THURSDAY, NOVEMBER 23, 1871

SCIENCE FOR WOMEN

IN the present condition of the two questions of Science Teaching and of the Higher Education of Women, it may be worth while to regard them for a moment from that point of view in which they coalesce, to inquire, in other words, what is being done for the scientific instruction of women. We do not propose now to argue the question whether it is desirable that women should learn science—that we take to be already decided; but rather to speak of the extent to which, at the present time, provision is being made for carrying out this object. The attention of the public was called to the subject a fortnight ago by the publication of the report of the Syndicate appointed by the University of Cambridge for the examination of women above eighteen years of age in July last. The following are the portions of this report which refer to the various subjects coming within our scope:—

"The answers in the present year in Mathematics show a marked improvement upon those in 1870. The Euclid was decidedly well done, one candidate answering every question except one rider. The conic sections were tried by only two, and without any great success, nothing being attempted in analytical geometry. The algebra was creditably done, but I observe, as I did last year, that while the candidates are fairly skilled in the management of symbols, they seem to have little idea of a logical proof. I should recommend, in this subject, a much more careful study of proofs of rules. The trigonometry, making allowance for the greater intrinsic difficulty of the subject, was better done than the algebra. Statics, astronomy, and dynamics were taken by very few candidates, one of whom, however, showed a knowledge of these subjects small in amount, but thoroughly sound as far as it went. It may be worth while to remark that one candidate, who took in Euclid and algebra only, was the best in each of these subjects.

"In Botany and Zoology the examiner states that the number of candidates was so small as to give little scope for a report. The examination was satisfactory, as far as was possible under the circumstances. One of the candidates passed with distinction. In Geology and Physical Geography the examiner reports as follows:—'No one has done well. The answers are in most cases shallow and full of bad blunders. The examinees seem not to have sufficient acquaintance with the simple laws of physics to make much progress; for instance, it was plain that some did not understand the ordinary laws of evaporation and condensation of vapour, and it seems to me impossible to understand the causes of clouds and rainfall without such preliminary knowledge. There seemed no better foundation laid in geology. More than one confounded Plutonic with Laurentian rocks. No one showed a tolerable acquaintance with the outlines of systematic geology, or any knowledge at all of Palæontology.'"

The report, though in some respects not unsatisfactory, shows how very much still remains to be done before even a fair start can be said to be made in a general training of our women in the elements of Natural and Physical Science. It is therefore with great pleasure that we welcome the attempts, unconnected and imperfect though some of them may be, which are now being made to remedy this defect.

To place the matter on its right footing, it is essentia that the work should be undertaken by the very best teachers we have at our command; and in London at least this is being done in a manner that must in time bring forth good fruit. The classes for women conducted last season at South Kensington by Professors Huxley, Guthrie, and Oliver were attended by large and highly appreciative audiences; and the programme for the present season, already announced by Professors Duncan, Guthrie, and Huxley, is no less attractive. The Ladies' Educational Association of London has wisely confined its teaching to that of the professors of University College, thus affording a guarantee that the instruction shall be of a first-class kind; and now that the whole scientific staff of the College has placed its services at the disposal of the Association, and the Council has given permission for the lectures to be delivered within its walls, with full use of its philosophical apparatus, a scientific training is for the first time offered to ladies on a par with that obtained by its male students. We learn that the classes named in the programme have all been started, and with a fair number of entries. That there is great room for instruction of this kind is shown also by the eagerness with which women take advantage of the opportunity of attending mixed classes wherever they are conducted by men of high repute. We need only refer to the success which has attended Prof. Huxley's lectures at the London Institution in Finsbury Circus, especially as regards the position taken by girls at the examinations in previous years, and to the crowded audiences, consisting at least half of ladies, who are now attending his course on Elementary Physiology.

In the provinces the same work is going on, though hardly with the same degree of organisation. The professors of the University of Cambridge in particular have shown a praiseworthy zeal in the cause, and have offered their time and their services for a more general system of instruction than could be comprised within the lectures which have been given during the last two years at Cambridge itself. We referred last week to the attempt now being made at the College for Women at Hitchin—to be removed, whenever sufficient funds can be obtained, to Cambridge—to inaugurate systematic instruction in Chemistry as an introduction to the other sciences, an attempt to which we heartily wish the success it deserves. When the College for Physical Science was founded at Newcastle, the Council took into consideration a request from a number of ladies of the neighbourhood that women should be admitted to its classes, and decided to make no restriction as to sex in the admission of students or in the rules to which they should be subject. Greatly, however, to the disappointment of the Professors themselves, after all this preparation, when the time came not a single lady presented herself as a pupil. We cannot but think that the ladies of Newcastle were ill-advised in urging the subject upon the Council when there was no actual demand among them for the instruction itself, and thereby giving occasion for unjust reflections on the genuineness of the desire among women for instruction in science.

We wish we could refer with the same satisfaction to the present position of the question in Scotland. The ladies of Edinburgh have shown their high appreciation of the opportunity that has been offered them by several of the Professors of the University for the highest intellectual training, and the Ladies' Educational Association of the Scottish capital has been among the most successful in the kingdom. Emboldened probably by the favour with which the cause of female education was received in Edinburgh, several ladies applied to the University for instruction in a purely medical course of studies; and, the required permission having been obtained, pursued with credit and success the earlier portion of their studies. When they had advanced thus far, however, an unexpected obstacle arose, and the highest governing body of the University, the Senate, stepped in and barred all further progress. The mode, indeed, in which the authorities of the University have played fast and loose with the question of the medical education of women redounds little to their credit. It remains to be seen whether the Council will consent, at the bidding of the Senate, to rescind the regulations which they themselves freely passed in 1869, with the sanction of the Senate, viz. :—

"Women shall be admitted to the study of medicine in the University. The instruction of women for the profession of medicine shall be conducted in separate classes, confined entirely to women. The professors of the Faculty of Medicine shall, for this purpose, be permitted to have separate classes for women. All women attending such classes shall be subject to all the regulations now or at any future time in force in the University as to the matriculation of students, their attendance on classes, examination, or otherwise."

Any proposal for mixed classes of both sexes in purely medical subjects excites so great a repugnance both among the teachers and students of medicine that it would be extremely unwise to press it; but it will be observed that no such question has been raised here, and no such request has ever been made by the lady medical students The best of the medical as well as the general press of London has been almost unanimous in pointing out the undignified position in which the Senate now stands; and it is earnestly to be hoped that wiser counsels will prevail, and that the University will in future pursue a course which will give greater satisfaction to all its best friends.

We noticed with pleasure the large and comprehensive views expressed by Lord Lyttelton when presiding last week over a meeting of the National Union for Improving the Education of Women of all Classes. Lord Lyttelton's position as Chairman of the Endowed Schools' Commission rendered peculiarly important the opinion he expressed as to the misappropriation of the enormous educational endowments of the country to the benefit of male students only.

The extreme importance to all women, as great if not greater than to men, of an acquaintance with the elements of human physiology and of the laws which govern the body in health and sickness, was admirably set forth in an introductory lecture by Prof. Bennett to his ladies' class at Edinburgh, a portion of which will be found in our present number. The advantage which the community, no less than individuals, will gain when some knowledge of Natural and Physical Science is spread throughout our female population, is so obvious that we have no fear but that the movement now happily inaugurated will spread and prosper in spite of temporary checks and disappointments.

ALLEN'S MAMMALS OF FLORIDA

On the Mammals and Winter Birds of East Florida: with an Examination of Certain Assumed Specific Characters in Bird Faunæ of Eastern North America. By J. A. Allen, Cambridge, U.S.A. 1871.

THIS essay forms a portion of the second volume of the "Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass.," in which work Prof. Agassiz and his disciples are giving to the world the results arrived at from the study of the rich collections accumulated during the past few years under their charge. Its author is almost new to the particular branch of zoology which he now enters upon, and puts forward his views in a very decided and uncompromising manner. Yet he has obviously taken great pains in the investigations which have conduced to his results, and has, it must be allowed, to a certain extent, proved his point, although, as is usual with most reformers, he has in some cases pushed his theories too far.

Mr. Allen's paper embraces, as he tells us in his Introduction, "five more or less distinct parts." The first contains remarks on the topography, climate, and fauna of Florida, based principally upon observations made during a three months' expedition to that country in the winter of 1868-9. The second portion contains an annotated list of the Mammals of Eastern Florida. In this list some unusual identifications are made—*e.g.*, the Common American Fox (*Canis fulvus*, auct.) is identified with *Canis vulpes* of Europe, and the American Black Bear (*Ursus americanus*) is considered inseparable from *Ursus arctos*. In Part III. we have the reasons which have led the author to adopt these and similar views as to certain species in the class of birds hitherto considered to be distinct put forward at considerable length. The examination of the extensive series of the common North American Birds in the Museum of Comparative Zoology "has disclosed a hitherto unsuspected range of purely individual differentiation in every species thus far studied. . . . Local or geographical variations have been likewise carefully considered, with results that were a short time since unsuspected. . . . These several lines of investigation have shown that in many instances what have been regarded as reliable characteristics of species have in not a few cases little or no value, that the importance of many diagnostic features has been too highly estimated, and that consequently a careful revision of our published faunæ will be necessary for the elimination of the merely nominal species." To all this every true naturalist will give his cordial assent. We are all for reform and revision, when founded on sufficient evidence. But on turning to Part IV. of our author's work, it would appear that some of his identifications have been based on mere conjecture without any evidence at all. For example: *Quiscalus brachypterus* of Porto Rica and *Q. crassirostris* of Jamaica are placed as synonyms of *Q. purpureus*. Yet it does not appear, or at all events is not stated, that the author has ever examined authentic specimens of the two former species. Again, *Chordeiles texercis* is united to *C. popetue* without any further remark than that "this widely distributed species presents the usual variations in size and colour." Such and similar errors will, we fear, tend to discredit the identifications which Mr. Allen has discreetly made between certain supposed species, of which he has examined a large series of specimens in a most exhaustive and painstaking manner.

