la Sarthe* in a recent number of NATURE (June 15, p. 120) is likely to convey a false impression of the accuracy of M. Cotteau, and throws considerable doubt on the value of his work. It is not often that French scientific men are as conscientious as he is in the examination of authentic types. There is hardly a collection of fossil Echini which M. Cotteau has not examined; and his

thorough acquaintance with all that has been written on his subject, as well as his intimate correspondence with the principal echinologists, is a sufficient guarantee that no important memoir (such as Wright's monograph) could have escaped him. Anyone who will take the trouble of turning to Cotteau's work (p. 111) will find, under *Pseudodiadema hemisphæricum*, a notice of Dr. Wright's figure of the same species (so much superior, with many others, to Cotteau's?) and a reference to his description. Nor is this an isolated case. Throughout the work M. Cotteau discusses and criticises more or less the results of this very monograph, said to have been overlooked by him. The mistake Cotteau is accused of making of assigning to Desor instead of Agassiz the specific name of *Pseudodiadema hemisphæricum* is entirely unfounded. Referring again to p. 111., we find, as a synonym, *Diadema hemisphæricum* Agass. M. Cotteau, like many continental and American writers, does not interpret the notation of species as is required by the laws of the British Association, but for that reason he should not be accused of committing mistakes which his own writings show him not to have committed. M. Cotteau, in common with others, looks upon nomenclature simply as a matter of registration; and when M. Desor transfers to *Pseudodiadema* the *Diadema hemisphæricum* Agass., M. Cotteau writes, therefore, *Pseudodiadema hemisphæricum* Desor, and not Agassiz; he may be wrong, according to the principles of the writer in NATURE, but he has not, either in this instance or in the other cases alluded to, committed a mistake through ignorance of the subject. A. AGASSIZ

Mr. Howorth on Darwinism

MR. HOWORTH sneers at "Survival of the Fittest" as an "identical expression" which "might have suggested itself even to a child," an axiom, in short, of which the truth cannot be disputed. This is satisfactory; but it is strange that he did not apply this axiom to his own theory, and see how they agreed together. He would probably admit, as another discovery "that might have suggested itself to a child," that *as a rule* the entire offspring of each animal or plant, except the one or two necessary to replace the parents, die before they produce offspring (this has never been denied since I put it prominently forward thirteen years ago). He would further admit, I have little doubt, that a great majority of animals and plants produce during their lifetime from ten to a thousand offspring, so that fifty will be a low average, but the exact number is of no importance. Forty-nine. therefore, of every fifty individuals born, die before reaching maturity; the fiftieth survives because it is "best fitted to survive," because it has conquered in the struggle for existence. Will Mr. Howorth also admit as self-evident, that this one survivor in fifty is healthy, vigorous, and well nourished, not sickly, weak, or half-starved? If he maintains that it is the latter, I shall ask him to prove it; if the former, then what becomes of his theory as an argument against Natural Selection? For, admitting as a possibility that his theory of the greater fecundity of the weak, &c., is true, how are these weak or sickly parents to provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves? The one in fifty who survives to leave descendants will inevitably be the strong and healthy offspring of strong and healthy parents; the forty-nine who die will comprise the weaker and less healthy offspring of weak and sickly parents; so that, as Mr. Darwin and myself have long ago shown, the number of offspring produced is, *in most cases*, the least important of the factors in determining the continuance of a species.

I have thought it better to go thus into the heart of the question, rather than defend myself from the charge of dogmatism, for stating as a fact that the most vigorous plants and animals are the most fertile. I repeat the statement, however, referring to Mr. Darwin's observations, and especially to those in which he *demonstrates by experiment* that cross-breeding produces the most vigorous and luxuriant plants, which again produce by far the largest quantity of seed. The facts that wild animals and plants are, as a rule, healthy and vigorous, that the head of the herd is the strongest bull, and that weak and sickly carnivora are rarely found because they must inevitably starve to death, sufficiently refute Mr. Howorth's theory as against Natural Selection. If he can point to any district upon the earth where the animals and plants are in a state of chronic debility, disease, and starvation, I may admit that there his theory holds good; but such a district has not yet come under my observation, or, as far as I am aware of, been recorded by any traveller.

I still maintain (Prof. Jowett's authority notwithstanding) that the phrase "Persistence of the Stronger" does not truly represent "Natural Selection" or the struggle for existence;" and, though it may often be true, is not the whole truth. The arguments of Mr. Howorth from the history of savages will, I think, not have much weight, if we may take as an example his putting together as cause and effect the extinction of the Hottentots and their now obtaining enough to eat. ALFRED R. WALLACE

MR. ALFRED WALLACE directs attention to the gross error of supposing that "the struggle for existence means the persistence of the stronger," and correctly stigmatises this view of Mr. Howorth's "a pure misrepresentation."

It is, as Mr. Wallace remarks, very curious and even ludicrous, after all that has been said and written upon the matter, that anyone should fail to recognise the advantages to their possessor of "obscure colours," "cunning," "nauseousness," "bad odour," and other qualities superior to strength alone. The creature having these properties, at last brought to perfection through the operation of natural selection, acting through countless generations, will assuredly have the advantage in the battle of life over its less fortunate neighbours. It will survive in the struggle for existence. Having survived, is it not better that it should at once teach the world the law of its survival, and proclaim itself the fittest to survive, than that it should remain silent until those whom it has destroyed may rise from the dead and admit that their doom was deserved because they were not fit to live? LIONEL S. BEALE

MR. HOWORTH, it seems to me, has not chosen a very favourable time for so strongly maintaining the truth of Mr. Doubleday's theory, seeing that the recent census has shown that the population of England has increased not only with an increment absolutely greater than that shown by any previous census, but also—and this is still more important—with an increase proportionally greater than during the last decade. Yet never, surely, has luxury been so prevalent among us as during these last ten years. The evidence thus afforded will perhaps be deemed more conclusive than the argument of Mr. J. S. Mill, who invites those who may be inclined to accept Mr. Doubleday's opinions "to look through a volume of the Peerage, and observe the enormous families almost universal in that class; or call to mind the large families of the English clergy, and generally of the middle classes of England" ("Principles of Political Economy," bk. I, ch. x., note). Mr. Howorth, however, states that "the classes among us who teem with children are not the well-to-do and the comfortable." If this statement were absolutely true, it would be of little service to Mr. Howorth, since it is in the classes referred to that prudential restraint acts with the greatest force, and the effects of this restraint, both direct and indirect, would have to be taken into account before his conclusion could be admitted. He further asserts that "a state of debility of the population induces fertility," since "where mortality is the greatest there is much the greatest fecundity." That births should be most numerous where the mortality is greatest, requires for its explanation no hypothesis respecting the fertilising power of debility. "The fact," says Malthus, "may be accounted for without resorting to so strange a supposition as that the fruitfulness of women should vary inversely as their health. . . . When a great mortality takes place, a proportional number of births immediately ensues, owing both to the greater number of yearly marriages from the increased demand for labour, and the greater fecundity of each marriage from being contracted at an earlier, and naturally more prolific, age" (vol. i., pp. 472, 473, 5th edit.). Man's reproductive power is always in civilised life more or less checked, and ready to be more or less exercised in proportion to the lessening by death of the restraining pressure.

THOMAS TYLER

MR. WALLACE, in replying to Mr. Howorth's objections to the theory of Natural Selection, points out that that gentleman first misrepresents Darwinism, and that having done so he does not employ the distorted doctrine as premisses to a further con-

clusion. But the second part of the criticism is not quite just. Mr. Howorth, after stating the Darwinism theory, introduces us to an order of facts which is at variance with that theory as apprehended by him; and not only does he do so, but he places an interpretation upon these facts which is utterly irreconcilable with the Darwinian theory as understood by its most able expositors. It is true that Mr. Howorth does not bring his interpretation of the facts he adduces and the theory of natural selection into such juxtaposition as to show their mutual contradiction; but a little consideration will enable Mr. Wallace to supply the missing links, and to see that in any generous construction of Mr. Howorth's letter, the real questions at issue are the correctness of the facts he adduces and the validity of the generalisation he makes from these facts. My object in writing is to direct Mr. Howorth's attention to Mr. Herbert Spencer's profound discussion of this subject, as it appears to have escaped his notice. This is the more surprising, since, on p. 111, vol ii. of "The Variation of Animals and Plants under Domestication," and to which Mr. Darwin refers him, there is the following marginal note:—"Since this MS. has been sent to press, a full discussion on the present subject has appeared in Mr. Herbert Spencer's 'Principles of Biology' vol ii., 1867, p. 457, *et seq.*" He is a bold man who undertakes to enlighten the public on a subject which Mr. Spencer has fully discussed, without first ascertaining what view that profound and original thinker adopts; and most certainly a fresh writer coming into the field ought to take up the discussion where an author of such eminence has left it. If Mr. Howorth will look at Mr. Herbert Spencer's "Principles of Biology," he will find in sections 78 and 79, an explanation of the process adopted by gardeners of cutting the roots, and "ringing" the bark of fruit trees. Section 355 explains the fact that fatness is often accompanied by barrenness. In a footnote at p. 483, vol. ii., he will find Mr. Doubleday's doctrine specially noticed, and the fallacies upon which it is based exposed; while in the chapters "On the Laws of Multiplication," vol. ii., p. 391, *et seq.*, he will find the whole subject treated with a fulness and exhaustiveness which leaves little to be desired. Mr. Howorth will notice that Mr. Spencer does not deny Mr. Doubleday's facts, but that he places upon them an interpretation which brings them into harmony with the general theory of evolution, and with the special part of organic evolution which constitutes the Darwinian theory.

Newchurch, July 17 JAMES ROSS

I HOPE you will allow me a few lines to reply to Mr. Howorth.

I had thought Mr. Doubleday's essay was among the things of the past. There can be no question that his conclusions are not the conclusions of accomplished naturalists like Mr. Wallace, whose assertions are certainly as good, if not far better, than those of Mr. Doubleday.

Quoting Mr. Chadwick, Mr. Howorth again puts cause for effect. There can be no doubt that the death rate increases in a crowded country *pari passu* with the crowding, and that the crowding is the result of fertility. It by no means follows that the crowding produces fertility.

There is one way in which poverty and overcrowding tend to increase the birth rate. Many of the children of the poor die during the first few months of life, and hence the mother, being relieved of her offspring, ceases to secrete milk, and soon again falls pregnant. It is the death of very young children in crowded districts which so largely increases the mortality, and this, as we have seen, may tend to increase the birth rate.

The large percentage of deaths in early life amongst the ill-nourished and weakly renders these less likely to bear children than the strong. With regard to the large families of the poor so often quoted, I have grave doubts of the fact. I have for many years seen hundreds of poor families every year in the exercise of my profession of surgeon, and although I know many instances of ten or fifteen children having been born of one mother, in the majority not more than two or three reached adult age, and hence these produced no offspring in the second generation.

The most remarkably prolific woman who has come under my notice has had twenty-two children in twenty years, and she is still continuing to present her husband with blessings. She is one of the fattest women I know.

Amongst the rich and the well-to-do it is no uncommon thing for eight or ten children to grow to man's and woman's estate and to rear families. I know as many well-to-do persons with large families as poor people, and the living percentage is far greater in the former.

I am not aware that consumptive patients are so extremely prone to breed as Mr. Howorth thinks, certainly their children do not live to produce a second generation as a rule.

Examples of fecundity and barrenness amongst wild tribes are not much to the purpose, because there are so many disturbing influences. To take, however, Mr. Howorth's case, the Red Indian feeds ill enough and is thin enough, yet he is not fertile. The backwoodsman, with his vegetable diet, would be far more likely to grow fat, and is certainly far better fed and far stronger than the Indian, yet he is more fertile than the Indian, although by no means fertile. He has many hardships to undergo.

With regard to the Patagonian women and their belief that bleeding produces fertility, evidence is wanting as to the truth of their belief. We know many wide-spread beliefs are erroneous, for instance, most savages believe in rain-makers.

In conclusion, Mr. Howorth thinks that wild animals in captivity are sterile from over-feeding. If he will try and make them fertile by starving them, I think I may assert positively he will fail. Hence, I suspect, we must look for a deeper cause of barrenness in them. B. T. LOWNE

99, Guilford Street

Recent Neologisms

IN using the word Mr. Ingleby objects to as hideous, I was not aware that I was coining a new one. If so, it was quite unconsciously on my part; but a word was wanted to express the property of being prolific, and if the choice lies between "prolificness" and "prolificacity," as I think it does, I am inclined to believe that the former will survive, as being the shorter, the easier to pronounce, and perhaps the less hideous, even though it may not be constructed on the best etymological principles. "Fertility" and "fecundity," which are often used, do not quite answer the purpose, although the latter has very nearly the same meaning. Our language must and will grow; and its growth will be determined by convenience rather than by grammatical rules. ALFRED R. WALLACE

DR. INGLEBY is in error as to the recent introduction of "survival," "impolicy," and "prolificness." All these words will be found in Chalmers's abridgment of "Todd's Johnson" (1820); the first with a reference to Sir George Buck, the second with one to Bishop Horsley, and the third with one to Scott (not Sir Walter). "Indiscipline" does not occur, but "indisciplinable" does, Hales being cited as the authority.

R. G.

IN his excellent custom of "registering the first appearance of new words and new phrases," Dr. C. M. Ingleby is surely very careless or superficial. He quotes "survival" as a new word introduced, he thinks, by Darwin. I have been familiar with it as long as I remember, and my life of careful observation has exceeded a quarter of a century. "Impolicy" is equally familiar, having had currency at least twenty years before the Franco-Prussian war, to which Dr. Ingleby accredits it. He will find both words, as well as "indiscipline," in "Webster's Dictionary," edition 1852, and probably much earlier on careful search. "To telegram" is clearly a vulgarism, rarely heard I imagine, and never seen in print. G. W. S.