In Part V. of his memoir Mr. Allen treats of the geographical distribution of the birds of North America, "with special reference to the number and circumscription of the ornithological faunæ." In this essay, which well merits perusal, although it is evident that the author has never made himself acquainted with some of the most certainly ascertained facts of the general distribution of bird-life,* a new and arbitrary division of the world's surface into eight "realms" is proposed.

The division of North America, however, into its constituent sub-faunæ is fully discussed and well worked out. An appendix to the volume contains a list of authorities to be consulted on the geographical distribution of North American birds, which will be useful, although by no means well arranged. Mr. Allen's knowledge of the geography of Central America seems, moreover, to be somewhat imperfect, as Mr. Salvin's articles on the birds of Veragua are placed under "Guatemala," and papers relating to British Honduras (*i.e.*, Belize), the Republic of Honduras, and Nicaragua, are all confounded under one head. P. L. S.

* *E.g.* The "Neotropical Region" of Sclater, *i.e.*, South and Central America, is divided between two "realms," an "American Tropical" and a "South American Temperate," than which nothing can be more unnatural, and North America is parcelled out into "three realms!"

OUR BOOK SHELF

Sir Isaac Newton's Principia. Reprinted for Sir W. Thomson, LL.D., and Hugh Blackburn, M.A. (Glasgow: Maclehose.)

FINDING that all editions of the Principia are out of print, the Glasgow Professors of Natural Philosophy and of Mathematics have issued a careful reprint of the last (third) edition as finally revised by Newton himself; attending, of course, to the *Corrigenda*, but wisely abstaining from the insertion of either note or comment. We have had far too much of such things. Think only of the painfully elaborate notes of poor Bishop Horsley, which deface an otherwise splendid edition, and of the truly amazing comments made by Lord Brougham in his "Analytical Views!" True, these are coarse attempts at painting, or rather at "whitewashing," while the Glasgow professors are quite able to "gild." But even gilding would have had a smack of profanation about it, and we are delighted to have Newton left to speak for himself in the old, imperishable, words whose full meaning is only now gradually dawning on the world. So far as we have compared it with other copies, this edition seems to be better than any of its predecessors; the printing and paper are excellent, and the cuts especially are greatly improved. There is, however, one remark which is forcibly thrust upon us by this performance. How eccentric and inscrutable are mathematicians! Comets are nothing to them; and the greater they are, the less do they seem subject to any law of what would be called common sense by mere average humanity. One man of exceptional genius is found wasting day after day in neatly rounding off a sonnet; anon he calculates, to fifty places more than can ever be required, the root of some transcendental equation. Others occasionally burst from their seclusion and rush wildly into gymnastic feats, high-jinks, and what not; but in cold blood to determine to verify, letter by letter, a reprint of a somewhat bulky Latin book seems a species of self-torture, of which nothing we ever before heard concerning our northern friends, could have led us to imagine them capable. They have gone through it, however; and, having done it well, deserve our hearty thanks.

Description of an Electrical Telegraph. By Sir Francis Ronalds, F.R.S. (London: Williams and Norgate.)

SIR FRANCIS RONALDS has done well in republishing this portion of his work, which was first printed in 1823. The hope which he expresses in the preface to this reprint that his name "may remain connected with an invention which has conferred incalculable benefits on mankind," is quite justified by the experiments which he made and published many years before the final success of telegraphy. Sir Francis, before 1823, sent intelligible messages through more than eight miles of wire insulated and suspended in the air. His elementary signal was the divergence of the pith balls of a Canton's electrometer produced by the communication of a statical charge to the wire. He used synchronous rotation of lettered dials at each end of the line, and charged the wire at the sending-end whenever the letter to be indicated passed an opening provided in a cover; the electrometer at the far end then diverged, and thus informed the receiver of the message which letter was designated by the sender. The dials never stopped, and any slight want of synchronism was corrected by moving the cover. Hughes' printing instrument is the fully developed form of this rudimentary instrument. A gas pistol was used to draw attention, just as now a bell is rung. The primary idea of reverse currents is to be found where Sir Francis suggests that the wire when charged with positive electricity should discharge not to earth but into a battery negatively charged. Equally interesting is the discussion on what we now call lateral induction, then known as compensation. The author clearly saw that in the underground wires which he suggests as substitutes for aerial lines, this induction would be or might be a cause of retardation. His own words must here be quoted:—"That objection which has seemed to most of those with whom I have conversed on the subject the least obvious, appears to me the most important, therefore I begin with *it*, viz., the probability that the electrical compensation, which would take place in a wire enclosed in glass tubes of many miles in length (the wire acting, as it were, like the interior coating of a battery) *might* amount to the *retention* of a charge, or, at least, might destroy the *suddenness* of a discharge, or, in other words, it might arrive at such a degree as to retain the charge with more or less force, even although the wire were brought into contact with the earth." This passage, written in 1823, is very remarkable, and would alone entitle the author to be mentioned in any history of underground or submarine telegraphs. Testing-boxes were invented by Sir Francis, and a code is suggested by him. If these things had been mere suggestions they would have been remarkable, but accompanied by practical experiments proving that the scheme could be carried out, they ought to connect his name permanently with the history of the Electric Telegraph. F. J.

LETTERS TO THE EDITOR

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

Oceanic Circulation

ON returning from my second Mediterranean cruise, I find that Mr. Croll has published in the *Philosophical Magazine* his promised demonstration of the theoretical impossibility of the production of under-currents by gravitation, according to the doctrine which I have advocated with reference to—

1. The Gibraltar Current.
2. The Baltic and Black Sea Currents.
3. The General Oceanic Circulation.

At the same time I find awaiting me a very important treatise on the Physics of the Baltic ("Untersuchungen über Physikalische Verhältnisse des Westlichen Theiles der Ostsee") by Dr. H. A. Meyer, of Kiel, containing the results of a continuous series of most careful and systematic observations on the temperature, specific gravity, and movement of the different strata of its water, dating back to the spring of 1868. With this work I received a letter from its author, of wh ch the following extracts will, I think, be interesting to your readers:—

"I have followed with special attention the splendid results of your different voyages, and hope that the experience which I have gathered on a more confined area may yet offer something which you may deem worth your attention. The favourable opportunity which I enjoyed for continuing regular observations at a spot where the waters of the North Sea mingle with those of the Baltic, enabled me to collect matters which cannot be brought together on sea-voyages only; and I should be much pleased to see similar work undertaken at Gibraltar and Constantinople. If among your large circle of acquaintance you might know of gentlemen who may be interested in this cause, I should be happy to send them my book.

"I regularly read NATURE, and am much surprised to find that your views on Ocean-currents should not be universally accepted. How one can suppose that such a vast force which constantly acts in one direction should remain without any influence whatever, is perfectly incomprehensible to me!

"Most probably the cold under-current coming from the pole will be—wherever it is not very confined—very slow; but I doubt not that, should you consider it of sufficient importance, you will succeed in proving that the current, when confined, is pretty fast, that is to say, fast enough to be measured by the instrument which you used in the Straits of Gibraltar.

"With a similar appliance, which I have used for years, and which you will find figured in my work, I have lately been able to trace the heavier under-current in the Baltic to a much greater distance. On board one of the despatch boats of the German Navy, accompanied by some friends, I have this summer made several trips through the Cattegat and Skager Rack, and into the eastern parts of the Baltic; and my views have been everywhere confirmed."

I have further to state that my prediction that a similar under-current of dense water must pass through the Dardanelles and the Bosphorus from the Ægean into the Black Sea, which, it has been alleged by Captain Spratt, is disproved by experiments made by him several years ago, is regarded by three of the ablest of our Hydrographers to be conclusively proved by those very experiments when rightly interpreted. This I shall shortly demonstrate in an appendix to the forthcoming Report of my recent cruise.

The case between Mr. Croll and myself, therefore, stands thus:—

1. I have experimentally proved the existence of an *outward* under-current in the Straits of Gibraltar, and have adopted the gravitation theory of Captain Maury as affording an adequate account of it.

2. I have shown that this gravitation theory is applicable, *mutatis mutandis*, to the converse cases of the Baltic and Black Sea *inward* under-currents, the existence of which has been experimentally demonstrated.

I have further shown that it is applicable to that general Oceanic Circulation, the evidence of which appears to me to be afforded by the aggregate of observations that indicate the prevalence of a temperature not far above 32° on the deep ocean-bottom, even under the equator, and by the intermediate soundings which indicate the existence of two distinct strata, separated by a "stratum of intermixture," in parts of the deep ocean which the Gulf Stream assuredly does not reach.

These views have been accepted by Physicists of the highest eminence; but, as Mr. Croll affirms, without due consideration of their theoretical difficulties. I venture to suggest, however, that it is not beyond the range of possibility that Mr. Croll's data may be erroneous; and I do so with the more confidence, because I have been assured by first-rate Mathematicians that the science of Hydro-dynamics has not yet attained a development which would justify the assertion, that (to use Dr. Meyer's words) "a vast force constantly acting in one direction remains without any influence whatever."

It happens that I very early became impressed with the power of very small differences in Temperature to produce currents in liquids, by the following remarkable fact, which has never (so far as I am aware) been published. More than thirty years ago Mr. West of Bristol (where I then resided) built an observatory on Clifton Down, the principal instrument of which was intended to be a refracting telescope of large aperture, the object-glass of which was to be made on the plan of Mr. Peter Barlow; the double concave of flint being replaced by sulphuret of carbon, or some other liquid of great dispersive power. The object-glass was constructed with the greatest care, Mr. Barlow kindly assisting in the computation of the requisite curves; but when tried it was found to be practically useless, in consequence of the movement produced in the liquid by the very minute differences of temperature occasioned by air-currents striking the surface of the outer lens.

I would also direct the attention of your readers to the very interesting paper by Prof. Karl Möbius, the coadjutor of Dr. Meyer, "On the Source of the Nourishment of the Animals of the Deep Seas," of which a translation will be found in the "Annals of Natural History" for September. Careful and prolonged observation of the movements of organic particles in aquaria satisfied him that very slight changes of temperature have a very important effect in producing changes in the stratification, so to speak, of the water; in one instance, he says, "a downward current, which readily carried organic bodies along with it, was produced when the difference between the superficial and bottom temperatures had scarcely attained half a degree of Reaumur (1°·1 Fahr.)."

Such being the facts of the case, and Mr. Croll having offered no explanation of them, whilst demonstrating to his own satisfaction that the explanation I advocate is untenable, I do not feel called upon to discuss the subject further. There can be no reasonable doubt that, within the next few years, a great mass of additional data will be collected, which will afford adequate materials for the construction of a definite Physical Theory, by Mathematicians fully competent to the task. At present I do not pretend to have done more than offer a hypothesis which accords with the facts at present known, and with what Sir John Herschel called the "common sense of the matter."

Nov. 14 WILLIAM B. CARPENTER

The Solar Parallax

IF Mr. Proctor had printed in full my memoranda on the errors and imperfections of his history of the solar parallax, or if he had said nothing about it, I should have said nothing more in defence of my review. But, in NATURE of September 28, he gives so inadequate an account of my notes, hiding the point of the most remarkable of his inaccuracies, and ignoring the imperfections entirely, that I am compelled in self-defence to explain. In describing the various discussions of the Transit of Venus which preceded that of Mr. Stone, he says (p. 61): "Newcomb, of America, was more successful. He deduced the value 8″·87 by a method altogether more satisfactory than Powalky's. But still the agreement between the different observations was not so satisfactory as could be wished, nor had Newcomb adopted any fixed rule for interpreting the observations of internal contact, which, as I have said, are affected by the peculiar distortion of Venus's disc at that moment."

To express my appreciation of this compliment it is only necessary to say that I have no recollection of having discussed the past transits of Venus at all, beyond correcting what I supposed to be an oversight in Mr. Stone's paper, and I am still utterly at a loss to know on what ground the compliment is based. In his letter he tries to throw the responsibility upon an anonymous correspondent of the Astronomical Register, which I regret to say does not circulate here, but he does not quote anything to justify a single statement in the preceding paragraph. The correspondent says nothing about 8″·87, which, it will be noted, is Mr. Petrie's pyramid value, nor about my treatment of contacts, so far as quoted by Mr. Proctor, so that I am as much in the dark as ever.

We have all heard suspicions that critics sometimes review books without reading them, but this is the first time I remember to have seen so circumstantial a description of a work which never existed, save in the writer's imagination. I really cannot help viewing it as something "remarkable" when coming from a writer of Mr. Proctor's accuracy and erudition, and must beg pardon if I measure his writings by too high a standard.

The imperfections consist briefly in the regularity with which the more recent and complete researches on the solar parallax are ignored, incorrectly given, or placed in the back-ground of older and less complete ones. If any one wants to satisfy himself of this, he has only to look at the papers and discussions which have appeared in the *Comptes Rendus*, the Monthly Notices, and the German "Vierteljahrsschrift des Astronomischen Gesellschaft" within the past four or five years, and see that only a single one of them all is expressly mentioned, and to note the values of the parallax adopted in the astronomical ephemerides of France, Spain, Portugal, and Germany, and see that not one of them can be traced in Mr. Proctor's history. If as he once said, he had not room to describe the recent researches, I should have supposed he would have condensed or omitted the older ones, which these recent ones have superseded, instead of doing the contrary. The importance of this matter arises from the fact that these discussions and researches put a different face on a number of questions connected with the determination of the solar parallax from that given by Mr. Proctor, and I do not think the latter can successfully argue that the astronomical world of to-day is nearly all wrong in the views to which it has been led by five years of discussion, experiment, and research.

On Nos. 3, 4, and 7, of Mr. Proctor's defence, it is only needful to remark (1) that I did not write No. 3 till I had verified Foucault's result by a careful calculation not made on my thumb-nail; (2) that Mr. Proctor leaves it to be logically inferred that the discussion alluded to in No. 4 was an unpublished one; (3) that, having disclaimed my interpretation of No. 7, his book gives no explanation of the reason why Mr. Stone's parallax was so much greater than those of Encke and Ferrer. It is only necessary to refer to the paper of the latter in vol. v. of the Memoirs of the Royal Astronomical Society at pages 254 and 264 to find a very full discussion of the apparent, and the so-called true contacts.

No. 6 involves one of the most important questions connected with the determination of the solar parallax from transits of Venus, and I am sorry to see that Mr. Proctor simply evades the issue, as the misinterpretation to which he refers consists in supposing him less erroneous than he really is. Let one make a drawing representing the limbs of Venus and the sun in mathematical contact. On each side of the point of tangency there will be an exceedingly thin thread of light, vanishing at that point. How much of this thread will be visible by an ordinary telescope? We must remember that the sun is viewed through a dark glass, which reduces its light to that of an ordinarily illuminated object. The narrowest visible line so illuminated subtends an angle of about 20". With a power of 120 this would correspond to a breadth of one-sixth of a second. But it is well known that atmospheric tremors, and, with most old instruments, imperfect corrections of the object-glass, prevent our seeing an object at all approaching the minimum visible, and that the same cause prevents the increase of magnifying power from giving a corresponding increase of seeing power. It is probable that the thickness of the least visible thread may have varied with the telescope, the observer, the dark glass, and the atmosphere, from one or two tenths of a second to one or even two seconds. Let us take the more favourable cases in which a thread of 0"·2 is visible. A simple calculation will show that there is a space of 3"·4 on each side of the point of tangency, in which the thread will be thinner than this, and therefore invisible, and that the visible cusps will be about 7" apart. How different this 7" from Mr. Proctor's invisibly thin ligament! This explains the observations of Wolf and André, who found that the black drop when seen at all continued after internal contact at ingress and preceded it at egress.

In answer to Mr. Proctor's letter of October 5, I beg leave to reply, if the "fringe" is something actually produced by the telescope or the atmosphere, it is simply bad definition. If it is not so produced, it is an optical illusion, of which the laws are obscure, and the very existence problematical under the circumstances in question. See, for instance, the celebrated paper of Prof. Baden Powell on Irradiation. Mr. Proctor's intimation that the great mass of astronomers who have observed transits of Mercury within the past forty years, among whom are included Bessel, Airy, and the Struves, were careless and inferior observers, because they did not see an optical illusion according to his view of it, is as good a *reductio ad absurdum* of his theory as I could ask for. It is comforting to know that one of his careless observers can be turned into a careful and attentive one by giving him a telescope with plenty of irradiating power.

To prevent misapprehension, allow me to say that the theory set forth in my letter of September 28 is in no way my own, but was promulgated by Bessel nearly forty years ago, and has, I believe, been since universally received on the continent of Europe. SIMON NEWCOMB

Washington, Oct. 23

The Aurora of Nov. 9th and 10th

I WITNESSED on the night of Nov. 9, at about 7.30 P.M., an aurora which, for symmetry of form and other features, was very remarkable; and unless, as is very likely, some more able observer has already sent you a description of it, you may like to put my account on record.

In the magnetic north horizon was the usual segment of auroral light, very brilliant, and stretching considerably to the east and west, its altitude being 20° or more. High above this, and extending in a complete arch from the east to the west horizon, was a remarkable and well-defined band of still brighter light, about 7° in breadth, and passing about 30° from the zenith.

Filling the space between these two arcs of light was what I can call nothing else than a dark shadow, which had somewhat of a mysterious appearance; for, though decidedly darker to the eye than other parts of the heavens, it did not in the least obscure even small stars, nor do I think this darkness was the effect of contrast. In this dark space faint auroral streamers occasionally shot up to the upper arch, but did not pass it. This shadow was what the French observers speak of as the *nuée*.

The light of the upper arched band was silvery, and increased much in intensity towards the horizon both east and west; the points of greatest intensity being about 5° above the horizon, as would be expected in the direction in which the arch appeared most foreshortened.

While watching this phenomenon I was impressed by the conviction that, to an observer in space, the north magnetic pole of our planet would have presented the appearance of being surmounted by a symmetrical cap of light, streaked by one or more bands, and terminated at its greatest distance from the pole by a well-defined brilliant margin.

In the hope that an observer in some other locality might have made similar observations, I was preparing to measure the distance of the upper arch of light from the zenith, as well as the positions in azimuth of the points where it touched the horizon, when the whole phenomenon was obscured by dense clouds.

Stretton Rectory, Hereford, Nov. 15 H. C. KEY

THE following brief extract from our observatory note book may be interesting:—

"Nov. 10.—For about 20° on each side of north, at 9.30—9 40 P.M., brilliant *waves* of light followed one another rapidly, from two to four in a second, moving upwards, following the direction of the streamers, fading away at about 40° from the horizon. Three or four waves could be seen at once, measuring about 5° to 8° by estimation, from crest to crest. . . ."

I heard some of the boys remark "How close it must be; it looks like puffs of steam from behind those houses."

Rugby J. M. W.