Fertilisation of the Bee Orchis

MR. DARWIN, in his "Fertilisation of Orchids," states his belief that the Bee Orchis presents a physiological difference from all other British orchids, and is habitually self-fertilised. I had, yesterday, an opportunity of observing a number of these plants in one of its abundant localities in Surrey, and at a time when fertilisation must have been completed. In every plant almost all the capsules were considerably swollen, and were loaded with apparently fertilised ovules. In most of the withered flowers, the remains of the pollinia were still visible in the position described by Mr. Darwin, hanging down before the entrance to the nectary, in immediate proximity to the stigma, and rendering it almost impossible to believe that the flower had ever been

entered by any insect of considerable size, which must inevitably have carried away the pollinia with it. The fact that the Bee Orchis, the most "imitative" of all our native plants, is never visited by insects, is a very suggestive one. If, as might well have been assumed, the object of the "mimicry" is the attraction of bees, the device appears to have signally failed.

London, July 17 ALFRED W. BENNETT

Saturn's Rings

As Lieut. Davies has thought it necessary to refer to your remarks about the satellite theory of Saturn's rings—and in so doing has named my work upon Saturn (which you had *only* referred to without naming) it may be as well for me to mention, that I nowhere in that work claim the theory as mine—and that, whenever I have seen it referred to as mine, I have as publicly as possible disclaimed all title to it.

Permit me to add, that, whatever opinion we form of Lieut. Davies's views, he deserves our thanks for bringing out a treatise so full of work, from cover to cover, as his "Meteoric Theories." Such examples are a good deal needed in these days.

8, Wellington Villas, Brighton RICHARD A. PROCTOR

Ocean Currents

I FIND that Dr. Carpenter does not consider his experimen probative. Judging from the air of triumph with which, both in his lectures and writings, he has announced its success, I had certainly imagined that he did. But if not probative, what is it? Dr. Carpenter says it is only intended to be illustrative. What does it illustrate? It does not illustrate any currents formed in the ocean by differences of temperature; for it does not illustrate the differences of temperature to which he attributes these currents. In his letter in NATURE of July 6, he proposes an unwieldy modification of his former well-known experiment, but which still, I would submit, in no way avoids the difficulty to which I have called attention. He describes a strong freezing mixture applied to the surface through one-tenth of the length of a trough half a mile long, and heat applied to the surface also through one-tenth of the length, measured from the other end: between the cold and the hot surface there is, then, an intervening space of four-tenths of a mile, or 2,112 feet; that is to say, there is a thermometric gradient of about 50° in 2,000 feet, or 1° in forty feet. This is small enough, and we may perhaps doubt whether such a gradient could give rise to any appreciable movement; but it is 15,000 times greater than the gradient observed in the ocean, which is about 1° in 100 nautical miles; and any movement shown by an experiment which, in its details, bears no reasonable proportion to the reality, cannot be accepted as an illustration of a movement in the ocean.

Mr. Proctor, in the same way, speaks of his proposed experiment as an illustration; and, in the same way, I would say that the distortion produced by magnifying 6,000,000 times that particular detail on which he wishes to lay an emphasis, precludes our accepting it as an illustration at all. Mr. Proctor says that it is intended specially to throw light on the easterly and westerly movements; it is surely unnecessary for me to point out to him that any easterly or westerly movements, as illustrated in a cylinder such as he describes, revolving continuously and uniformly, are direct consequents of the outward or inward movement due to the differences of temperature, and are, therefore, in the strictest sense, dependent on the thermometric gradient. If, with a thermometric gradient of $\frac{1}{6000000}$ of a degree in one foot, and with an angular velocity of 360° in 24 hours, Mr. Proctor succeeds in showing any appreciable movement, I and (I think I may add) many other readers of NATURE will be glad to learn the result. But this is, after all, the point I raised in my last letter (NATURE, June 29), and which Mr. Proctor considers would require many columns for its full discussion. I do not myself see that there is any room for discussion at all; and any difference of opinion that may exist can only be met by experimental demonstration.

Dr. Carpenter appears to wish to support his theory on "authority," and especially on that of the recent letter of Sir John Herschel. This is a point on which I touch with great reluctance; but I would point out, in the first place, that "authority" in matters of science carries very little weight; and, secondly, that Sir John Herschel, in the letter referred to, merely admits what he and everyone else have all along admitted, that hot water and cold, in juxtaposition, will establish a circulation. It was not for him, in a letter of private courtesy, to enter again on a discussion of the infinitesimal nature of the gradients—a discussion which he had already worked out very fully in his "Physical Geography."

But, leaving this consideration on one side, I maintain that, at the present time, the *onus probandi* rests with the supporters of the temperature theory. Its opponents have offered what is, at any rate, a rational, consistent, and tolerably complete explanation of all the known ocean currents; and they say, in so many words, that the explanation offered, in accordance with the theory of temperature and specific gravity, is neither complete, nor consistent with itself or with geographical observation. The theoretical objection of the infinitesimal nature of the thermometric gradients and of the differences of specific gravity, which has, indeed, formed the subject of these letters, is not one which I was inclined to put forward in any prominent degree. I preferred, and still prefer, to base my objection on the utter discrepancy between fact as observed, and fact as described by Captain Maury and Dr. Carpenter, in accordance with their theory.

I have elsewhere dwelt on this at great length, and do not intend to go over the same ground here, even if you were willing to afford me the space to do so; but this discrepancy, which actually and very markedly exists, does call attention to the thermometric gradients in the ocean; and when we find the same discrepancy between observation and description in the case of aërial currents, it leads to the conclusion that the infinitesimal nature of the thermometric gradients is as sound an objection to the temperature theory of atmospheric circulation, as it is to the temperature theory of oceanic circulation. I refer here to the last sentence but one of Mr. Proctor's letter. The last sentence, I must confess, I do not understand. I do not see what effects solar light can produce, or even be supposed to produce, on the depths of ocean, to which no light penetrates; still less do I see how to integrate them.

J. K. LAUGHTON

Formation of Flints

IN your report of the discussion that followed the reading of my paper on Flint, before the Geologists' Association on June 2nd, Prof. Morris is said to have asserted that the views I suggested were first propounded by Dr. Brown of Edinburgh. I think the Professor must have been slightly misrepresented in this; at all events I must most decidedly decline to be coupled with Dr. Brown, or to allow myself to be associated with his very remarkable statements. These may be found in the Trans. Roy. Soc. Edinb., vol. XV. He asserts that carbon is transmutable into silicon; at p. 229 he says, "Carbon and silicon are isomeric bodies, and that the former element may be converted into a substance presenting all the properties of the latter." At p. 244, "3·04 grains of silicic acid were extracted from 5 grains of paracyanide of iron;" at p. 245, "5·4 grains of silicic acid were procured from 30 grains of the ferrocyanide of potassium," and "there were obtained 9,334 grains of silica from 3,240 grains of ferrocyanide, although some of the product was lost in two of the operations." The view I advocated as explanatory of the formation of flints was the *substitution* of silicon for carbon, not a transmutation, and I distinctly showed the source from which the silicon was derived. Dr. Brown's statements are so extraordinary that I could scarcely believe them serious. I find, however, in the same volume of the "Transactions" that they were most patiently examined and confuted by Dr. George Wilson and Mr. John Crombie Brown, and they say, "We tried the greater number of Dr. Brown's processes, and rejected them one after another without pursuing their investigation further, on finding they would not yield quantitative proofs of the conversion of carbon into silicon. The limited time, which from various circumstances we could devote to the subject, obliged us to follow this course; and the confident expectation we entertained till a recent period that each new process would supply what the rejected ones had failed to afford, led us to neglect noting many particulars of our early trials which otherwise we should have recorded. . . . In conclusion, we need scarcely say that we have been unable to supply any proof of the transmutability of carbon into silicon."

I have one more objection to make to the report. I did not say that flints were merely silicified sponges. I believe that such is the case with *some* flints, but certainly not with all. I hope you will find space for this rectification of manifest errors.

M. HAWKINS JOHNSON

379, Euston Road, July 11

Affinities of the Sponges

MR. PARFITT seems to think that Mr. Carter has done Prof. Greene some injustice, because he has not referred to him as an original investigator of the Sponges, and he bases his opinion on the figures in Prof. Greene's "Manual of Protozoa," urging that the only difference between the forms figured by Carter in 1871, and those by Greene in 1859, is "the want of the funnel-shaped mouth, which seemed to have escaped the observation of Prof. Greene, probably owing to want of definition in the instrument used in the investigations." Allow me to point out that there is no pretence of originality in Prof. Greene's useful manual, that the figures alluded to are acknowledged (p. 85) to be copied from those illustrating the papers by Williamson and Dobie, and to express the opinion that much further research is necessary before the affinities of the sponges can be regarded as satisfactorily settled. When that day comes there is little doubt that a good deal of what is now guess work will require to be completely sponged out. W.

Sun-Spot

WHILE watching the sun set over the hills to the west of Halifax, on the evening of July 17, my attention was called to an intensely black spot upon its southern hemisphere, almost vertically below the centre of the disc, which was visible to the naked eye. I may add that the evening was fine, but a thin mist was rising from the valleys, and that it was about five minutes before the sun touched the horizon that the spot was first seen. THOMAS PERKINS

EDOUARD RENÉ CLAPARÈDE

AT the early age of thirty-nine, one of the most skilful, laborious, and honoured of European zoologists has been lost to Science in the person of Edouard Claparède. For the last three years his health has been such that his friends continually feared to receive the sad news which has at length come from Italy. In spite of a complication of pulmonary and cardiac disease, his indomitable spirit had kept the man at work to the last. Having taken up his residence in Italy for the benefit of his health, he produced during the last three years of his life a series of memoirs, so richly illustrated, and exhibiting such astonishing industry, that one would have fancied a man in full health and vigour was unequal to such abundant fertility. He once remarked to a friend, who expressed surprise that a man in his precarious state of health should work so hard, that he felt work was the only thing which kept him to life, if he left off working he should die at once. Claparède was a native of Switzerland, and a pupil of that great master of great zoologists, Johannes Müller. He could write French and German equally well, and consequently some of his researches are to be found published in the German periodicals, others in French in the Transactions of the Academy of Geneva. His earliest published work of large size is the "Recherches sur les Infusoires," which he produced in conjunction with his friend Lachmann, who unhappily died before it was completed. Though now to a great extent superseded by the later researches of Stein, Zenker, Cohn, and others, working with more accurate instruments, this treatise is one of classical importance, and forms the foundation of modern views on the Infusoria. Not long after the publication of this work, Claparède came to England, where he made the acquaintance of Dr. Carpenter, and spent a portion of the summer in his company in the Hebrides, working with the microscope, chiefly at the lower worms and annelids. From this expedition resulted a quarto publication, illustrated with plates (published by the Geneva Academy), giving accounts of new marine worms allied to the Earth-worms, and many valuable observations on the Turbellarian worms. In conjunction with Dr. Carpenter, he also published some observations on the curious *Tomopteris onisciformis* in the Linnean Transactions. Attracted by the Limicolous Annelids, Claparède continued his observations on the forms of this group inhabiting the streams around Geneva; and his "Recherches sur les Oligochêtes," also published by the Geneva Academy, furnished zoologists with a very complete account of the anatomical and systematic differentiæ of many of these worms, till then almost entirely neglected and misunderstood. In this work the homology of the segmental organ with the reproductive ducts was demonstrated. The circulation of spiders, which he studied in the transparent young of the genus *Lycosia*, and the development of the freshwater gasteropod, *Neritina fluviatilis*, also about this time furnished occupation for his pen and pencil; and an elaborate work on the development of the Nematods, in which the important questions of the signification of the parts of the egg are discussed, was completed by him. In the collections of miscellaneous observations, always finely illustrated, which he from time to time published, such as "Glanures zootomiques," "Beobachtungen über wirbellosen Thiere," &c.," he recorded observations principally on the Annelids and free-living worms, which he made from year to year on the coasts of Normandy or the shores of the Mediterranean, and many strange forms, paradoxical marine larvæ, and unsuspected annectant genera, are briefly figured and described, which excite the interest of the zoologist, and awaken the desire to know more of them; whilst in other cases new modes of reproduction, new anatomical details, or physiological observations are related (for Claparède was no narrow zoologist) of rare and little known forms. The great work which he took in hand after his health had compelled him to reside in a warm climate during winter, was the study of the Annelids of the Bay of Naples. Under this title he has left two thick quarto volumes, illustrated by more than fifty coloured plates, consisting of anatomical and enlarged coloured drawings of these beautiful worms. Many new and curious forms were added by one winter's work to the known species of the Annelida; but his work is even more valuable for the anatomical and histological observations which are there recorded, and for the great critical ability displayed in dealing with the perplexing questions of synonymy. M. Claparède appears to have found especial pleasure in doing justice to Delle Chiaje, who preceded him in the investigation of the fauna of the Bay of Naples; whilst he does equal justice to M. de Quatrefages, whose errors in a recently-published "Histoire des Annelées" he does not hesitate repeatedly in the course of his book to expose, at the same time dedicating the first volume of his work to that distinguished French naturalist, and naming many new species in his honour. Whilst this splendid work on the Neapolitan Annelids was in press, M. Claparède also gave to the world some very interesting studies on Acarids (published in German), in which many new facts are detailed, and the Darwinian theory, in the manner of Fritz Müller, is shown to furnish a satisfactory explanation of the modification of dissimilar parts in different genera, to form identical organs. During the same period he also published in the *Zeitschrift für wiss. Zoologie* a memoir on the histology of the Earth-worm, illustrated with nine coloured plates, which is certainly the most minute and careful piece of work which he ever produced. The structure of the nervous system and of the three "riesige Rohren-faden," soon to become very celebrated in zoological circles, are here for the first time fully described; and, indeed, the subject had been so slightly handled before that the whole work abounds with new matter. M. Claparède's last published paper appeared this year in the *Zeitschrift*, and as if to show that he did not intend to abandon himself to the study of one group, consisted of observations on the anatomy and reproduction of some marine polyzoa, illustrated by three coloured plates, drawn with his accustomed facility and grace. He has, we understand, left

behind him ready for publication a large work on the Embryology of Insects, and an immense collection of microscopic preparations, of Annelids of great value. Perhaps the most striking discovery recorded in any of M. Claparède's writings (which should, however, be judged by the accumulated value of their immense number of anatomical observations) is one among those relating to the Annelids of the Bay of Naples. Claparède found that the *Nereis Dumerilii* lays eggs, sexually fertilised, which, on hatching, produce a worm which had been placed in quite a distinct genus (*Heteronereis*), and this worm lays similar true eggs, which produce sometimes a second kind of *Heteronereis*, or at other seasons the original form *Nereis Dumerilii* again. The difference between *Heteronereis* and *Nereis* is very great, and one extending into such details as the form of the setæ of the feet. At present this appears to be the only *real* case of alternation of generations on record, if, by "generations," we understand "sexual generations."