AS none of your correspondents who described the brilliant auroræ of Nov. 9th and 10th last week, speak of their being seen earlier than from 7 to 10 P.M., it may be interesting to note that in the Midland Counties the latter was visible at a considerably earlier period of the evening. On the evening of the 10th I was walking from Reading in Berkshire to Caversham in Oxfordshire, from 5.45 to 6.5 P.M. During the whole of that time I had before me the steady *white* light of the coming aurora, extending perhaps 25° to 30° in width, and 20° in height, its centre being immediately beneath Polaris. The appearance was exactly that of the departing twilight in a clear winter sky, for which, indeed, but for its position and the time of the evening, it would have been mistaken. As I noticed the light immediately on leaving the railway station above the lights of the town, I have little doubt that it had been visible since sunset. I had no opportunity of watching its progress after 6.5 P.M.; up to that time there were no coloured streamers, nothing but the white light I have described.

ALFRED W. BENNETT

The Ghost of Flamstead

I OUGHT earlier to have thanked this venerated shade for a communication which will enable me to correct (at some future time) an omission in my treatise on the Sun. Let me hasten to assure him (or it), however, that the omission has been in no way connected with those "queer notions of honour, and justice, and fairness," which he conceives to be rife in our times. Why should I seek to wrong the honoured dead? And who would gain in this case by the injustice? The present Astronomer Royal? Surely no. To add this small matter to his real claims to our esteem would be

To gild refined gold, to paint the lily,
And throw a perfume on the violet.

Neither, I am sure, has any other writer who has overlooked Flamstead's claims, desired to do him injustice. On this point I would merely remark, "Rest, rest, perturbed spirit."

But now "we'll shift our ground," by the Ghost's good leave. Our visitor from Valhalla remarks that "a stir was lately made about what was represented as a new method of investigating the motion of the solar system in space, and instead of a new there was brought forward an old acquaintance (known to Science since the times of our grandfathers)." Here the spirit of Flamstead refers obviously to the Astronomer Royal's method. I am sure that Prof. Airy would desire greatly that if his method be indeed so ancient, the fact should be made widely known. I myself am particularly anxious to be set right on this point, about which I am at this very time writing. For though I care more about explaining this and the other methods than about their history, yet it is desirable to be accurate even in historical details.

If I may say so without offence, I would remark that a ghost was not needed—certainly not the ghost of the first Astronomer Royal—to teach astronomers that the opposition of Mars in 1877 will be exceptionally important. At page 25 of my "Sun" I have already pointed this out, and I dare say others have done likewise.

I hope the "great injustice" to which our ghostly correspondent refers as endured by him in life, does not relate to his difficulties with Newton, for at the present time the opinion of Brewster on this point is in vogue—not Baily's; and the warmest admirers of Flamstead are those who least desire to moot the subject. R. A. PROCTOR

Brighton, Nov. 4

Creators of Science

PERMIT me to do my little towards clearing up a most unfortunate confusion of thought respecting the intellectual ranks of mathematicians and metaphysicians, which is, in my experience, widely prevalent. We may safely divide the mathematicians into three orders:—(1) Inventors, (2) Experts, (3) Readers or Students, so as to discriminate from one another those who create systems, those who manipulate with them, as "ministers and interpreters of nature"—just as easily and familiarly as Professor Tait (*e.g.*) employs and applies the theory of Quaternions—and those who have merely studied into an understanding of an author or subject. It was an expedient of the late Sir William Stirling Hamilton to confound all these orders, and from the heterogeneous lump to extract—if not extort—testimonies to the worthlessness of mathematics as a mental discipline, without the least discrimination of their sources.

On the other hand, the metaphysicians cannot be trichotomised; for, even in the present advanced state of metaphysics, there is no class of philosophers corresponding to the mathematical experts, the reason of which explains why examiners in mental science do not set problems. There are, in fact, only two classes of metaphysicians: I., Creators; II., Students, more or less thoroughly versed in the systems of the leaders, and more or less accepting or rejecting, with more or less reason, those creations. Accordingly, when on May 17, 1869 (I *think* that was the date), Professor Tait, at a meeting of the Royal Society of Edinburgh, challenged the metaphysical world to produce a metaphysician who was also a mathematician, he not being able at the moment to call to mind a single instance, he was to be understood as asking for a person of the order 1, who was also in the class I. Professor Calderwood's reply, then, was not wholly unexceptionable, for of the three names he adduced, viz., Descartes, Leibnitz, and Hegel, the last was that of a reader of mathematics, and not of a mathematical inventor. The challenger might have spared the respondent the trouble of reply, had he known what De Morgan wrote in Notes and Queries, 2nd S. vi. 293–4, where are distinguished five mathematical inventors, as *facile principes*: viz., Archimedes, Galileo, Descartes, Leibnitz, and Newton; and in which Aristotle, Plato, and D'Alembert are allowed a very high rank in mathematics. Had the inventor of Quaternions been then dead, I have little doubt that De Morgan would have added to the five the name of Sir William Rowan Hamilton, who, besides being a mathematical inventor of the very first rank, was also a diligent and accomplished student of Plato, Kant, Reid, and the other Hamilton, and a writer on Logic; *i.e.*, as good as D'Alembert as a philosopher, and perhaps better than he as a mathematician. Now, it is not a little curious and very instructive to observe that, *pace Platonis*, the two who were creators of strictly defined metaphysical systems, viz., Descartes and Leibnitz, are the only two among the five metaphysicians adduced by De Morgan who belong to the highest rank as mathematical inventors.

It is quite incredible that a man of Professor Tait's learning (I say here nothing of his judgment) should not have been aware of the identity of Descartes (the poor dreamer!) and Cartes, the founder of the Cartesian Geometry; still more so that he should not have known that the immortal analyst, the co-inventor of the Differential Calculus, was the most eminent metaphysician native to Germany before Kant. It was, then, not "ignorance," but "ignoration," on the part of the Scotch mathematician, that was involved in his challenge; and that challenge was doubtless intended as mere *badinage*, at the expense of a science which he had taken no pains to understand.

Be that as it may, I trust I am not singular in adjudging (as De Morgan did) these two grand intellectual pursuits to be worthy of being cultivated together, and to be able to give material aid to each other. For myself, I cannot but look upon any man as the enemy of intellectual progress, who delights in setting the one class of investigators against the other, and endeavours to prolong the controversy which has raged between them since the "Principia" was promulgated.

Highgate, Nov. 8 C. M. INGLEBY

Descartes' "Animated Machines"

AS you open your valuable columns to philosophical discussions, may I request you to publish the following remarks on a passage in Mr. Lewes's popular "History of Philosophy" (Vol. ii. p. 148 of the new edition), where he confesses himself puzzled, along with other critics, to account for Descartes' theory that animals were only *animated machines*. "I am not prepared," he says, "with a satisfactory explanation." I cannot but think that a careful perusal of the "Discourse on Method" (Part 5, *sub. fin.*) and of the treatise on *les Passions de l'âme*, makes Descartes' reasons perfectly clear. In the first place, the use of the word *machine* has misled most of his critics, and if the story of Malebranche and his dog be true, even this great disciple had grievously mistaken the principles of his master. For in the last-named treatise Descartes endeavours to show that such feelings as joy, grief, fear, &c., though in us accompanied by really mental acts (*pensées*), are produced by physical causes, and produce physical effects apart from the mind. Descartes would therefore never have denied to brutes any of the bodily sensibilities which we possess; and says expressly that he calls them machines in a special sense—machines made by the Deity, and therefore infinitely more subtle and perfect than any which we can construct. He says that we could not ourselves be ranked higher in the scale of beings did we not possess the gift of *language*, the phenomena of which can only be accounted for by an internal principle different in kind from those which appear to guide the lower animals, though there are also those passions in us which we have in common with them.

But to come to the psychological reasons for the theory. Historians of philosophy before the 18th century should be particularly alive to theological *idola*, even in sceptical writers; much more so in good Catholics like Descartes. Just as Berkeley put forward prominently the theological advantages of his Idealism, so Descartes indicates plainly in his "Discourse on Method" (*loc. cit.*) that these were the chief reasons of his theory. "Next to the error of those who deny the Deity, which I have already refuted, there is none more apt to seduce feeble minds *from the path of virtue* than to imagine that the soul of beasts is the same as ours." But the *locus classicus* has, I think, escaped Mr. Lewis, and will be found in a letter to a Lord (supposed to be the Duke of Newcastle), the 54th of the 1st volume in the original quarto edition. Descartes there specially answers objections made to him on this point, and in the way above indicated; adding however the following passage: "Yet it may be said that although the beasts perform no action which convinces us that they think, nevertheless, as the organs of their bodies do not differ much from ours, it may be conjectured that some sort of thought is joined to these organs, such as we experience in ourselves, but much less perfect;

to which I have no reply to make, except that if they thought as we do, they must have an immortal soul as we have, which is not likely, as we should apply the argument to *all* animals, such as sponges, oysters," &c. I am sure these ideas are not unfrequently repeated in his correspondence, as for example, in one of his replies to Morus (vol. i. No. 67 of the 4to edition, in Cousin's Edition, x. p. 204 *et seq.*). He there even talks of two souls, an *âme corporelle* which is the cause of passions and affections, and an incorporeal principle of thought, which he elsewhere says was infused by the Deity into man at the first moment of his existence. He also observes, I think logically enough, that as no boundary line can be drawn elsewhere, we have no choice between conceding a soul to oysters or refusing it to all animals save man. I am not however concerned to defend the validity of his reasons, but rather to contribute this information as an historical point of interest.

Trin. Coll., Dublin, Nov. 11 J. P. MAHAFFY

Plane-Direction

I THINK "plane-direction" is the best of the competing names. The planes of cleavage in a crystal are the "plane-directions" in which it is most easily split. They cannot be called either "aspects" or "positions." The opposite faces of a cube certainly cannot be said to have the same "aspect."

If a rigid body receives a movement of translation, it retains something unchanged. What is this something to be called? It might be called "lie" or "set," but both names are equivocal. Two equal and similar figures possessing this something in common might be very well described as "similarly laid," "similarly set," or "similarly placed." We may say that they have "similar positions," but we can scarcely say that they have "the same position;" for change of position is commonly held to include movements of translation as well as of rotation, and a point is usually defined as having position but not magnitude. I think it is worth while to consider whether "position" cannot be restricted to the more limited sense, "place" being employed in the wider sense.

I wonder that no one has yet raised a murmur against the proposition itself, which your correspondents are so anxious to render literally into English. It appears to me that the plain English form in which Mr. Wilson first stated it is clearer and more precise than the German abridgement. In the strictest sense of "determine," one "Richtung" determines one "Stellung" and one "Stellung" determines one "Richtung," inasmuch as to one plane-direction there corresponds one normal direction.

In a special sense it is true that two "Richtungs" determine a third (perpendicular to them both), and that two "Stellungs" determine a third (also perpendicular to both); just as two points may be said to determine one plane (bisecting their joining line at right angles). In all these instances the fact is that not one only but many are "determined," but all except one come out in pairs or multiples of two. It is this one, which has no fellow, that is in a special sense "determined."

I think it is paradoxical and misleading to state, without qualifying words, that two linean directions determine one plane-direction; inasmuch as two linean directions really serve to define as many different pairs or multiple pairs of plane-directions as we please, and if we are permitted to distinguish the two linean directions by dtfferent names, three plane directions can be separately defined by them without any ambiguity. Similar remarks, of course, apply to the other half of the proposition.

J. D. EVERETT

Rushmere, Malone Road, Belfast, Nov. 11

"Wormell's Mechanics"

WILL you do me the favour of inserting a brief reply to the few remarks made concerning the above text-book in last week's NATURE?

1. On page 8 of the book occurs an explanation of what is usually termed the transmissibility of force, and a statement of the axiomatic principle that we may imagine a force to be applied at any point in the line of its direction, *provided this point be rigidly connected with the first point of application*. On page 14 a deduction from this principle is made and employed to prove the rule for finding the directions of the resultant of two forces acting on a point. The reviewer says that this deduction, "if true, would assert that the attraction of the sun and the earth upon the moon might be transferred to any heavenly body in space which happened to be in the line of direction of the resultant of the forces." If the restriction laid down with emphasis in the book, and printed in italics as quoted above, be not ignored, this is a legitimate inference, and if the point to which the forces are transferred parallel to themselves be rigidly connected with the moon, any conclusion having reference to the magnitude or direction of the resultant action on the moon derived as a consequence of the imaginary transposition of the point of application of the forces will be correct.

2. In finding the direction of the resultant of two parallel forces, the same transposition of the point of application is employed, and, of course, it is understood with the same proviso. This proof your reviewer qualifies as "meaningless," whereas I feel sure that, taken in connection with the original axiom and the deduction above referred to, it would be accepted by any mathematician as both intelligible and correct.

3. The next statement is that the definition of a rigid body is given as a property of forces. This is not so, but the whole theory of statics, when developed independently of dynamics, rests on the properties of a force and the properties of a rigid body jointly.

4. The reviewer next dwells upon a curious error which unfortunately escaped my notice until it was pointed out but a short time ago by a schoolboy, and which forms one of three corrections on a slip of errata. Any student would, however, have been able to make the correction for himself by the help of the preceding pages and the applications to the following exercises, a circumstance which I think an unprejudiced critic should not have overlooked.

5. Your reviewer next remarks that a student who tries an experiment with a block and tackle would naturally be surprised at finding that the result of experiment does not agree with that of the theory, and adds, "nor can we find a single word in the book which would enlighten his difficulty." The reviewer cannot have read section 71.

6. The subjects included in the book are such as comprise the course described in the curriculum and examination papers of the University of London, and if occasionally the discussion of unpractical arrangements of mechanical powers is required, I am not answerable. Indeed, I hope to see the day when a reform of this part of the curriculum will necessitate my rewriting the work on an entirely different plan, namely, one according to which kinematics forms the first part, kinetics the second, and statics the third, the propositions of the third part being special cases of those of the second. But that at present it answers the purpose for which it is intended, is proved by the fact that all the questions set this year can be answered from it.

So far as most of the facts and illustrations are concerned, "I am but a gatherer and disposer of other men's stuff," and a writer of an elementary text-book to suit the requirements of a particular examination could not easily be more.

The tone of depreciation with which the writer of the article has been pleased to refer to the work, so directly opposed to a previous notice of the same book in the same journal, seemed to me to call for some reply, and I should wish to describe more fully the objects I have aimed at in compiling the work, but that I know I have already taken up enough of your valuable space.

RICHARD WORMELL

ONE OF THE GREATEST DIFFICULTIES OF THE DARWINIAN THEORY

SIR JOHN LUBBOCK has done good service to science in directing attention to the metamorphoses of insects, by admitting freely the great difficulty in conceiving "by what natural process an insect with a suctorial mouth, like that of a gnat or butterfly, could be developed from a powerful mandibulate type like the Orthoptera, or even from that of the Neuroptera" (NATURE for Nov. 9, page 28). Such "difficulties" have struck many from the first, and it is in no small degree encouraging to those who love the liberty of science, to find that the time is approaching when difficulties may be brought under consideration and discussion.

"There are," Sir John Lubbock remarks, "peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva, and suctorial as an imago." The power of mastication during the first period of life being an advantage, on account of a certain kind of food being abundant, and that of suction during the second, when another kind of food prevailed, or *vice versâ*, is suggested as a possible explanation of the origin of species which are mandibulate during one period of life and not during another. In such cases it is said we have "two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions." It is suggested that the change from one condition to the other would take place "contemporaneously" with a change of skin. Then it is urged that even when there is no change of form, the softness of the organs precludes the insect from feeding for a time, and when any considerable change was involved, "this period of fasting, it is remarked, would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two."

There is much that is assumed in this reasoning; but I shall now venture to call the attention of naturalists to one point only, namely, the analogy between the period of fasting caused by the temporary softness of the organs while the caterpillar changes its skin, and the more prolonged fasting period when the organs undergo that more considerable(!) modification involved in the change from the mandibulate to the suctorial type of mouth. The change from a small mandibular apparatus to a larger one seems to be compared with the change from a mandibular to a suctorial apparatus—the change of skin of the caterpillar with the change of skin when the caterpillar becomes the pupa, and the latter the imago—the temporary softness which prevails when the little mandibles grow into bigger mandibles, with the temporary softness which prevails while the mandibles become converted (!) into the suctorial mouth. But these changes are surely of different orders, and the operations of a different nature. The mandibles do not change. The one type of mouth does not pass through gradations of any kind into the other kind of mouth. But one abruptly ceases, its work having been discharged, while the other is developed anew. As compared with the change of skin of the caterpillar, the change of skin from chrysalis to butterfly is indeed a "*considerable change*." It would require an amazing intelligence to premise from the study of a caterpillar that from it, after certain changes of skin and periods of rest, would emanate a butterfly.

It is very well to suggest that "in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth"; but what, I would ask, is the evidence of the connection implied? Between the *change* from the small mandibles to the large, and the *change* from the latter to the suctorial apparatus, there can be no comparison—no analogy, for the suctorial mouth is developed anew during the pupa state, and its formation is not commenced until all traces of the mandibles are gone. Nay, every tissue of the caterpillar disappears before the development of the new tissues of the imago is commenced. The muscular and nervous systems of the latter are as different from those of the former as are the digestive apparatus, the oral mechanism, and the external covering. These organs do not change from one into the other: but one, having performed its work, dies, and is removed entirely. Not a vestige of it remains. Its place is occupied by formless living matter, like that of which the embryo in its early stages of development is composed; and from this *formless matter* are developed all the new organs so marvellously unlike those that preceded them; and others unrepresented at all in the larval stage, make their appearance. To explain, according to Mr. Darwin's theory, the "period of change and quiescence" intermediate between the caterpillar and imago states of existence, is likely to remain for some time a very difficult task. If the difficulty cannot be resolved until the period of quiescence during which the imago is formed, is proved to be analogous to the periods of quiescence during the change of skin of the larva, the life history of a butterfly will remain for a long time a puzzle to Mr. Darwin and those who believe in the universal application of his views.

LIONEL S. BEALE

ON THE RECURRENCE OF GLACIAL PHENOMENA DURING GREAT CONTINENTAL EPOCHS

IN the August number of the Geological Society of London I published two papers "On the Physical Relations of the New Red Marl, Rhætic Beds, and Lower Lias," and "On the Red Rocks of England of older date than the Trias." In the latter I attempted to prove that for the north of Europe and some other parts of the world, a great Continental epoch prevailed between the close of the upper Silurian times and the end of the Trias or commencement of the deposition of the Rhætic beds; in other words, that the Old Red sandstone, Carboniferous strata, Permian beds, and New Red series were chiefly formed under terrestrial conditions, all, with the exception of the Carboniferous series, in great lakes and inland seas, salt or fresh.

The Permian strata, in particular, appear to have been deposited under conditions to which the salt lakes in the great area of inland drainage of Central Asia afford the nearest modern parallel.

While brooding over the whole of this subject for several years past, I have often been led to consider its bearing on those recurrent phenomena of glacial epochs which now begin to be received by many geologists.

The phenomena of moraine-matter, scratched stones, and erratic boulders, whether deposited on land by the agency of glaciers, or in the sea and lakes by help of floating ice, are evidently intimately connected with the contemporary occurrence of large areas of land, much of which may, or probably must, have been mountainous.