Whilst working so largely as an original observer, M. Claparède occupied himself also in reviewing the labours of others from time to time in the *Archives Suisses* published at Lausanne. Though holding the title of Professor in the Academy of Geneva, we believe he never (certainly not of late years) gave any public lectures on zoology; yet that he was admirably fitted for such work, had he thought fit to devote his time to it, is evident from the admirable style of his writings, especially the reviews and criticisms published in the *Archives Suisses*. His criticism of Mr. Wallace's views on the Descent of Man is known to our readers. Having access to the French world of science as a speaker and writer of the French language, and being thoroughly familiar with German writings and thought, both from education and continued association, M. Claparède appears to have taken an honest delight in every now and then dealing a severe blow at some one or other of the French naturalists who might venture to exhibit superficiality or dishonesty in his field of study. Dujardin is roughly handled in the "Recherches sur les Infusoires;" Rouget also, who appears to have personally resented the correction. Balbiani's researches on the development of the Aphides are made the subject of special investigation by M. Claparède, who, three years since, studied the embryology of a species of aphis at Naples solely with the view of testing some extraordinary statements then recently advanced by the French doctor, and came to the conclusion that they were utterly unfounded, and that M. Balbiani had not done justice to the work of his predecessors, which conclusions he stated in very plain language. The attack on M. de Quatrefages, gracefully made and richly deserved, was perhaps the most entertaining. For M. de Quatrefages, charged to present to the French Academy the work which was dedicated to him, and in which, while his good work was appreciated, his errors were exposed, thought it advisable to reply to some of M. Claparède's criticisms, and displayed some temper, and even hinted that the dedication was objectionable. The sequel to this is to be found in the dedication of the second volume of the "Annélides du Golfe de Naples." It is dedicated to Delle Chiaje. Perhaps, says M. Claparède, were he alive he would object to this dedication; he would see with regret many of his errors pointed out; although so much of his work is here confirmed, human vanity would suggest to him to refuse the dedication of a work, to which, however, posterity considers he is justly entitled. It is, he concludes, easier sometimes to dedicate a book to a dead than to a living man.

The ardent naturalist, the accurate observer, the brilliant artist, the keen critic, the lucid exponent, has ceased his work, but has left a name which may well cheer the most faint-hearted among us—even those who feel to want the physical vigour of their fellows—for it is to be remembered that the works which do most honour to the name of Edouard Claparède were the labours of a dying man.

E. R. L.

ALEXANDER KEITH JOHNSTON, LL.D.

A MEMOIR of Mr. Johnston would be the record of a life laboriously and successfully devoted to the spread and popularisation of a single science. Mr. Johnston's first maps, the result of a walking excursion through the north of Scotland, appeared in 1830, and were issued in a Traveller's Guide-Book. His first large work was the "National Atlas," folio, on which he was assiduously engaged for upwards of five years, having projected and drawn the greater part of the maps (forty-five in number) and written nearly all the names they contain with his own hand. This work went through many editions, and was considered the best of its time.

Having, in the course of his residence in Germany, been much interested in the writings of Ritter, Humboldt, and Berghaus, on Physical Geography, and having learned that Humboldt had expressed a desire to see an English physical atlas constructed in a manner suited to the taste of the British public, and on a scale sufficient to admit of entering fully on the details of physical phenomena, Mr. Johnston visited Germany in 1842, travelling from Hamburgh to Vienna, collecting materials for such a work, and making arrangements for an extensive correspondence.

Previous to the commencement of Keith Johnston's Atlas, Physical Geography was an unknown term in Britain. Hence it was predicted that the work would be a failure, and it required great faith to enable him to persevere in his self-imposed task. He was unfortunate in his first publisher, who was not able to do much with so expensive a work; however, the first edition was sold off, and a second was called for, and published in 1856. The two editions occupied Mr. Johnston ten years of the best period of his life. These writings procured for him, in 1850, a Fellowship in the Royal Society of Edinburgh.

In 1850 appeared the first edition of his "Dictionary of Geography, Descriptive, Physical, Statistical, and Historical," 1 vol. 8vo., on a new plan, embracing numerous facts in the different branches of science not before noticed in similar works.

In 1851 the author constructed a Physical Globe of the earth, thirty inches in diameter, showing in colours its Geology, Hydrography, Meteorology, &c. &c. For this, the first Physical Globe ever drawn, the medal of the Great Exhibition of 1851 was awarded. The globe was not intended to be published.

Between 1851 and 1855, he constructed and published for educational purposes four Atlases, royal 8vo.—namely, General, Classical, Physical, and Astronomical, and one Elementary Atlas, small 4to. All these have been improved, some of them re-engraved, and as many as from five to thirty editions of each have been published, of 1,000 copies each. In 1852 he prepared an Atlas of Military Geography to accompany Alison's "History of Europe," 1 vol. 4to. This work was most favourably reviewed, and commended by military men.

In 1855 was commenced the "Royal Atlas of Modern Geography," on which the author brought to bear the geographical experience gained during the labours of a quarter of a century.

In 1865 the University of Edinburgh conferred on him the Honorary Degree of Doctor of Laws.

During the last four years Mr. Keith Johnston was engaged in the production and increase of a complete series of geographic works for schools.

From the brief notice which appeared in our last number, it will be seen that Mr. Johnston may be said to have died in harness, his active labours having been carried on till the close of his life.

PAPERS ON IRON AND STEEL

V.—THE BESSEMER PROCESS (*continued*)

IN the previous papers I have described the phenomena presented during the different stages of the blow, and have endeavoured to explain the chemical actions upon which they depend. The next stage, that of adding the molten spiegeleisen to the iron which has been fully acted upon by the blast, also presents some interesting phenomena which have not hitherto been fully examined.

In a paper on "Burnt Iron and Burnt Steel," read before the Chemical Society 6th April last,* I showed that the "burnt iron" of the workman is really what its name implies, viz., iron which has been more or less oxidised throughout its substance, and that "burnt steel" is quite different,—that the presence of combined carbon in sufficient quantity effectually protects iron from oxidation by heat.

These conclusions are strikingly illustrated in the Bessemer process. In spite of the excessively high temperature and the abundant supply of oxygen during the blast producing most violent combustion of the material in the converter, I have found no "burnt iron" during the early or middle stages of the blow. This only appears at quite the latter stages when the carbon is nearly all burnt out. At the termination of the blow, the material left in the converter is burnt iron of a very exaggerated type in all cases where the burning out of the carbon has been carried to its full extent.

Mr. Bessemer failed in his attempts to produce malleable iron by his process, and all subsequent attempts have equally failed, even when the very finest qualities of hæmatite pig-iron have been used. I am not aware that any explanation of this has yet been given, but have no doubt that it depends upon the principle above stated, viz., that some combined carbon is absolutely necessary to preserve the iron from oxidation, and thus, as the carbon is removed, the iron begins to oxidise throughout, and we have an incoherent mixture of iron and particles of oxides, which crushes under the hammer or the rolls, is neither malleable nor ductile.

The degree of rottenness depends upon the extent to which the blow has been carried, and the iron thus produced varies from a quality which simply cracks at the edges when hammered or rolled, to a mass that crushes into granules like a piece of coarse sandstone. If inattention or some hitch in the machinery prevents the immediate turning over of the converter, and the blow is continued a few minutes too long, the amount of oxidation is so considerable that the mass in the converter loses its fluidity, and becomes a spongy and pasty mixture of melted iron and infusible oxide, which is rather troublesome to the manufacturer.

By the simple method described in the paper above-referred to, I have been able at once to detect the presence of entangled particles of oxide in the midst of the iron remaining in the converter at the end of the blow. They are even visible on the fracture of overblown iron.

The presence of this free oxide explains some otherwise inexplicable phenomena which accompany the pouring of the spiegeleisen. A furious ebullition of the molten metal takes place, jets of burning carbonic oxide spurt up violently from all parts of the surface; the converter is filled with the blue flame which pours forth from its mouth, producing the weird illumination I have already described. The outpouring flame so completely occupies the whole dimensions of the mouth of the converter, that no air can possibly enter, and thus all the oxygen required for the combustion which is going on must be derived from the material inside the converter. Some of the carbon of the spiegeleisen is thus burning at the expense of the oxide of the original charge, and this oxide is thereby reduced.

The sole function usually attributed to the spiegeleisen is that of converting the iron into steel; but if the above be correct, it performs, in addition to this, the important service of reducing the free oxide of the rotten burnt iron, and thereby rendering it malleable. We shall now understand why Mr. Bessemer and others have failed to produce malleable iron by directly oxidising the silicon, carbon, &c., of the pig-iron in the converter. It may be asked how then does the puddler remove the carbon from pig iron? My answer is simply that he does it by a far less violent process of oxidation; that towards the end of his work when the iron is "coming to nature," *i.e.*, when the proportion of protecting carbon has become very small, he takes especial precautions by closing the dampers, and otherwise diminishes the rate of oxidation as much as possible, and thus he is able to work down to less than $\frac{1}{10}$ per cent. of carbon without burning his iron.

The more violent oxidising agency of the Bessemer blast demands a greater quantity of carbon for the protection of the iron, and accordingly it is found that about 0·25 per cent. is the minimum limit of carbon which is practically obtainable without sacrifice of malleability. I have determined the carbon of many hundreds of samples of Bessemer steel which has been specially made as "mild" as possible, where it was a primary object to reach the minimum proportion of carbon, and have never found any sound metal to contain less than 0·20 per cent. The usual range of this (which is sometimes called "Bessemer metal" being scarcely steel although not true iron) is from 0·25 to 0·30 per cent. of carbon. I do not here speak of the limits of absolute possibility, but of the practical limits of the process as at present conducted.

I should add that, in the course of subsequent working the proportion of carbon is reduced, but the extent of this reduction is very variable, depending on the number of re-heatings, the amount of surface exposed, and the kind of furnace in which the reheating is conducted. By using a reducing flame the oxidation of the carbon may be wholly prevented, but in the ordinary reheating or mill furnace and in the exposure of rolling, &c., a certain amount of oxidation commonly occurs. Rails and tyres usually contain two or three hundredths per cent. less than the ingots from which they were made, thin plates and sheets lose a larger proportion, even as much as one-tenth per cent. in extreme instances. I have removed the whole of the carbon from the surface of a hard steel plate by exposing it for several days to the low red heat of an annealing furnace.

W. MATTIEU WILLIAMS

* An abstract of this paper will be found in NATURE, April 20, p. 497.

THE CAUSE OF LOW BAROMETER IN THE POLAR REGIONS AND IN THE CENTRAL PART OF CYCLONES

IN none of the treatises on Meteorology or Physical Geography is there to be found any satisfactory explanation of the observed low barometer in the polar regions, or in the centre of a cyclone. Observations show that in the Antarctic region there is a permanent depression of more than one inch below the average height nearer the equator, and in the Arctic region a depression of about half that amount; and also that for several days frequently the barometric pressure of the central part of a cyclone is one or two inches less than that of the exterior part. Mr. Buchan, in his excellent treatise on Meteorology, attributes the low barometer in the polar regions to the effect of the vapour in the atmosphere. If the amount of vapour in the polar regions was greater than in the equatorial, this effect, so far as it would go, would be in the right direction; but just the reverse is the case; for it is well known that the amount of vapour in the warm equatorial region is much in excess of that in the cold polar regions. Attempts have also been made, without success, to account for the depression in cyclones by the effect of centrifugal force.

By whatever cause so great a difference in the barometric pressure in the different regions might be produced, it may be shown from the principles of dynamics that the equilibrium would be restored in a very short time, if there was not some constant force tending to drive the atmosphere from the polar regions towards the equator, or from the centre of the cyclone to the exterior, and to keep it in that position. Such a force may be found in the influence of the earth's rotation. In a paper by the writer in the *Mathematical Monthly* in 1860, published in Cambridge, U.S., a full abstract of which was also published in the January No. of *Silliman's Journal* for 1861, the following very important principle was demonstrated:—In whatever direction a body moves on the surface of the earth, there is a force arising from the influence of the earth's rotation, which tends to deflect the body to the right in the northern hemisphere, and to the left in the southern hemisphere. This force, which is the key to the explanation of many phenomena in connection with the winds and currents of the ocean, does not seem to be understood by meteorologists and writers on physical geography. We see it frequently stated that the drift of rivers and currents of the ocean running north or south always tends to the right in our hemisphere, and that a railroad car running north or south presses to the right; and this is the case. But the same is true, and to exactly the same amount, of a current or of a railroad car running east or west, or in any other direction.