The late Mr. Cumming, in his History of the Isle of Man, "hints at the glacial origin of certain Old Red conglomerates in that island, conceiving that the bony external skeletons of some of the fish of the period may have been provided to enable them to battle with floating ice. In lectures and in print I have frequently stated my belief that the brecciated subangular conglomerates and boulder beds of the Old Red sandstone of Scotland and the north of England are of glacial origin, so distinct, indeed, that when these masses and our recent boulder clay come together, there is often actual difficulty in drawing a line of demarcation between them. I frequently felt this difficulty years ago, when, commencing the Geological Survey of Scotland, I mapped the strata in the country south of Dunbar, and the same difficulty was occasionally felt by others in the valley of the Lune, near Kirkby Lonsdale.

If, as I believe, the Old Red sandstone was deposited in inland Continental waters, the Grampians, as a mountain tract, bordered these waters, and they must have been much higher then than now; not only because of the probably greater elevation of the whole continent, but also because the Grampians formed land during the whole of the Upper Silurian epoch, and suffered great waste by denudation, then and ever since. The glaciers of these mountains marked an episode in Old Red sandstone times, and yielded much of the material of the boulder beds of the Old Red sandstone.

In these regions and in North America, the Carboniferous strata were evidently formed under the influence of "a warm, equable and moist climate," and I know of no glacial phenomena in connection with this epoch

But respecting Permian times I attempted in 1855 to prove the existence of ice-borne boulder beds during part of that epoch, and by degrees this opinion has been more or less adopted. These boulder beds were derived by glacial transport from the mountains of Wales, which then, also, were necessarily much higher than now. As the Old Red boulder beds were formed during a glacial episode or episodes of parts of that epoch, so the Permian boulders mark another glacial episode occupying part of Permian time, just as our last great glacial epoch formed an episode in those late Tertiary times of which the present time forms a part. At the time of the publication of this paper, I conceived the Permian boulders to have been deposited in the sea by the agency of icebergs, but I now consider them to have been deposited in inland lakes.

This, if true, formed a second glacial epoch, of unknown intensity, during the long continental period that lay between the close of Upper Silurian and the beginning of Liassic times.

During the Triassic period there is no certain sign of glacial phenomena in the British area.

I have elsewhere attempted to show that at the present day there is an intimate connection between past glacial phenomena and the occurrence of lakes, large and small, many of which are true rock-bound basins.

I further believe that this cause would be found to characterise ancient Continental recurrent glacial epochs through all past time, if perfect data were accessible, or had been preserved from destruction by denudation and disturbance of strata. In the Palæozoic cases mentioned above, there is, in my opinion, an evident connection of some kind between inland lakes and glacial action, and in stating this it must be borne in mind that I do not consider the Old Red and Permian strata of Britain to have been deposited solely in two lake basins during two epochs, but in various basins during each of two special eras of geological time. For example, the Magnesian Limestone beds of Yorkshire and Northumberland were formed in a hollow quite distinct from the great conglomerates (locally called "brockram") and sandstones of the Vale of Eden. Prof. Harkness in 1856* showed that in the South of Scotland Permian beds, partly formed of brecciated conglomerates, lie in rocky hollows entirely surrounded by lips of Silurian and Carboniferous strata, in fact, in rock basins; and he attributed this singular circumstance to a sinking in of the Silurian strata in each case underneath the Permian rocks.

Ever since the publication of my paper, in 1862, on the Glacial origin of certain lakes in rock basins, I have suspected that these Permian rock basins may also have been scooped out by the agency of glacier ice. I connect this view with my paper on Permian glaciers, published in the *Geological Journal* in 1855, but as I have not yet seen the country where these hollows lie, I have not been able either to verify or disprove this supposition. I expect, however, that some day this view will be proved, not for these areas alone, but for others of larger area and very different date, which as yet I have only partially examined, in other European countries.

The unravelling of nearly all stratigraphical phenomena of every geological age resolves itself simply into attempts to realise ancient physical geographies, and we may rest assured that those forces that are now in action have played their part in the world sometimes with greater, sometimes with less intensity, through all known geological time, as far as it can be studied by an examination of the rocks that form the crust of the earth. If glacier ice scooped out many lake rock-basins in the latest great glacial epoch, it did the same during glacial epochs of earlier date.

A. C. RAMSAY

* Geol. Jour., vol. xii. p. 254.

WOOD'S "INSECTS AT HOME"*

THIS bulky volume of 670 pages appears to us to be altogether a mistake. It is far too voluminous and too uninteresting for a beginner, while for the more advanced student it is almost valueless, being a very incomplete compilation from the works of well-known writers. It consists of brief and imperfect descriptions of a selection of, perhaps, one-twentieth of the insects inhabiting Great Britain, with occasional notices of their habits and economy, and extracts from a few entomological works. These descriptions are generally introduced by such words as "Our next example," "We next come to," "We now come to," "Next in order comes," "Next on our list is," &c. &c.; and for the most part are mere amplifications of short technical characters, conveying a minimum of useful information, with a maximum expenditure of words. Let us take two examples at random. At p. 76 we have two-thirds of a page devoted to a beetle:—

"Our first example of the Staphylinidæ is one of the finest, in my opinion the very finest, of that family. It is called scientifically *Creophilus maxillosus*, but has, unfortunately, no popular name, probably because it is confounded in the popular mind with the common black species, which will be presently described. Its name is more appropriate and expressive than is generally the case with insect names. The word *Creophilus* is of Greek origin, and signifies 'flesh-lover,' while the specific title, *maxillosus*, signifies 'large-jawed.' Both names show that those who affixed them to the insect were thoroughly acquainted with its character and form, for the Beetle is a most voracious carrion eater, and has jaws of enormous size in proportion to its body. The colour of this beetle is shining black, but it is mottled with short grey down.

"In some places this Beetle is tolerably plentiful, but in others it is seldom if ever seen. It can generally be captured in the bodies of moles that have been suspended by the professional mole-catchers, and, indeed, these unfortunate moles are absolute treasure-houses for the coleopterist, as we shall see when we come to the next group of Beetles. A figure of this insect is given on woodcut No. viii. Fig. 3. It is the only British insect of its genus which is distinguished by having short and thickened antennæ, smooth head and thorax, and the latter rounded."

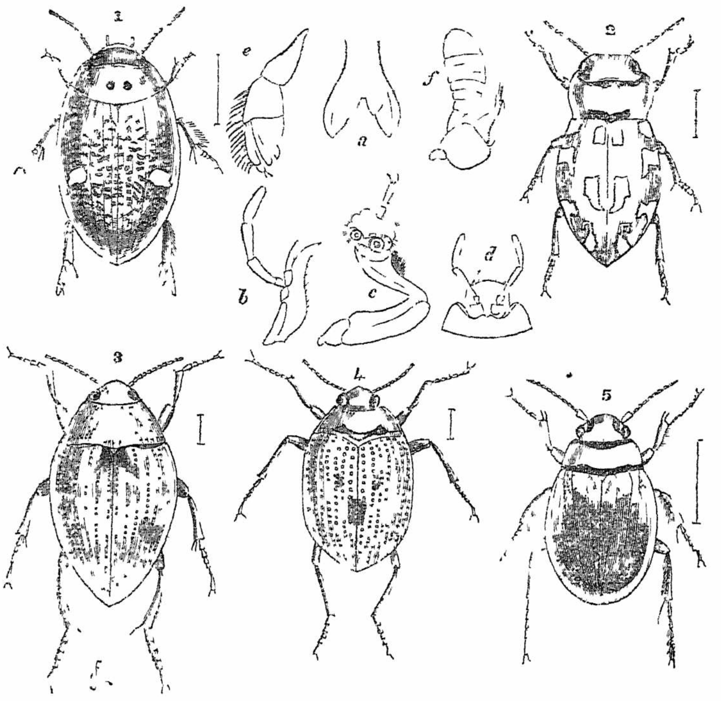
The descriptive portion of this characteristic passage could be easily compressed into two or three lines. In the other twenty we are told that the original describers of the insect were well acquainted with it, that the public are not, and that moles caught by professional mole-catchers are unfortunate!

Turning to page 447, we have a moth described as follows:—

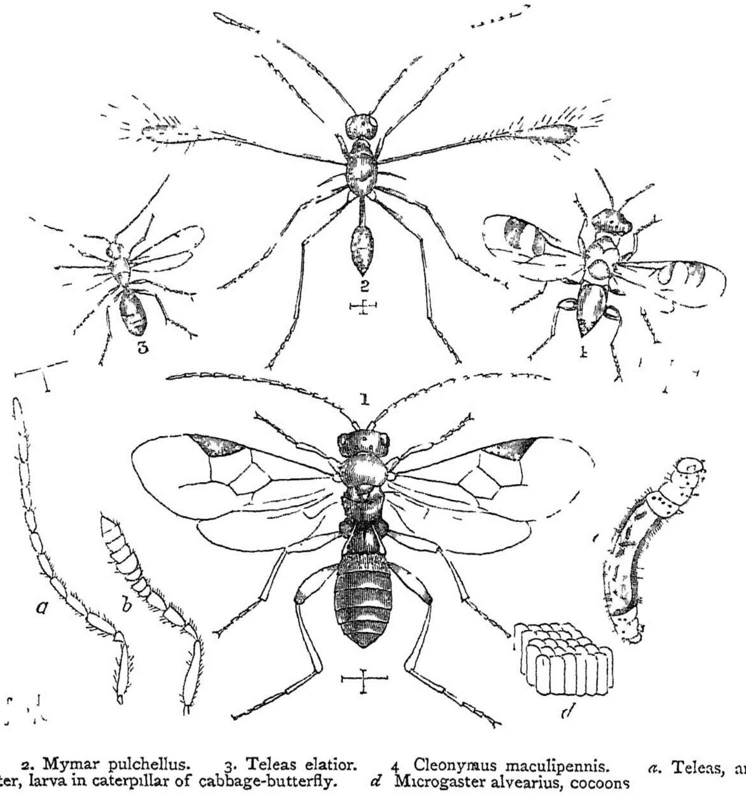
"The first family of the Geometræ is called Urapterydæ, or Wing-tailed Moths, because in them the hinder wings are drawn out into long projections, popularly called 'tails.' In England we have but one insect belonging to this family, the beautiful, though pale-coloured, swallow-tailed moth (*Urapteryx sambucata*). The generic name is spelt in various ways, some writers wishing exactly to represent the Greek letters of which it is composed, and others following the conventional form which is generally in use. If the precisians are to be followed, the word ought to be spelt Ourapteryx.

"There is no difficulty in recognising the moth, the colour and shape being so decided. Both pairs of wings are delicate yellow, and the upper pair are crossed by two narrow brown stripes, which run from the upper to the lower margin. These stripes are very clear and well defined, but besides these are a vast number of very tiny

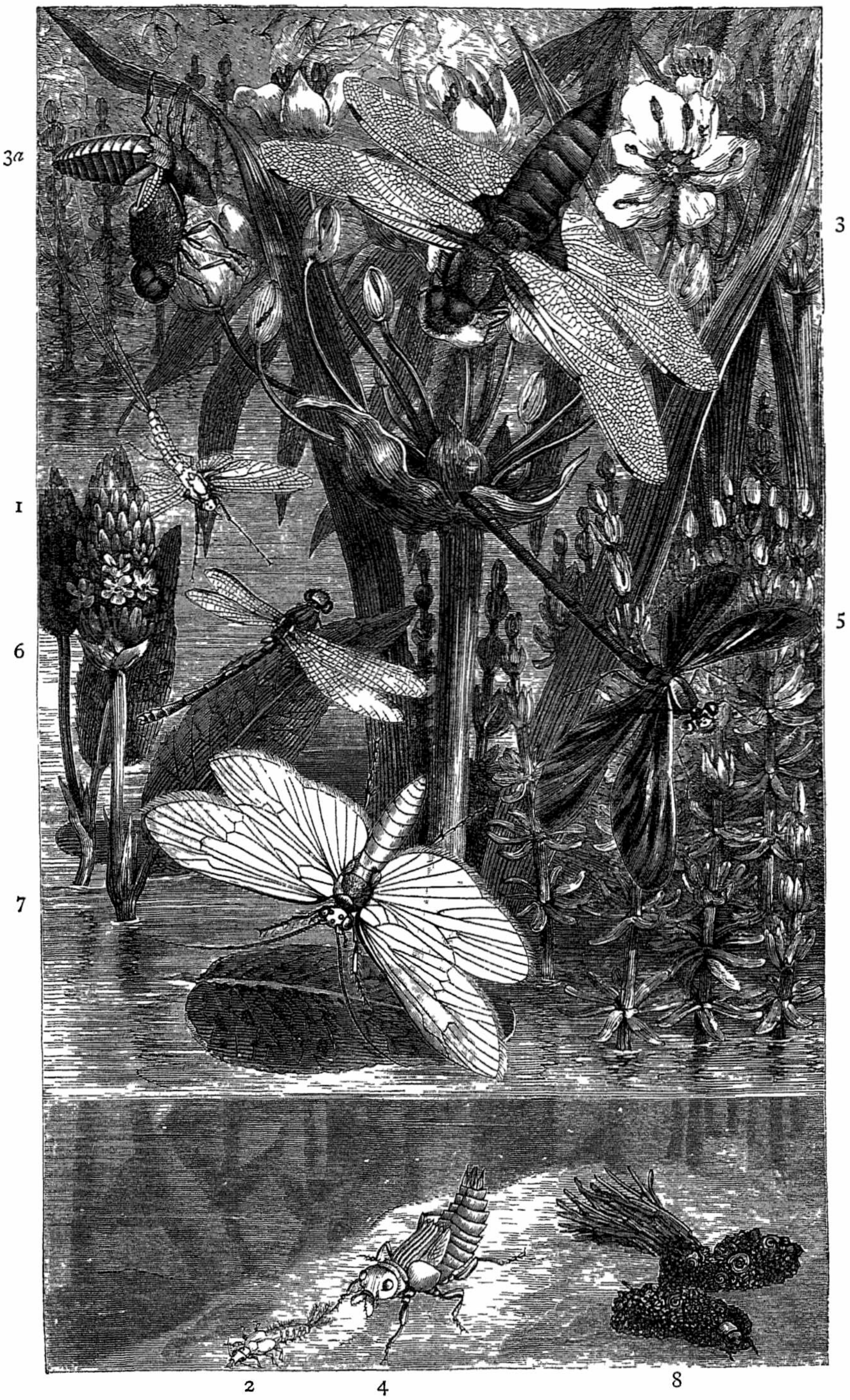
* "Insects at Home: Being a Popular Account of British Insects, their Structures, Habits, and Transformations." By the Rev. J. G. Wood, M.A., F.L.S., &c. With upwards of 700 Figures by E. A. Smith and J. B. Zwecker. Engraved by G. Pearson. (Longmans, Green, and Co. 1872.)



1. Agabus biguttatus. 2. Hydrophilus duodecim-pustulatus. 3. Haliplus variegatus. 4. Cnemidotus caesus. 5. Pelobius Hermanni. *a* Dyticus, process of metasternum. *b* Dyticus, maxillary palpus. *e* Dyticus, anterior leg of male. *d* Dyticus, labial palpi. *c*, Gyrinus, posterior leg. *f* Gyrinus, antenna.



1. Microgaster glomeratus. 2. Mymar pulchellus. 3. Teleas elatior. 4. Cleonymus maculipennis. *a*. Teleas, antenna, female. *b* Do., antenna, male. *c*. Microgaster, larva in caterpillar of cabbage-butterfly. *d* Microgaster alvearius, cocoons.



DRAGON-FLIES, MAY-FLIES, AND CADDIS

1. *Ephemera vulgata* 2 *Ephemera*, larva 3 *Libellula depressa* 3a *Libellula* emerging from pupa case. 4. *Libellula*, larva 5. *Calopteryx virgo* 6 *Agrion minium* 7 *Phyrganea grandis* 8 *Phryganea*, larva cases, or Caddis.
PLANTS —Flowering Rush (*Butomus umbellatus*) In Centre Mare's tail (*Hippuris vulgaris*) On Right Water Bistort (*Polygonum amphibium*). On Left

streaks of a similar colour, which look as if they had been drawn in water-colours with the very finest of brushes, and then damped so as to blur their edges. The hind wings have only one streak, which runs obliquely towards the anal angles, and, when the wings are spread, looks as if it were a continuation of the first stripe on the upper wings. The shape of the moth almost exactly resembles that of the Brimstone Butterfly, described on page 393.

"The larva affords an admirable example of the twig-resembling caterpillars. It is exceedingly variable in colour, but is always some shade of brown. It has seven bud-like humps, and a few pale stripes along the sides. I is a very general feeder, and may be found on a considerable number of trees and plants. It is quite common, and but for its curious form, would certainly be found much more frequently than is the case. The perfect insect appears about July, and can be beaten out of bushes and hedges. Though the wings are large, they are thin and not very powerful, so that there is no difficulty in capturing the insect."

Of course much of the book consists of more interesting matter than this, but hundreds of pages are filled with such verbose and meagre passages as those quoted, which are far more repulsive to the learner than the most condensed and technical description. Those given in Stainton's Manual, for instance, contain more than double the actual information in about one fourth of the space.

The book is illustrated by copious woodcuts in the letterpress and by several whole-page pictures. The former are most admirable, and do great credit to the artist, Mr. E. A. Smith. We select a group of Water Beetles (Cut vi.), and one of the minute and curious parasitic Hymenoptera (Cut xxxii.) as examples of these excellent figures, which would do credit to a far more scientific work. The whole-page illustrations are by another hand, and are in every respect inferior. Some of them contain fair representations of insects in their haunts, but the vegetation is generally badly drawn, and the plants said to be figured are often quite unrecognisable. The best and most artistic picture is Plate VIII., representing a group of Neuroptera with aquatic vegetation. The worst is Plate XIX., representing aquatic Heteroptera. The insects are pretty well drawn, but the plants are dreadful. One of them is said to be the Starwort (*Aster tripolium*). What is meant for this stands prominently out in the view; but the artist has evidently never seen the plant, and, trusting to his imagination to invent something suited to the name, has perched three thick six-rayed star-fish on bending stalks. We venture to assert that no plant having the faintest resemblance to this monstrosity forms part of the British flora, and its introduction into a modern work on natural history is most discreditable. It is painful to have to speak in these terms of the work of an author who has done so much to popularise natural history as Mr. Wood, but we must protest against mere book-making; and in this case there could be no pretence of a want to be supplied, since the excellent series of "Introductions" published by Messrs. Reeve and the more general works of Prof. Duncan, Dr. Packard, and others, are far better guides to the student or to the general reader than such a hasty and imperfect compilation as the present volume.

A. R. W.

NOTES

THE Council of the Royal Society have awarded the medals in their gift for the present year as follows:— The Copley Medal, to Julius Robert Mayer, of Heilbronn; the Royal Medals to Mr. George Busk, F.R.S., and Dr. John Stenhouse, F.R.S.

PROF. ARCHIBALD GEIKIE is desirous of addressing himself through our columns to those of our readers who were friends and correspondents of Sir Roderick Murchison. They would much oblige and assist him if they would let him have the use of such of his letters as they can allow to be employed in the preparation of the biography which, at Sir Roderick's request, he has undertaken to write. If the documents are sent to him at Ramsay Lodge, Edinburgh, they will be returned at the earliest possible date.