The amount of this deflecting force, when the velocity of the body is small in comparison with that of the earth's rotation, is expressed by $2 \cdot \frac{1}{289} \cdot \frac{v}{n} \cos\theta\, g$; in which v is the lineal velocity of the body relatively to the earth's surface, n that of the earth's rotation at the equator, θ the angle of polar distance, and g the force of gravity. If the velocity is expressed in miles per hour, the expression in round numbers becomes $\frac{v \cos\theta}{150{,}000} g$; that is, for each mile of velocity per hour, the force is $\frac{1}{150{,}000}$ of gravity, multiplied into the cosine of the polar distance. Hence a railroad car on the parallel of 45° north, running in any direction at the rate of forty miles per hour, presses to the right with a force equal to about $\frac{1}{5{,}000}$ part of its weight.

The effect of this deflecting force upon what Mr. Stevenson calls the barometric gradient is easily estimated. Since the strata of equal pressure of the atmosphere, so far as this force is concerned, must be perpendicular to the resultant of this force and gravity, the sine of inclination of any such stratum to the earth's surface must be $\frac{v \cos\theta}{150{,}000}$, and the change in barometric pressure for any given distance is equal to the weight of a column of atmosphere of a height equal to the change of level of the stratum of equal pressure, and of a density equal to that at the earth's surface. The barometric gradient, then, as expressed by Mr. Stevenson, for any distance d expressed in miles is $\frac{v \cos\theta\, d}{5 \times 150{,}000} \times 30$ inches; putting five miles for the height of a homogeneous atmosphere, and thirty inches for the pressure at the earth's surface. Round numbers are used throughout, since it is only the order of the effects we wish to determine, and not their exact amount.

According to all observations, there is a steady and very strong wind blowing all around the earth in the middle and higher latitudes of the southern hemisphere, with a velocity of at least twenty-five or thirty miles per hour at the surface of the ocean, and this is perhaps much greater in the upper strata of the atmosphere. If at the parallel of 50° we suppose the velocity of the wind v to be thirty miles per hour, the preceding expression of the barometric gradient for a distance d of 5° or 350 miles, using the cosine of 40°, is 0·33 inches of mercury. By reference to § 113 of Mr. Buchan's Meteorology, it will be seen that the barometric gradient for that parallel is only 0·28 inches for 5° of latitude, and that this is about the maximum gradient in the southern hemisphere. Hence a velocity less than 30 miles per hour at the surface of the sea, especially if we suppose that it increases in the higher regions, is sufficient to account for this maximum barometric gradient; and, according to observations, 20 or 30 miles per hour for the wind in that region is no unreasonable assumption. The eastward velocity of the wind in the different latitudes being known, and, consequently, the corresponding barometric gradients, the difference of barometric pressure between any parallel near the pole and one toward the equator, is readily obtained by integration. As the wind near the equator is toward the west the deflecting force there is *toward* instead of *from* the pole, and hence the greatest barometric pressure is about the parallel of 30°, and there is a slight depression at the equator. The deflecting force and the consequent depression are small, then, on account of the small value of θ near the equator.

Since there is more land and mountain ranges in the northern than in the southern hemisphere to obstruct the eastward motion of the atmosphere, its velocity is not so great, and consequently the polar depression is much less there than in the southern hemisphere. According to Mr. Buchan the barometric depression in the Arctic regions is much greater in the northern part of both the Atlantic and Pacific oceans, than it is in the same latitudes on the continents. The explanation of this is, that the eastward velocity of the atmosphere over the oceans being much greater than it is on the continents, where it is obstructed more by friction and mountain ranges, the force driving the atmosphere from the poles toward the equator is less, and consequently the barometric pressure is less in the northern part of both oceans than it is on the continents in the same latitudes.

Upon the relative strength of the forces tending to drive the atmosphere from the poles towards the equator, depend the positions of the equatorial and the tropical calm belts. This force being strongest in the southern hemisphere on account of less resistance from friction and mountain ranges, the mean position of the equatorial calm belt is a little north of the equator, and the positions of the others a little farther north than they would otherwise be. The prime motive power also in both hemispheres being the difference of density of the atmosphere between the polar and the equatorial regions, arising from a difference of temperature and of the amount of aqueous vapour, during our summer when this difference is less than the average in the northern hemisphere, and greater in the southern, these calm belts are forced a little north of their mean positions. Of course, just the reverse of this happens during our winter; hence we have an explanation of the annual variations of the positions of these belts.

In the case of cyclones, the atmosphere at the earth's surface being forced in from all sides towards the centre, by the force arising from a difference of density of the atmosphere in the central and exterior parts, it cannot, on account of the deflecting force which has been explained, move toward the centre, without, at the same time, receiving a gyratory motion around that centre. Neither can it have a gyratory motion without also having a motion towards that centre, since in that case there would be no force to overcome the frictions of gyration. Hence, neither the radial theory of Espy, nor the strictly gyrating theory of Reid and others, can be true, though either of them may be approximately so in special cases. But the gyratory part of the motion is not caused by the motion of the atmosphere from the north and south only toward the centre of the cyclone, as stated by Mr. Buchan and others, but equally by the different parts moving in from all sides,

since in whatever direction they move toward the centre there is the same deflecting force, either to the right or the left according to the hemisphere.

The motion of the atmosphere being in a spiral toward and around the centre of the cyclone, the deflecting force depending upon the earth's rotation, at right angles to the direction of motion, being resolved in the directions of the radius of gyration and tangent, the latter overcomes the friction of gyration, and the former causes a pressure from the centre, decreasing the height of the strata of equal pressure in the cyclone, and consequently diminishing the barometric pressure. The barometric gradient of a cyclone is estimated in precisely the same way as in the case of the hemispheres, using for v the lineal velocity of gyration obtained by resolving the real motion into the directions of the tangent of gyration and of the radius. It has been seen that a velocity of 30 miles per hour gives a barometric gradient of $\frac{1}{3}$ of an inch in 350 miles on the parallel of 50. A gyratory velocity therefore of 100 miles per hour would give a barometric gradient of one inch of mercury in about 300 miles. The velocities of gyration being known at all distances from the centre of motion, and consequently the barometric gradients, the difference of barometric pressure between the centre and the exterior, so far as it depends upon the gyratory motion, may be obtained by integration. The effect of the centrifugal force of the gyrations is generally only a very small quantity of a second order, in comparison with the other, and the effect of it is entirely insensible, except in the case of small tornadoes, when the gyrations are very rapid close around the centre.

In all the preceding estimates of the barometric gradient, it should be understood that the results belong merely to the force depending upon the earth's rotation, and to this must be added the part belonging to a difference of density of the atmosphere, which in the case of cyclones increases the gradient, but diminishes it in the case of the hemisphere. For the general motions of each hemisphere form a cyclone, with the pole as a centre; but having the denser instead of the rarer portion of the atmosphere at that centre. Hence the motions in any vertical plane through the centre are reversed, and it becomes what has been called an anti-cyclone.

Cambridge, Mass. WM. FERREL

RECENT MOA REMAINS IN NEW ZEALAND

II.

THE Moa's neck with the integuments attached, the discovery of which was announced in my communication dated April 3, has since then been forwarded to this Museum for examination by Dr. Thompson, and the following particulars may not be without interest to your readers.

The total length of the specimen is 16·5 inches, and includes the first dorsal and last six cervical vertebræ with the integuments and shrivelled tissues enveloping them on the left side. The surfaces of the bones on the right side, where not covered by the integuments, are free from all membranes and other tissues, but are quite perfect and in good preservation, without being in the least degree mineralised.

The margin of the fragment of skin is sharply defined along the dorsal edge, but elsewhere it is soft, easily pulverised, and passes into adipocere.

The circumference of the neck of the bird at the upper part of the specimen appears to have been about 18 inches, and the thickness of the skin about $\frac{2}{16}$ of an inch.

The only indication of the kind of matrix in which it had been imbedded was a fine micaceous sand, which covered every part of the specimen like dust, there being no clay or other adherent matrix. On removing this sand with a soft brush from the skin, it was discovered to be of a dirty red-brown colour, and to form deep tranverse folds, especially towards the upper part. The surface is roughened by elevated conical papillæ, from the apex of some of which springs a slender transparent feather barrel, never longer than half an inch. On the dorsal surface a few of these quills still carry fragments of the webs, some being 2 inches in length. From these it appears that the colour of the feather barbs was chesnut-red, as in Apterix Australis, but that each barrel had two equal plumules to each quill, as in the Emu and Cassowary, and in this respect differed from the Apterix, the feathers of which have not even an accessary plumule. On the other hand the barbs of the webs of the feathers do not seem to be soft and downy towards the base as in the Emu.

From the direction of the stumps of the feathers, it is evident that the portion of the neck which has been preserved is that contained within the trunk of the body, and which, in the natural position, has a downward slope, the conical end of the specimen being where the upward sweep of the neck of the bird commenced, which accounts for the absence of the trachea with its hard bony rings, none of which are found among the soft parts which have been preserved.

The integument was easily removed by dividing the few threads of dried tissue by which it was attached. The shrivelled-up soft parts thus displayed could not be clearly distinguished, but may be grouped as follows:—1. A strong band of ligamentous tissue connecting the spinous processes. 2. Inter-vertebral muscles and ligaments. 3. A sheath diverging from the lower part as if to enclose the thorax. The only bone besides the vertebræ was attached to this sheath by its tip, the other extremity being articulated to the first dorsal.

Respecting the nature of the circumstances to which this remarkable specimen owes its preservation, I can only conjecture that the body of the bird must for a considerable period have lain on its side in water or a swamp, and that the portion immersed was thoroughly macerated, while the exposed parts were desiccated and shrivelled up; and that subsequently the whole remains were embedded in dry sand.

As a fact of some interest connected with the history of the Moa, I should mention that in December last, Archdeacon Williams informed me of the discovery of a series of enormous bird-foot marks on the surface of a layer of sand beneath a bed of alluvium at Poverty Bay. The specimens he collected for me have unfortunately gone astray, but others have been placed in the Museums in Auckland and Napier, and I have just seen a pencil rubbing from the latter, taken by Mr. Cockburn Hood, which leaves no doubt that they are the footprints of a bird like the smaller-sized species of Dinornis, the largest of these footprints being about eight inches in length.

JAMES HECTOR

Colonial Museum, Wellington, New Zealand, May 15

[We exceedingly regret that we are unable to reproduce woodcuts of the beautiful illustrations by which Dr. Hector's article is accompanied.—ED.]

NOTES

WE are glad to learn that our anticipations last week with reference to the Australian observations of the Total Solar Eclipse of December next are being realised. The Royal Society of New South Wales is organising an expedition to Cape Sidmouth, a little south of Cape York. The President of the Royal Society of London has arranged that a few instruments of the newest construction shall be sent out from this country.

IT perhaps is not so generally known as it ought to be that the Emperor of Brazil, now in this country, is an enthusiastic astronomer, and has an appreciation of the value of science which places him in the highest rank among reigning sovereigns. During the last week he has visited the Royal and Mr. Huggins's Observatory, and in a long interview with Mr. Lockyer has discussed the bearings of the recent solar discoveries.

THE *Pall Mall Gazette* states that the Emperor Napoleon is about to visit Mr. R. S. Newall, whose magnificent refractor has already been described in these pages.

THE Royal Commission on Scientific Instruction and the Advancement of Science adjourned on Tuesday last till November. We are informed that the publication of some of the evidence already taken may shortly be expected.

THE first General Meeting of the approaching session of the British Association at Edinburgh will be held on Wednesday, August 2, at 8 P.M., when Prof. Huxley will resign the chair, and Prof. Sir William Thomson will assume the presidency, and deliver an address. On Thursday evening, August 3, at 8.30 P.M., there will be a Discourse by Prof. Abel on some recent Investigations and Applications of Explosive Agents; on Friday evening, August 4, at 8 P.M., a Soirée; on Monday evening, August 7, at 8.30 P.M., a Discourse by Mr. E. B. Tylor on the Relation of Primitive to Modern Civilisation; on Tuesday evening, August 8, at 8 P.M., a Soirée; on Wednesday, August 9, the concluding general meeting will be held at 2.30 P.M.

WE are glad to notice the step recently taken by the Committee of St. Mary's Medical School, in establishing Scholarships in Natural Science, open to public competition. Through the very proper action of the Governors of the Hospital, the share of school fees formerly paid to the charity has been appropriated to the improvement of the school. By this means the Committee has been enabled not only to provide a tutor to assist the students in the practical portion of their work, but also to establish three Scholarships in Natural Science, each of the annual value of 40*l*., and tenable for three years. The first of these, and an annual exhibition of 20*l*., will be awarded by open competitive examination in September next. The tendency of these Scholarships will be to favour what we have so often advocated, the acquisition of a proper amount of scientific knowledge previous to entering upon a regular course of medical study.

THE *Lancet* states that the Council of the College of Surgeons of England have withdrawn their opposition to that portion of the scheme of the College of Physicians which provides for the selection of examiners by a central Board composed of the representatives of the various licensing bodies and universities, and have agreed to give up the power of specially nominating examiners in special subjects. Thus the main difficulty in coming to an agreement upon the question of a conjoint examination has been removed. The Apothecaries' Hall will probably be left out in the cold.

THE number of successful candidates at the recent Matriculation Examination at the University of London was 242, of whom only 30 passed in honours. This shows a larger proportion of failures than on any previous occasion, notwithstanding that the novel practice was introduced of optional questions, only a certain proportion being expected to be answered in some of the papers.

THE Anniversary meeting of the Quekett Microscopical Club will be held on Friday the 28th inst., at 8 P.M., at University College, Gower Street.

THE first number is issued of the "Journal of the Anthropological Institute of Great Britain and Ireland," being the first substantial result of the union of the two old societies, the Ethnological and the Anthropological Societies. We are glad to see this evidence of the concentration of power thus effected.

THE Annual Meeting of the British Horological Institute was held on July 11, Mr. John Jones, vice-president, in the chair. The report of the Council for the past year was read by Mr. Henry Moore, resident secretary. One of the most interesting features of the report was the fact that the Baroness Burdett Coutts has, agreeably to that line of action for which she has been distinguished, volunteered a prize for the best essay on "The Balance Spring and its isochronal Adjustments." The Astronomer Royal, Sir C. Wheatstone, and Mr. James F. Cole will be the judges. The attention of the Lord Mayor, the head of the guilds of the City of London, bodies entrusted with power specially to promote purposes similar to those aimed at by the Institute, has been attracted by the efforts of the Institute, and he has promised to distribute the prizes to the successful students in horological drawing. Lectures were delivered in the past half year by Mr. Perrell, Mr. Herrmann, Mr. Charles Frodsham, and Mr. John Jas. Hall. The following are the chief officers elected for the ensuing year:—President: Mr. Edmund Beckett Denison, LL.D., Q.C. Vice-presidents: Mr. C. I. Klaftenberger, E. D. Johnson, and John Jones. Treasurer: S. Jackson.

THE Government of Bavaria has been long engaged in the publication of a History of Science in Germany. The following volumes have already appeared:—Bluntschli's History of Political Law; Kobell's of Mineralogy; Fraas's of Agriculture; Peschel's of Geography; Lotze's of Æsthetics; Benfey's of Philology; Raumer's of German Philology; Kopp's of Chemistry; and the following are in preparation:—Zeller's of Philosophy; Bursian's of Classic Philology; Bernhardi's of Military Science; Wegele's of History; Stintzing's of Jurisprudence; Karmarsch's of Technology; Gerhard's of Mathematics; Jolly's of Physics; Wolf's of Astronomy; Ewald's of Geology; Hirsch's of Medicine and Physiology; Carus's of Zoology. When may we look for anything of the kind from our enlightened Government?

WITH the July number of the Journal of the Franklin Institute of Philadelphia, Dr. W. H. Wahl becomes sole editor.

WE have received the Catalogue of the Iowa State University for 1870-71. The students are divided into law, medical, normal, and academical departments, the students in all the departments, except that of law, being of both sexes, and some of the instructors being also ladies. The full course of instruction in the academical department occupies five years; during the first three years nearly the same course of study is followed by all the students; during the last two years the course is elective, either literary or scientific. The University is wholly sustained by endowments and state appropriations, the fees even for the medical classes being merely nominal. Good opportunity appears to be afforded for the practical study of natural and physical science, and the "School Laboratory of Physical Science," edited by Dr. G. Hinrichs, the Professor of Physical Science, is published at the University.

Les Mondes prints a list of the new taxes imposed by the French Government on articles of consumption and commerce.

AN additional excursion of the Geologists' Association took place on the 10th and 11th inst. to Warwick and neighbourhood. The sections of the Lower Lias were examined, and the extensive quarries worked for material for hydraulic cement were visited. A special object of interest was the insect-beds occurring in the Lias at Wilmcote, near Stratford-on-Avon.

WE learn from the *Journal de Méd. de l'Ouest*, and *Bull. Génér. de Thér.* that Dr. Weir Mitchell, from observations on the bite of the rattlesnake, and MM. Gicquain and Viaud Grand-Marais, from observations on that of the viper, have arrived at the conclusion that the application of carbolic acid immediately on the receipt of the injury prevents both local and general poisoning. The pure acid however, if applied in too great quantity, is liable to produce sloughing, and even dangerous symptoms; hence it is best used in the proportion of two parts

of acid and one of alcohol. Given internally, or applied to the wound at a late period, it produces no effect. It is believed to act, not by neutralising the poison, but by causing contraction of the small vessels, and thus preventing its absorption.

THE following interesting account was published in *Notes and Queries* of August last year without eliciting any reply. Mr. Alexander Williams writes :—"As the Commissioner for Western Australia of the International Exhibition of 1862, I received from the Colonial Committee at Perth several specimens of native shields. The long narrow form of these implements of defence is common to all the Australian colonies I believe, but I cannot say whether the ornamentation is uniformly the same. But among the Swan River nation it consists of an oblong pattern (following the shape of the shield) composed of border within border, traced in different coloured paint. The late Mr. Christy called my attention to the exact similarity of these shields to those used by the natives of Central Africa—a similarity not only in shape and pattern but actually in the succession of colours in the pattern. How is this to be accounted for? It is possible (and no other theory seems admissible) that it is purely an accidental coincidence. It is perhaps not difficult of belief that the native mind in two races in all respects so utterly distinct should have hit upon the same shape and form of weapon to meet and throw off the common spear. It is even not very surprising that savages unacquainted with 'lines of beauty' should adopt the same crude form of ornamentation, but it is somewhat startling I think that they should have used apparently the same pigments, and very extraordinary as it appears to me that they should have adopted precisely the same succession of colours."

WHILST we have been literally overwhelmed with rain in this country for the last three months, it is interesting to hear that in Tientsin in China there was so little snow in the winter, and hardly any rain has fallen since, that the peasantry are complaining of the want of water, and consequent injury to the crops.

A SEVERE earthquake shock is reported from Brooklyn and Staten Island, New York, on the 19th of last month at about 10 P.M. No great amount of damage was done, and the motion appears vertical rather than horizontal in character.

THE *American Journal of Science* gives a long report of the severe earthquake at Oaha, Hawaian Islands, on February 18 of this year. It commenced at about 11 minutes past 10 P.M., and lasted about a minute. The direction of motion was vertical, with a rocking movement N.E. and S.W. The usual roaring sound preceded the earthquake and was heard far out at sea. No earthquake wave is reported from any quarter, although the earthquake itself seems to have been felt on all the islands more or less severely. No unusual volcanic action is reported. Slight shocks were also felt on the 22nd and 24th of the same month. It should be noticed that a severe earthquake is reported from Chile on the 25th, and shocks were noticed in Peru on the 22nd and 23rd of February.

THE existence of certain plants only in limited districts is one of the most remarkable points of interest in connection with the problem of the distribution of species. Mr. Moggridge, in his valuable "Contributions to the Flora of Mentone," figures a very elegant species of *Leucojum*, of which no drawing had hitherto been published. "It is believed to have but one habitat on the face of the earth, claiming only a small strip of rocky shore reaching from Nice to about two miles east of Mentone. *Leucojum hyemale* grows in a stony soil, and out of the cracks of the hardest limestone rocks at Port St. Louis, Cape Veglio, on the way to Monaco, and at some height on the Aggel mountain, besides other less abundant localities." We are not aware whether this species has been introduced to English gardens, but it would be a very desirable acquisition. At Mentone it flowers in April.

THE Ant-eating Woodpecker (*Melanerpes formicivorus*) a common Californian species, has a curious and peculiar method of laying up provision against the inclement season. Small round holes are dug in the bark of the pine and oak, into each one of which is inserted an acorn, and so tightly is it fitted or driven in, that it is with difficulty extracted. The bark of the pine trees, when thus filled, presents at a short distance the appearance of being studded with brass-headed nails. Stowed away in large quantities in this manner, the acorns not only supply the wants of the woodpecker, but the squirrels, mice, and jays avail themselves likewise of the fruits of its provident labour.

DR. GEORGE STUCKLEY gives an interesting account of the Western Mole (*Scalops Townsendii*), which occurs in the Oregon and Washington Territories. He kept a specimen for some time in a box, at the bottom of which was a quantity of rich black loam. When disturbed it instinctively endeavoured to escape by burrowing in the earth of the box, using its long-pointed nose as a wedge to pioneer the way. The excavation was performed by its broad stout hands, which, surmounted with their long sharp claws, seemed admirably adapted for the purpose. The fore paws were worked alternately as in swimming, the hind feet acting as propellers. Although the earth in the box was soft and friable, it was nevertheless a matter of astonishment to see how rapidly the little creature could travel through it. When he slept it was in a sitting posture, with the body curled forward and the neck strongly bent, so that the nose rested between the hind legs. He thus assumed the shape of a ball, evidently his ordinary position when asleep.

THE cultivation of the poppy in China, which has been more than once prohibited by Imperial edicts, appears to be increasing everywhere, and becoming a profitable trade. In Szechuen, where the climate is warm and the season early, two crops at least are produced on the same ground annually. The seed of the poppy is sown in February, the plants flower in April, and the fruits are so far matured by the middle of May, that the juice is collected, and the stalks removed and burnt directly after, but previous to this the second crop, which may be either Indian corn, cotton, or tobacco, is sown, so that almost by the time the poppy is cleared from the field the new crop makes its appearance. The profit derived from the cultivation of the poppy is not only the result of a fair market value and a ready sale, but also from the fact that much of the work in the plantation, especially the gathering of the juice, can be done by the children of the family. The scratchings or incisions being made in the capsules in the morning, the juice which has oozed out in the course of the day is collected in the evening, and after simply exposing it to the sun for a few days it is ready for packing. The seed not required for sowing is used for food.

*ON THE RECENT SOLAR ECLIPSE**

I.

MY duty to-night, a pleasant one, although it is tinged with a certain sense of disappointment, is to bring before you the observations which were made of the recent eclipse in Spain and Sicily, to connect them with our former knowledge, and to show in what points our knowledge has been extended. In these observations, as you know, we had nothing to do with the sun as ordinarily visible, but with the most delicate phenomenon which becomes visible to us during eclipses. I refer to the Corona.

General Notions of the Corona

Let me, in the first place, show you what is meant by this

* A Lecture delivered at the Royal Institution, Friday, March 17, 1871

term, and state the nature of the problems we had before us. I have here some admirable drawings, which I will show by means of the lamp, of the eclipse that was observed in 1851 by several astronomers who left England in that year to make observations in Sweden, where the eclipse was visible. You must bear in mind that the drawings I shall bring to your notice were made in the same region, at places not more than a few miles apart.* The first drawing was made by an observer whose name is a sufficient guarantee for its accuracy—I refer to Mr. Carrington—*and when the sky was absolutely free from clouds.* In the next diagram you will see the corona is changed. The bright region round the sun is no longer limited to the narrow border of light round the dark moon, as seen by Mr. Carrington, but it is considerably expanded. The third gives still a greater extension, although that picture was drawn within a quarter of a mile of the place where Mr. Carrington's was taken. And lastly, we have a drawing made by the present Astronomer Royal, of that same eclipse, *through a cirrostratus cloud*, as unlike Mr. Carrington's as anything can possibly be. So that you see we began with a thin band of light about the moon, which would make the corona a few thousand miles high, and we end with a figure which Mr. Airy graphically likens to the ornament round a compass-card, and which gives the corona a height equal to about once and a half the sun's diameter.

I will next bring before you some drawings made during the eclipse of 1858, which was not observed in European regions, but in South America by two first-rate observers—one, M. Liais, a French astronomer, who was stationed at Olmos, in Brazil; the other, Lieutenant Gilliss, who was also there as a representative of the American Government, and observed some thousand miles away in Peru.

I will throw on the screen the appearances observed by these gentlemen, and I think you will acknowledge the same variations between their results, as to degree, while in one case we get a perfectly new idea of the phenomena—a difference in kind. I would especially call attention in the Olmos drawing to those extraordinary bundles of rays of wonderful shapes, which you see are so much brighter than the other portions of the corona. Such forms have been seen in other eclipses, but they are somewhat rare. The drawing made by Lieutenant Gilliss bears the same relation to that made by M. Liais as Mr. Carrington's did to the Astronomer Royal's; so that we may say that we not only get variations in the dimensions of the corona as seen at different stations, but that we furthermore get a strange structure introduced now and then in our drawing in regions where absolutely no corona at all exists in the other.

So much by way of defining the phenomena and giving an idea of the eye observations generally.

Let me now attempt to show you how the phenomena observed in the last eclipse bear upon the results which had been previously accumulated by means of telescopic and naked-eye observations, and by means of the polariscope and spectroscope.

I.—Telescopic and Naked-eye Observations

a.—A Part of the Corona is undoubtedly Solar

The first use I propose to make of the telescopic and naked-eye observations of last year is to show you a photographic copy of an admirable drawing made by Mr. Brett, who, though unfortunate enough to see the sun only for a very short time, was yet sufficiently skilled to make good use of that brief period. This drawing will bring before you the fact that even when a large portion of the sun remained unobscured by the moon, Mr. Brett was enabled to see a dim ring of light round the unobscured portion, which since the year 1722 has been acknowledged, beyond all question, I think I may say, to represent something *at the sun.* It was observed in 1722 round the uneclipsed sun, and in more recent times by Mrs. Airy in 1842, and by Rumker 1½ minute before totality in 1860, not to mention other instances. Therefore we have one observation made during this eclipse, confirming the old one, that in the corona there is a region of some small breadth at all events which is absolutely solar, and which it only requires a diminution of the solar light to enable us to see. This, then, we may look upon as the known; now let us feel our way gradually outwards.

b.—Rays, or Streamers, are added at Totality

The drawings made in all the eclipses which have been carefully recorded bring before us quite outside this narrow, undoubtedly *solar region*, observed before totality, as I have shown, and also by Mr. Carrington, and by Lieutenant Gilliss during totality in 1851 and 1858, extraordinary appearances of a different order. While in fact we have a solar ring from 2′ to 6′ high, we have rays of all shapes and sizes visible outside, in some cases extending as far as 4°, and in all cases brighter than the outer corona on which they are seen, the rays being different in different eclipses, and appearing differently to different observers of the same eclipse, and even at the same station. Here is a copy of a drawing made by M. Rumker of the eclipse of 1860, and I show it for the purpose of calling your attention to the fact that the two curious rays represented in it belong to a different order of things from those which we see in the rest of the corona From the beginning to the middle of the eclipse the east rays were the most intense. In the next drawing, which was made by the same observer, you see something absolutely new; and now the western side of the corona is the most developed; we have a new series of bright rays, and altogether it is difficult to believe that it is a drawing made by the same observer of the same eclipse.

The third drawing is a representation of the same eclipse by M. Marquez, who observed with a perfection of minute care which has scarcely ever been equalled: I bring it before you to show that the rays he saw were altogether differently situated. We may conclude then that the rays, though extremely definite and bright—as bright or brighter than the other portions of the corona which are visible before totality, they being *invisible* before totality—appear different to different observers of the same eclipse, and to the same observer during different *phases.*

c.—They Change from Side to Side

I have already said that M. Rumker observed that from the beginning to the middle of totality the rays on the *east* side of the sun were longest and brightest, and that from the middle to the end of totality the rays on that side of the sun where the totality ended were longest and brightest.

We will now carry this observation a step further, by referring to three drawings made by M. Plantamour in the same eclipse, that of 1860. In the first drawing we have the beginning of the total eclipse as seen in the telescope; with the naked eye naturally we should get the sun disappearing at the east or left-hand side, the moon moving from west to east; in the telescope things are reversed, and we have it right instead of left: and here we have the same thing that M. Rumker observed, namely, that when the eastern limbs were in contact, bright rays (M. Plantamour saw three) were visible on the side at which the contact took place. When the moon was half way over the sun, *two* rays of reduced brilliancy were observed on that side, not necessarily in the same position as *those* first observed, but one of *these* has been abolished altogether; and on the other side of the sun, where totality was about to end, we have three rays gradually suggesting themselves: at the end of totality the rays visible at the commencement are abolished, and now instead of them and of those seen at the middle of the eclipse, we have a bran new set of rays on the side of the moon from whence the sun is about to emerge.

This observation I need hardly say is of considerable importance in connection with the fact that from the year 1722 almost every observer of a total eclipse has stated that there is a large increase of brilliancy, and an increase of the size of the corona on the side where the sun has just been covered, or is just about to emerge.

Now, what was there bearing on this point in the recent observations? I have here three drawings, which, though roughly done, you will see are of great importance side by side with those of M. Plantamour. These are drawings which have been sent in to the Organising Committee by Mr. Gilman, who lives in Spain, and who took considerable interest in the eclipse, and sent the results of his observations to England with the eclipse party when they came home; and it is of importance that you should see everything that Mr. Gilman has done. If you agree with this explanation of the square form of the corona, which was observed in Spain this year, it will explain the quadrangular form observed in the corona in a good

* Mr. Carrington observed at Lilla Edet, on the Grota River. The Astronomer Royal observed at Göttenburg The second drawing referred to was made by Pettersen, at Göttenburg; the third by a friend of the Rev. T. Chevallier, at the same place; and I might have added another by Fearnley, taken at Rixhöit, in which the corona is larger than in any of the others. The series is most instructive. See Mem. R. A. S. vol. xxi.

many previous eclipses. Mr. Gilman says that at the commencement of totality—let me remind you, the commencement was determined by the disappearance of the sun at the east limb of the moon, which is east in Mr. Gilman's drawing, as he was observing with the naked eye—the commencement, he says, was determined by the corona flashing out very much like a capital D. You see on the black board exactly the outline, and you will at once mentally associate one half of the diagram with the rays observed by M. Plantamour, and the other half, in which there is a nearly perfect ring of light round the moon, with the corona observed by Mr. Carrington all round it in a cloudless sky. At mid-eclipse Mr. Gilman also observed the corona, sketched out its outline carefully, and found rays coming out on the opposite side, adding themselves on to the perfect ring first seen there. Opposite the two salient angles he observed at the commencement of totality—represented by the top and bottom of the upright stroke of the capital D—there were two others; *the corona now appeared square*, and then, just before the end of totality came on, the two corners first seen were observed to disappear altogether, leaving nothing but a perfect ring, and where, at the beginning of the eclipse, nothing was seen but a perfectly round ring, the two exactly similar forms on the opposite side shot forth, and you got a D reversed (C). Mr. Warrington Smyth, who drew a square corona, saw the light flash out into the corona before the end of totality, and believes that all the angles of the square were not visible at one and the same time.

Here, then, you have observations of exactly the same character as those of M. Plantamour, to which I have referred. In the drawings of both are shown the inner part of the corona, which you saw growing in the observations of 1851, to which were added the strange forms observed in 1858. You have these strange variations positively growing at the same place and the same time, in the same and in different eyes. Obviously there must be very much that is non-solar, call it personality, atmospheric effect, or what you will, connected with it. We have added to the stable the unstable. The question is, to what is this unstable portion due?

d.—They are very variously represented

I will now refer to other drawings of the late eclipse, which were made in Sicily. For some reason or other, which I do not profess to understand, the corona, which appeared in Spain to be square, and to Mr. Gilman like a D at the beginning, and like a D reversed (C) at the end,—to all those with whom I have conversed who saw it in Sicily, it appeared as round as you see it here, in this drawing made by Mr. Griffiths; and, instead of being square, we had sent to us all sorts of pictures, a large number of them representing a stellate figure. Here is a drawing made by a Fellow of the Royal Society, on board one of Her Majesty's ships (the *Lord Warden*) which were trying to save the poor *Psyche* at Catania. In this we have perfectly regular rays drawn from every region of the sun, some long, some short, but similar rays are almost invariably opposite each other; but in the interior, inside these rays, the corona is just as it was observed by Mr. Griffiths at Syracuse. I now show you a drawing made by an American gentleman at sea, between Catania and Syracuse, with one ridiculously long ray, a ray as long as was seen by Otto Struve in 1860. Other drawings were made, even on board the same ship, so unlike each other, and so bizarre, that I need only refer to them as showing that there at all events must be some personality. We have then to account for the variations between the observations made in Spain and those made in Sicily. I regret that we have not a third order of difficulties to contend with, as doubtless we should have had if observations had been made by Mr. Huggins' party in North Africa.

e.—The Rays are accompanied by a Mass of Light.

These changes of the rays from side to side are accompanied by, and are perhaps to a certain extent due to, the bursting forth of brilliant light in their neighbourhood, where the limbs are nearest in contact. This was first observed by Miraldi in the eclipse of 1724, and has frequently been recorded since. Mr. Warrington Smyth, to whom I have before alluded, states that he noticed this in the last eclipse, and the photographs, I think, have recorded it; but as there is some uncertainty on this point, I need only suggest it.

f.—Long Rays are seen extending from the Cusps before and after Totality

So far I have referred only to the rays visible during totality, but long rays were seen when a crescent of the sun was visible in 1860 and 1868 by Mr. Galton and Mr. Hennessy. Mr. Brett caught the same phenomenon last year; but as the sky was cloudy the commencements of the rays only were seen, appearing like delicate brushes in prolongation of the cusps. These observations are of great value, *as no one for one moment imagines that these rays are solar*, and yet they are very like those seen during totality.

g.—Sometimes Dark Rays, called Rifts, are seen instead of Bright ones

Those rays to which I have referred are, however, not the only kind of rays that are observed. At times are seen, as it were, openings in the corona; the openings being of the same shape as the rays, that is, expanding as they leave the dark moon, and opening more or less exactly as the rays do. Like the rays also they are sometimes very numerous; in other eclipses they are few in number. Let us take the eclipse observed in India in 1868. Several drawings made there showed the corona as square as it was drawn in Spain last year; others as round as it was seen in Sicily; but the eclipse was not observed only in India, it was observed at Mantawalok-Kelee by Captain Bullock, and at Whae-Whan, on the east coast of the Malayan Peninsula, by Sir Harry St. George Ord, Governor of the Straits Settlements. In the former place we had rifts expanding rapidly as they left the sun—one forms an angle of 90°, the sides of another being *parallel*—separating patches of corona, which in some places extends 2½ diameters of the moon from the sun.

At Whae-Whan we are told that at one particular moment of the eclipse "it was noticed that from several points in the moon's circumference darker rays emanated, extending to a considerable distance into space, and appearing like shadows cast forth into space by something not very well defined;" these dark rays afterwards "diminishing."

Now let us pass on to the eclipse of 1869. In two drawings made by Dr. Gould, in which the changes in the bright bundles of rays come out in a most unmistakable way, we get similar rifts, which changed as violently as did the rays; while in another drawing made by Mr. Gilman, the whole corona is furrowed by narrow rifts in all regions lying between violet, mauve-coloured, white, and yellowish white rays!

Now, what have we bearing on this point in the recent observations? No rift was *seen* in Sicily; one rift was recorded by the sketchers in Spain, but more than one rift was photographed in both places. We must remember, however, in thus bringing eye-sketches and photographs into comparison, first that the eye too often in such observations retains a general impression of the whole phenomenon, while the plate records the phenomenon as it existed at the time at which it was exposed; and secondly, that we know that the plates record chemically, while the eye records visually. We are dealing with two different kinds of light.

I will show you two photographs on the screen. Although the lucid intervals were very rare, we were fortunate enough to get one photograph of the coronal regions in Syracuse, and one in Spain. I now show you the photograph made by the American party in Spain. You see here that, probably owing to a cloud, we get a certain amount of light driven on to the dark moon, and you also see the indications of the rifts. This photograph was taken with an instrument with a small field of view, so that the most important parts of the corona were rendered invisible by the instrument itself.

Lord Lindsay, who also photographed in Spain, recorded no rifts.

In the other photograph, taken at Syracuse, the result is better. We have the equivalent of the rift in the photograph I showed you before. The instrument was extremely unsteady, and the definition not so good as it would have been if Mr. Brothers had had a good opportunity of displaying his skill. We get other fainter indications of other rifts here and there, and the question whether these rifts agree in the photograph taken in Spain with those in that taken in Syracuse is one of great importance; and it is to be hoped that before long it will be set at rest. Some observers think they agree; others think they do not.

But there is an important consideration based on that photograph, to which I must draw your particular attention. I have shown you the photograph as it may be thrown on the screen; but in the photograph itself there are delicate details which it is impossible to reproduce. The dark portions in the corona indicated in the copy I have shown you are merely the bases of so many dark wedges driving out into space, like their prototypes

in the Indian eclipse. It is Mr. Brothers's opinion, I believe, that all you see on the screen round the dark moon, all that enormous mass of light, nearly uniform in texture, and these beautiful broad rays between the rifts are really and absolutely parts of the solar corona. I confess I do not wish to commit myself to such an opinion. We want more facts, and the *onus probandi* lies with those who insist upon that view, and I have yet to hear an explanation of them on that basis.

h.—The Corona sometimes seems to be Flickering or Rotating.

We now come to the next point. Time out of mind, that is, for the last two centuries, the corona has been observed to be flickering, waving, or rotating, moving in every conceivable way and direction. In 1652 it was described as "a pleasant spectacle of rotatory motion." Don Antonio Ulloa remarked of the corona observed in the eclipse of 1788, "It seemed to be endued with a rapid rotatory motion, which caused it to resemble a fire-work turning round its centre." The terms whirling and flickering were applied in the eclipse of 1860. This extraordinary condition of things was also thoroughly endorsed by the late observations. It certainly exists, and is among the observations we have to take into account. When I saw an officer of one of the ships at Catania, I asked him if he had taken a drawing of the corona. "No," he said. I asked him, "Did you see any rays?" "Yes." "Then why did you not make any drawing of them?" His answer was, "How on earth could you draw a thing that was going round and round like a fire-work?" This was not the only observation of the kind, and the tendency of such observations I need hardly say is to strengthen a belief in the unstable, and therefore uncosmical, nature of their rays.

Is this variation of light due to the brilliancy of the corona, and the rapid change of the rays, which is one of the results which comes out clearest? In 1842 the brilliancy of the corona was stated to be insupportable to the naked eye. A similar remark was made to me by several of those officers who saw the last eclipse in Sicily.

J. NORMAN LOCKYER

(*To be continued.*)

SCIENTIFIC INTELLIGENCE FROM AMERICA*

PROF. LEIDY has lately announced to the Philadelphia Academy of Natural Sciences the existence of some new fossil mammals from the Tertiary formations of Wyoming Territory. One was a lower jaw, discovered by Dr. J. Van A. Carter in the vicinity of Fort Bridger. The animal to which it belonged was as large as a hog, but was more nearly allied to the rhinoceros or tapirs. It was especially remarkable for the possession of a large pair of front teeth, resembling, both in form and construction, the incisors of the beaver. The name proposed for it was *Trogurus castoroideus*, or the beaver-toothed gnawing-hog. Another of the fossils indicates a carnivorous animal, a contemporary of the former, and about the size of the gray fox. The animal was related to the weasel and canine families, and was called *Sinopa rapax*, the former name being that applied by the Blackfeet Indians to a small fox. Prof. Leidy also exhibited photographs of the lower jaw of the American mastodon, recently received from Prof. W. C. Kerr, State Geologist of North Carolina. The jaw was found in Lenoir County of that State. It belonged to a mature male, and was of special interest from its retaining both tusks, as well as the molar teeth.—Among objects of great ethnological import are the aboriginal inscriptions or carvings upon rocks, which are met with in North America and elsewhere, and are sometimes of a very remarkable character. Ordinary copies of such inscriptions, unless they be photographs, are rarely of sufficient accuracy to be of much value; and those of our readers who are likely to come across such inscriptions may like to know a method by which an absolutely perfect fac-simile can be made. This process has been applied with much success in copying carvings in Egypt and other places, and it will be equally serviceable in our own country. For this purpose the inscription is to be first well cleaned from dust or mud by means of a hard, stiff brush; stout, unsized paper is then to be wetted rapidly, but uniformly, in a tub of water, and applied to the inscription, and forced into the irregularities by repeated and forcible strokes with a hard brush, an ordinary clothes-brush being as good as any for the purpose. If the stone be clear of dust, the paper adheres, and, when dry, falls off, forming a perfect mould of the inscription. If the carving be deep or broad, it is sometimes advisable to apply several sheets of paper, one after the other, brushing over the surface of one with glue or gum before applying the next, so as to obtain, when dry, a firm body. By making a plaster cast of the paper relief thus prepared a fac-simile of the inscription will be obtained.—The present year seems to be marked with a great deal of activity and enterprise in researches connected with the natural history and physics of the deep seas, especially on the coast of America. We have already referred to the enterprise proposed by the Coast Survey, of sending a steamer, especially adapted to this purpose, around Cape Horn to the California coast, on a ten-months' journey, to be accompanied by Professor Agassiz and Count Pourtales, and a corps of assistants, all prepared to make observations and collections on the most perfect scale. The expense of the scientific work will, it is understood, to the amount of 15,000 dollars, be defrayed by Mr. Thayer (the same gentleman who supplied the funds for Professor Agassiz's expedition to Brazil), a sum which will probably enable Professor Agassiz to accomplish his object in the most perfect manner.—Professor Verrill and party, from Yale College, will also, it is expected, prosecute an exhaustive research into the deep sea and littoral fauna of the Vineyard Sound and the adjacent waters, in connection with the inquiries of the United States Commission of Fish and Fisheries relative to the decrease of the food fishes of our coast. Corresponding researches will also be carried on in the deeper waters of Lake Michigan, where, it may be remembered, the interesting discovery was made last year of crustaceans and fish of marine types at a depth of 300ft. and over. The inquiries this year will be under the immediate direction of Dr. Stimpson and Mr. Milner in a still deeper part of the lake, and it is not at all improbable that discoveries of the highest interest will be made.—The Arctic expedition of Captain Hall will also undoubtedly do its part in the general work, as the naturalist of the party, Dr. Emil Bessels, has had large experience in such labours, and is practically conversant with the fauna of the arctic seas from his connection with the Spitzbergen expedition of 1869.—At the June meeting of the California Academy of Sciences the subject of inviting the American Association for the Advancement of Science to meet in San Francisco in 1872 was discussed, and the treasurer was instructed to call upon the trustees, and to solicit the co-operation of the Chamber of Commerce in taking measures toward this object. The meeting for the present year will be held in August next in Indianapolis, and a large attendance is expected, especially of Western members, to whom the places of meeting in the East have generally proved too remote to suit their convenience.

* Communicated by the Scientific Editor of *Harper's Weekly*.

SCIENTIFIC SERIALS

THE *American Naturalist* for June contains no article of very striking value, though several of interest in special subjects. Dr. Elliott Coues contributes an account of the yellow-headed blackbird, *Xanthocephalus icterocephalus*, first described by Prince Buonaparte in his continuation of Wilson's Ornithology.—An article on Cuban Seaweeds, by Dr. W. G. Farlow, includes outline drawings of a number of distinct types.—Dr. Lebaron describes a new species of moth, the larva of which is extremely destructive to young apple trees, which he calls *Tortrix malivorana*, or the Lesser Apple Leaf-folder.—Mr. E. L. Greene contributes June Rambles in the Rocky Mountains, with special reference to their flora.—From Dr. Henry Shimer we have "Additional Notes on the Striped Squash Beetle," and from Prof. W. H. Brewer, "Animal Life in the Rocky Mountains of Colorado."—A larger space than usual is occupied by Reviews, among which is one of Mivart's "Genesis of Species," comparing the views of the author with those of the American writers, Cope and Hyatt.

THE first article in the *Journal of Botany* for June is an important one, by Prof. A. H. Church, on Sugar in Beet-root, with a record of investigations on the effect of the amount of rainfall in the development of the sugar.—Dr. Henry Trimen discusses the question, "Is the Sweet Flag, *Acorus calamus*, a Native?" showing that it was unknown in this country before 1596, and that it was not till about 1660 that it was reported as a wild plant from Norfolk. The plant appears to be originally a native of south-east Europe.—Prof. Dickson has an article on the Phyllotaxis of *Lepidodendron*, and the allied, if not identical,

genus *Knorria*.—Mr. A. G. More continues his Supplement to the "Flora Vectensis;" and the Rev. Jas. M. Crombie his additions to the British Lichen-Flora.

The number for July contains Mr. Ernst's "Jottings from a Botanical No e-book," and concludes Mr. A. G. More's "Supplement to the Flora Vectensis." Dr. Trimen contributes some notes on plants observed in Jersey and Guernsey in April. There are several other short papers and notes of special interest to British botanists.

OF the *Bibliothèque Universelle et Revue Suisse*, one of the most valuable of continental periodicals, whether we consider the quality of its original articles, or the admirable extracts of scientific memoirs which it contains, we have just received the part published on May 15, which forms the commencement of a new volume. The first and most important of the three papers contained in it is on the action of magnetism on gases traversed by electrical discharges, by MM. A. de la Rive and E. Sarasin, in which the authors describe a long series of experiments made by them, leading to the following conclusions:—1. The action of magnetism exerted only upon a portion of an electric jet traversing a rarefied gas, causes an augmentation of density in this portion. 2. This action exerted upon an electric jet placed *equatorially* between the poles of an electro-magnet, produces in the rarefied gas an *augmentation* of resistance proportional to the conductivity of the gas itself. 3. On the contrary, it causes a corresponding *diminution* of resistance, when the jet is directed *axially* between the two magnetic poles. 4. When the action of the magnetism is to impress a continuous movement of rotation upon the electric jet, it has no influence upon the conductivity if the rotation be in a plane perpendicular to the axis of the iron cylinder detaining the rotation, and diminishes it considerably if the rotation takes place so that the jet describes a cylinder round the axis. 5. These effects do not seem to be due to variations of density, but to perturbations in the arrangement of the particles of the rarefied gas.—A second paper is an excellent abstract and discussion by M. Emile Gautier, of the observations of solar protuberances, made at Rome by Prof. Respighi; and the third consists of an account of geological, meteorological, and archæological explorations made in the province of Constantine (Algeria), by M. Tissot.

THE first part of the twenty-third volume of the *Zeitschrift der deutschen geologischen Gesellschaft*, containing the proceedings of that society for the months of November and December 1870, and January 1871, includes one paper which will be of especial interest to British geologists, namely, "Some Geological Sketches from the East Coast of Scotland," by Prof. F. Zirkel, extending over 124 pages of text, illustrated with four plates. In this paper the complicated geology of the islands of Arran, Mull, Iona, Staffa, and Skye is discussed in considerable detail, and the author winds up with a description of the east and west section of the north of Scotland. Another long paper is the first part of a geological description of the annular mountain of Santorin, by M. K. von Fritsch.—M. C. Struckmann describes the *Pteroceras* beds of the Kimmeridge formation at Ahlem, near Hanover, which he divides into three series (upper, middle, and lower), indicating the characteristic fossils of each deposit. M. R. Richter publishes a fourth notice on the Thuringian slates, for which he claims an Upper Silurian age, an opinion here supported chiefly on the evidence of Graptolites. The author discusses the affinities of the Graptolitidæ, and adopts an opinion expressed by Leuckart (MS.) that this group is to be regarded as nearly allied to the Bryozoa. The author describes a new genus, *Triplograptus*, the chief character of which is that the canal has three vertical rows of alternating cells, of which the type is *T. nereitarum* (Richt.), and also as new species *Diplograptus pennatulus* and *Monograptus crenatus*. These and some other species are figured in the plate accompanying the memoir. A new species of *Nautilus* (*N. veles*) is also described and figured in this paper (p. 243). From M. Emanuel Kayser we find a notice of the occurrence of *Rhynchonella pugnus* with traces of colour in the limestone of the Eifel (Devonian), to which is appended a tabular list of those fossil shells on which traces of colouration have been observed.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 21.—Joseph Prestwich, F.R.S., in the chair.—R. J. Watson, W. T. Scarth, Gen. A. C. Bentinck, and John Brooke were elected Fellows of the Society.—"On some supposed Vegetable Fossils," by William Carruthers, F.R.S. In this paper the author desired to record certain examples of objects which had been regarded, erroneously, as vegetable fossils. The specimens to which he specially alluded were as follows:—Supposed fruits on which Geinitz founded the genus *Guilielmites*, namely, *Carpolites umbonatus* Sternb., and *Guilielmites permianus* Gein., which the author regarded as the result of the presence of fluid or gaseous matter in the rock when in a plastic state; some roundish bodies, which, when occurring in the Stonesfield slate, have been regarded as fossil fruits, but which the author considered to be the ova of reptiles, and of which he described two new forms; and the flat, horny pen of a Cuttlefish from the Purbeck of Dorsetshire, described by the author as *Teudopsis Brodiei*, sp. n. Mr. Seeley remarked on the compressed spheroids found in so many rocks, that there was a difficulty in accepting the view of their originating in fluid vesicles, though he was unable to suggest any other theory by which to account for them. He observed that the eggs from the Stone field slate closely resemble those of birds, and that it was of the highest interest to find such eggs in strata containing so many remains of ornithosaurian forms, such as *Rhamphorhynchus* and *Pterodactylus*, of which genus probably these were the eggs. Prof. Rupert Jones fully recognised the ingenious explanation of the bubble-formed limited slickensides, that looked so much like possible fossil fruits, and Mr. Carruthers's masterly treatment of the other specimens. But he wished that the author would take up the subject exhaustively, and define the nature of other supposed vegetable fossils, such as the so-called fucoids, *Palæochorda*, *Palæophyton*, *Oldhamia*, &c, many, if not all, of which Prof. Jones thought to be due to galleries and other tracks made by Crustaceans. Prof. Ramsay had known many instances of such blunders as those pointed out, made, not by experienced geologists, but by those unacquainted with the science. Though he had never regarded the flattened spheroids as fossils, he was unable to account for their presence in the clay-beds of different ages. Mr. Hulke inquired whether Mr. Carruthers considered the limited slickensides common in the Kimmeridge shales as due to gaseous origin. He remarked on the rarity of Pterodactylian remains as compared with those of other Saurians in the Wealden beds, in which the presumed eggs of Pterodactyls were found. Mr. Seeley did not regard the Wealden egg as being that of a Pterodactyle. Mr. Carruthers, in reply, remarked that the local slickensides mentioned by Mr. Hulke differed in character from those to which he had referred.—"Notes on the Geology of part of the County of Donegal," by A. H. Green, F.G.S. In this paper the author described the geological structure of the country in the neighbourhood of the Errigal Mountain, with the view of demonstrating the occurrence in this district of an inter-stratification with mica-schist of beds of rock, which can hardly be distinguished from granite, the very gradual passage from alternations of granitic gneiss and mica-schist into granite alone, and the marked traces of bedding and other signs of stratification that appear in the granite, to which the author ascribed a metamorphic origin. He also noticed the marks of ice-action observed by him in this region, and referred especially to some remarkable fluted bosses of quartzite, and to the formation of some small lakes by the scooping action of ice. Mr. Forbes stated that none of the facts of this communication were new, but he dissented altogether from the conclusions arrived at by the author in regarding these rocks as originally of sedimentary origin, and for the following reasons: (1) That this district has been studied in detail by Mr. Scott and Prof. Haughton, who declare the rock to be undoubtedly intrusive, as it not only sends out veins into the neighbouring strata, but also encloses fragments of the rocks through which it has broken. (2) Because the author starts from the idea that if such rocks are found to lie conformably on beds of undoubted sedimentary origin, it is a proof of their being themselves sedimentary or stratified,—a conclusion which is totally unwarranted, since there are innumerable instances, not only of beds of lava or other igneous rocks being conformable to fossiliferous strata, but of their also being found intercalated with such beds even for considerable distances. (3) The strata, so far from being proved by him to be of truly sedimentary origin, are of a most questionable origin, since they are neither in themselves fossiliferous, nor can they be correlated with any containing fossils as proofs of true sedimentary deposition; and the description of his section is sufficient to show this; for although it looks well on paper on a scale of three feet to the mile, the author has so little confidence in it that he is not even certain as to which is the top or bottom of the section on which so much generalisation is based. (4)

That a parallel structure equally, if not better developed than any occurring in the gneiss of Donegal, is common to many volcanic rocks, as in a specimen laid before the meeting, in which this parallel foliated structure due to crystallisation-layers is so well developed as to make it appear exactly like a stratified rock, and even split along these lines, and this, although the product of volcanoes still active is found for great distances both overlying conformably and intercalated between beds of the Cretaceous and Oolite formations. Mr. Scott was unwilling to accept the section given by the author as satisfactory. He agreed, however, as to the bedded appearance of the granite, and to the masses lying in general conformably with the lines of stratification of the country. The nearest spot at which fossiliferous rocks occurred was separated from the beds described by the whole width of the county of Tyrone, though some presumed Eozoonal forms had been found at a less distance. He was not prepared to believe in the original absolutely fused condition of granite, nor in there being two distinct forms under which it occurred.—"Memoranda on the most recent Geological Changes of the Rivers and Plains of Northern India, founded on accurate surveys and the Artesian well-boring at Umballa, to show the practical application of Mr. Login's theory of the abrading and transporting power of water to effect such changes," by T. Login, The author commenced by referring to the general conditions of the surface of the country under consideration, and to the evidence afforded by it of a great decrease in the amount of rainfall, and a great change in the nature of the rivers. His object was to show that the superficial deposits of the plains of India were formed by the action of mountain streams, the deposits being irregular transversely, but exhibiting a uniform section longitudinally, in a curve which the author believed to be a true parabola, as indicated by Mr. Tylor. The connection of this with the author's theory as to the transporting power of water was indicated. The author also showed that the beds of the large Indian rivers are rising rather than being lowered, and pointed out that this was in accordance with his theory.

Halifax, Nova Scotia

Institute of Natural Science, May 8.—Mr. J. Matthew Jones, F.L.S., president, in the chair. Mr. Frederick Allison read a paper entitled "Results of Meteorological Observations at Halifax, Nova Scotia, for 1870." The temperature of January had not been approached since 1863. Mean pressure was great. Cloud was scanty, and winds strong, N.W. prevailing. Very large total precipitation, due to heavy rain, the snow-fall being deficient. No fog, and but five days' sleighing during the whole of January. Strong east gales at the close of the month. February was nearer to normal temperature. Mean pressure very light. Cloud far exceeded that of January and its own average. Prevalent wind, N.W., strong. Great precipitation, nearly doubling the average amount, and especially large in rain. One fog, and sleighing from the 1st to 25th. On the 9th strong east gale in morning, and blowing at night from the west. March bore much the same relation to normal temperature as did February; but the minimum of the year, 6°, occurred on the 12th of that month. Pressure still extremely light. Cloud in decided defect. Prevalent wind N.N.W., with mean force great. Precipitation, both of rain and snow, small. Only one fog. Eleven days of sleighing. Three gales, all more or less eastwardly. Wild geese (*Anser Canadensis*) passed over on their northerly migration on the 19th. Peach, trained against a south wall, blossomed on the 24th. The American robin (*Turdus migratorius*) appeared on the 30th. April was warm. Pressure 29·743, but ·001 below an eight years' average. Cloud still deficient. A peculiar direction of wind was prevalent—E.S.E. Mean force small. Precipitation close to average; rain being abundant, but snow only one inch. Five fogs recorded. First thunder and lightning this year on the 12th. One short gale from E.S.E. Frogs (*Hylodes Pickeringii*) first heard on the 8th; and May flowers (*Epigæa repens*) in full bloom on the 12th. The mean temperature of May was a little less than average. On the 30th 80°·2 was reached. Mean pressure a little light. A very bright month, with only 3·19 inches of rain, the average being 4·33. Snow inappreciable, the latest falling on the 24th, and melting as it fell. Four fogs. Thunder and lightning on the 9th and 12th. The garden cherry blossomed on the 23rd, and the humming-bird (*Trochilus colubis*) was first seen on the 18th. June was slightly cool, somewhat low in pressure and decidedly bright. Only 1·69 inches of rain fell. Mean velocity of wind but 8·8 miles per hour; direction W.S.W. Three fogs noted. No frost after the 24th of the preceding month, either at five feet high or on the surface of the earth. Thunder and lightning twice. The apple blossomed on the 6th, and red clover same date; horse-chesnut on the 2nd; wild strawberries ripe on the 20th. Grass mowing began about Halifax on the 30th. July temperature was 1°·85 above the average. On the 24th 91°·5 was marked. The mean of six equidistant observations on 25th, 75°·27, being the warmest day recorded at this station for at least twelve years. Mean pressure low. Great want of cloud. Light winds; direction S. 59° W.; velocity 8·1 miles per hour. Rain, 3·21 inches, being much above average. Four fogs. Thunder and lightning twice. August was warm also. Mean pressure almost identical with July, being 29·659. Very little cloud. Wind, resultant direction, N. 77° W. Mean velocity 10·5 miles per hour. Rain scanty, giving but 2·20 inches. Fogs three. Thunder and lightning thrice. September mean temperature 57°·20, having fallen 7°·60 below August. On the 30th exactly 32° was registered by grass minimum; but atmosphere never descended to freezing point. Mean pressure still low, and cloud also deficient. Wind, resultant direction N. 15° W., and mean velocity only 10·6 per hour. Rain, half an inch less than average. Three fogs. Hoar frost on the 30th. Thunder once. Lightning twice. Three gales. October had a mean temperature of 48°·14. Mean pressure 29·825. Still a quantity of cloud, though October is frequently a bright month in Nova Scotia. Resultant direction of wind N. 42° W., and mean velocity 12·45 miles per hour. Heavy rainfall, and eight inches of snow. One fog. Three gales from N.W., S.S.W., and S. First frost, five feet above ground on the 26th; temperature having been above 32° 155 days. Measurable snow on 31st. The mean temperature of November remained above the average. The whole pressure again small. The month was rather less cloudy than usual. Resultant direction of wind N. 87° W., and mean velocity only 10·75 miles per hour. Rainfall large, 5·67 inches, and snow depth great, 7·7 inches. Three fogs. Three gales, N.W., S S.E., and S.E. Meteors on the night of the 14th. December was very mild. Mean temperature 30°. Pressure very low. Much cloud. Resultant direction of wind N. 76° W., and mean velocity 11·6 miles per hour. Rain was heavy, and snow small, though containing larger amount of water than average. One fog. Four days' sleighing. On Christmas Eve thermometer reached 4°·6, minimum of month. After noting the cyclone of the 3rd and 4th of September, Mr. Allison proceeded to connect it with the gale of the 7th moving in the Bay of Biscay, in which the *Captain* foundered. Giving the following figures from a mass of observations, to show the storm path:—S.S. *Robert Lowe* at sea, lat. 43°2′ N., long. 65°3′ W., September 4, 4 A.M., bar. 28 700; wind 25lb. per square foot. Halifax N.S., lat. 44°39′ N., long. 63°36′ W., September 4, 9.30 A.M., bar. 28·952, 6 to 7 A.M.; wind velocity 65·7 per hour, and reaching 70 miles in gusts, fully 24·5lb. per square foot. Glace Bay N.S., about 250 miles from *Robert Lowe*, E.N.E., bar. 3 P.M. 29·333; wind 3 P.M. 86 miles per hour. This storm was travelling at direct rate of about 23 miles per hour in this longitude, its speed being accelerated as it progressed eastward. It would be due, with its south-eastern edge, in diminished force probably, in Bay of Biscay on evening of 6th of September. From these and other data a world-wide system of telegraphic storm warnings was urged.—Another interesting paper, "On the Meteorology of Glace Bay, Cape Breton, N,S.," by Mr. Henry Poole, was also read.

Paris

Académie des Sciences Morales et Politiques, June 24.—M. Jules Simon in the chair. Notice was given of the death of M. Ramon de la Sagra, a Spanish gentleman who had been a great traveller in America, and was well known as a botanist.—M. Egger read some pages of his great work "On the Progressive Development of Infants."

July 8.—M. Paul Janot in the chair. Notice was given of two letters received from M. Henry Martin, a member of the National Assembly, and M. Filon, an inspector of the Academy of Paris, both contending for the seat left vacant by the late M. Pierre Clement, who wrote, many years ago, a history of the *Revocation de l'Edit de Nantes*, vindicating Protestantism, and published many articles in the *Journal des Economistes*, in support of free trade policy. The contest will be severe, as M. Henry Martin is very popular, being the author of a History of France. M. Filon is a gentleman of a wider intellect, and has written a Comparative History of France and England. The election will take place on the 22nd.

Académie des Sciences, July 10.—M. Claude Bernard in the chair. Notification was received of the death of M. Haidinger, the keeper of the great aerolitic collection at Vienna and a correspondent in the section of mineralogy.—The public sitting, which, according to the rules, was held before the secret one, was rather long and interesting. M. Puiseux was unanimously elected a member of the section of geometry (this honour is very seldom paid to any member). M. Puiseux belongs to the scientific staff of the National Observatory. He was much praised many years ago by Cauchy for his calculations on variations of weight and of its effects. He was a contributor to Lionville's *Journal de Mathematiques*.—M. Boussingault described some experiments showing that water is not liable to freeze irrespective of the degree of cold to which it is submitted, as long as it is not allowed to expand in order to change into ice. It is the complement of the celebrated Florentine experiment. M. Boussingault exposed water to $-13°$ Cent. enclosed in strong steel tubes as used for rifled guns, without any congelation taking place. On unscrewing the steel end of the barrel, the congelation was instantaneous. The fluidity of the water was made manifest by small steel spheres, which moved freely inside the guns during the whole process, and would have been stopped by congelation. A very long conversation took place between M. Boussingault and several members who proposed many objections, to which he found ready answers.—M. Saint-Venant read a long report on a memoir presented by M. Maurice Levy on several Equations showing the internal movements of molecules when a ductile body is submitted to external pressure.—M. Faurneyron was a French engineer of great reputation, known by the invention of "turbines" or hydraulic wheels. He bequeathed to the Academy a certain sum in the funds to give a 40*l.* prize to the best memoir on Practical Mechanics every two years. The Academy appointed a committee of five of its members to draw up a programme for the next competition. The competition is to be open to all, irrespective of nationality and qualification, except to the members of the different French academies.—M. Brown, the astronomer at the celebrated Trevandum Observatory, read a most important note on the "Diurnal Lunar Variation," which he proved has sometimes to exceed the solar variation. The law is illustrated by calculating the maximum Every day there are two maxima of lunar action. In June, when the moon is on the 6th and 18th horal meridian, in December on the 0th and 12th, and in the intermediate months on the intermediate meridians, according to progressive changes. The excursions are greater when the moon is nearer to us (perigee), and when the passage of the moon to the maximum meridian is by daylight. This difference is very great, the *nocturnal* max. reaching only $\frac{1}{3}$ of *diurnal* max. The law is worth the most serious consideration, as connections between variations of magnetism and temperature are becoming every day more and more frequent. It may lead to the discovery of the lunar influence on meteorology, which discovery will be *initium sapientiæ*.—M. W. de Fonvielle sent a note discussing certain singular phenomena which were observed in Scotland during the stormy periods of June 18 and July 18. The facts were quoted from the *Scotsman*, an Edinburgh paper. The note was printed in the *Comptes Rendus*. The author is anxious to see if "mirages," as observed on the Isle of Man, can be considered as having been a presage of the stormy weather. He wrote also upon certain accidents, showing that it is dangerous to move metallic objects during thunderstorms. M. Chapelas presented the results of observations made during twenty years (1848-1868) on 39,771 meteors, out of these 23,481 were observed in summer when the nights are short, only 2,145 in winter when the nights are long. The mean direction is S.S.E. The numbers of meteors vary in *inverse ratio* with their magnitude:—1st magnitude 2,497, 2nd magnitude 3,918, 3rd magnitude 7,137, 4th magnitude 8,847, 5th magnitude (an exception to the rule) 8,050, 6th magnitude 9,322 (very slight augmentation). He says, moreover, it shows that falling stars are more frequent in high altitudes. It is true, assuming falling stars to be essentially of the same magnitude, and differing only apparently from distance.

RIGA

Society of Naturalists, February 1.—Prof. Schell discoursed upon the importance of water-levels on the coasts of the Baltic provinces, and described some anemometers.—M. Schroeder communicated a notice relating to the avifauna of the Baltic provinces, in which he mentioned several species to be struck out of or added to the previously published lists. He made the total number of species, 272.—Baron F. Hoyningen-Huene communicated a continuation of his phænological observations, during the year 1870, containing a report on natural phenomena observed from March to October.

PHILADELPHIA

American Philosophical Society, April 1.—Prof. Cope made remarks on the Vertebrata obtained in the Port Kennedy bone cave by Chas. M. Wheatley, stating the number of species to be forty-two. The Mammalia were referred to orders, as follows:—Edentata, 6 species; Rodentia, 14; Insectivora, 1; Chiroptera, 1; Ungulata, 8; Carnivora, 4; total, 34, of which about half are new to science. Birds and Reptiles, 8 species. He made remarks on the nature and origin of the post-pliocene fauna, the origin of the caves, and possible topographical history of the country in that connection.—Pliny E. Chase read a paper on "Resemblances between Atmospheric, Magnetic, and Ocean Currents."—Lieut. Dutton presented some views on regional subsidence and elevation, and mentioned the physical changes produced by the metamorphism of rocks as an agent in changing the contour of the earth's surface. The obliteration in specific gravity produced by change of chemical constitution of interior rock strata was an important cause of the elevations and subsidences of the earth's crust, generally overlooked.

BOOKS RECEIVED

ENGLISH.—Our Sister Republic; a Gala Trip through Mexico in 1869 70 (Trübner and Co.).

FOREIGN.—(Through Williams and Norgate)—Medizinische Jahrbüchen: S. Stricker, &c., vols. 1 and 2.—Naturwissenschaftliche Vorträge: J. R. Mayer.

PAMPHLETS RECEIVED

ENGLISH.—National Health: H. W. Acland, M D.—How to Live on 6*d.* a Day: Dr. Nichols.—A Sanitary Inquiry: R. Weaver.—Art and Religion: J. Gilbert.—The Universal Change in Natural Elements: R. Mansill.—Fauna Perthensis, part 1: Lepidoptera: F. Buchanan White, M.D.—Proceedings of the Liverpool Field Club for 1870-71.—Transactions of the Chemical Society of Newcastle-on-Tyne, vol. 1, for 1868-71.—Mechanical Building: G Ryland.—Proceedings of the Geologists' Association for 1870.—A Key to the Natural Orders of British Flowering Plants: T. Baxter.—Natural History Transactions of Northumberland and Durham, vol. iv., part 1.—The Manufacture of Russian Sheet-Iron: J. Percy, M D.—The Quarterly Weather Report of the Meteorological Office.—Transactions of the Norfolk and Norwich Naturalists' Society, 1870-71.—Papers on the Cause of Rain, &c.: G. A. Rowell.

AMERICAN.—Report of the Committee on Building Stores to the Board of Capitol Commissioners of the State of Iowa: Prof. Hinrichs.—The School Laboratory of Physical Science: Prof. G. Hinrichs.—The Principles of Pure Crystallography: Prof. G. Hinrichs.—Third Annual Report on the Noxious, Beneficial, and other Insects of the State of Missouri: C. V. Riley.—Bulletin of the Museum of Comparative Zoology at Harvard College, vol. iii, No. 1.—Preliminary Report on the Vertebrata discovered in the Port Kennedy Bone-Cave: Prof. E. D. Cope.

FOREIGN.—L'Académie des Sciences pendant le siège de Paris: G. G. de Caux, Paris.—Bulletin de l'Académie Imp. des Sciences de St Petersburg, vol. xv., No. 17-31, vol. xvi., No. 1-4.—Ricerche sulla propagazione dell' electricità nei liquidi: Dr. D. Macaluso, Palermo.

CONTENTS

ERRATUM.—Vol. IV. p. 203, first column, line 27, for "503° C." read "50° C."