FROM the English Government Eclipse Expedition we learn that since leaving Malta, on the evening of Saturday the 4th, the weather has been all that could be wished, and that Mr. Lockyer and the other members of the expedition have not failed to take all possible advantage of the calm weather in testing their instruments and preparing themselves in every possible way for rapid yet correct observations during the few minutes over which the phenomena of the morning of the 12th December will extend. The *Mirzapore* commenced steering through the canal at 2.30 on the 8th, and anchored in Suez Roadstead at twelve on the 10th, all well. It was hoped that she might sail by daylight on the morning of Sunday, the 12th. In that case she might get to Galle by the 25th, and the Expedition would then have seventeen days at their disposal for arranging themselves and their instruments over the line of totality, from the north of Ceylon to the western shore of Southern India. The passage through the Canal has been a pleasant and interesting one.

THE Falconer Memorial Fellowship, at the University of Edinburgh, which is of the annual value of about 100*l*., tenable for two years, has been conferred on Mr. William Stirling, B.Sc. The Baxter Physical Science Scholarship, vacant by the appointment of Mr. William Stirling, to the Falconer Memorial Fellowship, has been conferred for one year on Mr. Alexander Hodgkinson.

MR. P. L. SIMMONDS is now delivering at the London Institution, Finsbury Circus, the Travers Course for 1871-2, on Science and Commerce, illustrated by the raw materials of our manufactures, in two lectures, the first of which will be this evening, and the second on November 30th.

PROF. PARTRIDGE commenced his annual course of lectures on Anatomy to the pupils and Royal Academicians in the new theatre at Burlington House on Monday last week, and will continue the same every Monday evening up to December 11 inclusive, at eight o'clock.

AMONG the disastrous results of the recent fire at Chicago, one not referred to in the public papers was, we regret to learn from *Harper's Weekly*, the entire destruction of the building and collections of the Academy of Sciences of that city. This institution, first started by the energy of the late Mr. Robert Kennicott, and carried to its late condition of prosperity under the charge of Dr. William Stimpson, had already taken a front rank among the learned establishments of the country. Its publications embraced material of the utmost value, while its museum ranked at least as high as the fifth in the United States. Although believed to be fire-proof, the building, like others of the same character in Chicago, presented but little resistance to the flames, and everything within the walls was destroyed. The loss included, besides the collections in natural history of the Academy, a large number of marine invertebrates belonging to the Smithsonian Institution, which had been forwarded to Dr. Stimpson for investigation. The private cabinet of this gentleman, and a large mass of valuable manuscript belonging to him, embracing extended memoirs upon the mollusca, radiata, and crustacea of North America, with numerous illustrations, were entirely destroyed.

AFTER a seven years' tour of exploration in South America, Dr. A. Habel, a former resident of Hastings-on-the-Hudson, has returned to New York, where he is assiduously engaged in preparing the results of his labours for the press. Among the regions traversed by this gentleman may be mentioned the greater part of Central America, the Cordilleras of the Andes in Colombia, Ecuador, and Peru, and finally the Chincha Islands and the Galapagos. During this whole period Dr. Habel was diligently occupied in gathering information in regard to the natural and physical history of the countries mentioned, especially in the departments of ethnology, meteorology, and zoology. He has already made some communications on the subject of his travels to the Academy of Sciences at Paris, and other learned bodies, and we look forward to his detailed report with anticipations of much interest. The Guano deposits of the Chinchas were thoroughly explored by the doctor, who found them to be of a much more complicated structure than has hitherto been supposed.

MR. MESTRE, the secretary of the Academy of Sciences of Havana, has lately offered on its behalf certain prizes for memoirs on subjects of medicine and natural history, indicating a gratifying condition of scientific activity in Cuba. Competition is open to persons of all nations, although the memoirs are to be written in the Spanish language. Among the prizes mentioned by Mr. Mestre is one of three hundred dollars, proposed by the president of the society, Dr. Gutierrez, for the best paper upon a certain beetle, which is very destructive to the sweet-potato. A full account of the animal and its habits is required, and the best method of protecting the plants against its ravages. The Zayas premium of one hundred dollars is offered for a paper upon the hygiene of children—to be written as an aid to mothers. Competition for these prizes is to close on the 1st of March, 1872.

THE Royal Geographical Society has again invited the following public schools to take part in the competition for its prize medals in 1872:—*English Schools.*—St. Peter's College, Radley, Abingdon; King Edward's School, Birmingham; Brighton College; Cathedral Grammar School, Chester; Cheltenham College; Clifton College; Dulwich College; Eton College; Haileybury College; Harrow; Hurstpierpoint; Liverpool College; Liverpool Institute; London,—Charter House; Christ's Hospital; City of London School; King's College School; St. Paul's; University College School; Westminster School; Royal Naval School, New Cross;—Manchester School; Marlborough College; University School, Nottingham; Repton; Rossall; Rugby; King's School, Sherborne; Shoreham; Shrewsbury; Stonyhurst College, Blackburn; Uppingham School; Wellington College; Winchester School. *Scotch Schools.*—Aberdeen Grammar School; Edinburgh Academy; Edinburgh High School; Glasgow High School. *Irish Schools.*—Royal Academical Institute, Belfast; Dungannon Royal School; Ennis College; Portora Royal School, Enniskillen; Foyle College, Londonderry; Rathfarnham, St. Columba's College. Examinations will be held in both Physical and Political Geography, the special subjects for 1872 being as follows:—In Physical Geography; the Physical Geography of South America and the adjacent Islands, Trinidad, Galapagos, Falkland Islands, and Tierra del Fuego. In Political Geography; the Geography of the same districts.

THE Bussey Institution School of Agriculture and Horticulture, in connection with Harvard University, has been established in execution of the Trusts created by the will of Benjamin Bussey, to give thorough instruction in Agriculture, Useful and Ornamental Gardening, and Stock-Raising. In order to give the student a sound basis for a thorough knowledge of these Arts, the school supplies instruction in physical geography, meteorology, and the elements of geology, in chemistry and physics, in the elements of botany, zoology and entomology, in levelling and road-building, and in French and German. Connected with it are the names of such eminent professors as Asa Gray in botany, Whitney in geology, Shaler in zoology, and Trowbridge in physics.

A REPORT on the Physical Laboratory of the Massachusetts Institute of Technology, has been presented to Prof. J. D. Runkle, President of the Institute, by E. C. Pickering, Thayer Professor of Physics. The object designed by the establishment of the laboratory was to provide apparatus and other convenience for the performance of the more common lecture-room experiments, to supply a place where investigations of a high order can be carried on, and to train instructors in physics for the numerous colleges now springing up all over the Continent of America. Particulars are given of a number of experiments of high order successfully carried on in the laboratory during the past year.

ENGLAND is beginning to acknowledge her forgotten scientific worthies. We learn that a public meeting was held last week at Birmingham, for the purpose of taking steps to establish a memorial to Dr. Priestley. It was resolved that the memorial should embrace three objects, viz., the purchase of a site, a scholarship, and a statue, so as to pay honour to Dr. Priestley both as a pioneer of science and as a champion of civil and religious liberty. A committee was appointed to carry the resolution into effect. It was stated that a sum of 3,000*l.* would be required, and several handsome subscriptions have been promised. The proposed site is that of the house at Fairhill, where Dr. Priestley resided for eleven years. The building was burnt down by rioters in 1791, after which he went to America.

THE Hartley Institution at Southampton has issued its Report for the year ending June 30, 1871. Although the managers of the Institution appear to have especially cultivated the training of engineering students for the Cooper's Hill College and elsewhere, the report refers with satisfaction to the increased number of students who have entered for general educational training as compared with former years.

THE Proceedings of the Bristol Naturalists' Society, vol. vi., part 1, for January to May 1871, contains the following papers:—The Natural History of the German People, by Dr. Beddoe; On the Origin of Species in Zymotic Diseases, by D. Davies; Personal Experiences in the Deep-sea Dredging Expedition in H.M.S. *Porcupine*, by W. L. Carpenter; On the Strata comprising the shores of Waterford Haven, with especial Reference to the Occurrence of Llandeilo Fossils in that Locality, by Major T. Austin; On the Development of the Carboniferous System in the neighbourhood of Edinburgh, by E. W. Claypole; and On some Gravels in the Valley of the Thames in Berkshire, by E. W. Claypole. Valuable as these papers may be in themselves, it will be seen that not one of them has any special reference to the natural features of the neighbourhood of Bristol.

HERE is a grand opportunity for our young teachers of science; we give the trustees the benefit of the advertisement gratis:—"Grammar School of King Edward VI., Morpeth.—Wanted for the above school during the year 1872, a competent Master, to instruct the boys in modern languages (French and German indispensable). He will be required to attend at least forty days in each half year, and to teach not less than three hours each day. Salary, 50*l.* per annum. Travelling expenses at the rate of 10*l.* per annum will be allowed if the master does not reside in Morpeth. Also, a Master to teach elementary science (*botany, chemistry, and geology indispensable*). He will be required to attend twenty days in each half year, and devote three hours each day to teaching. *Salary,* 25*l.* *per annum.* Travelling expenses at the rate of 5*l.* per annum will be allowed as above. Also, a Master to teach practical drawing (to include mapping, planning, mechanical and architectural drawing). He will be required to attend twenty days in each half year, and devote three hours each day to teaching. Salary, 25*l.* per annum. Travelling expenses at the rate of 5*l.* per annum will be allowed as above. The trustees will not object to one master holding the two latter appointments. Applications, accompanied with testimonials, &c., to be sent to me on or before Friday, the 1st day of December next.—By order, BENJ. WOODMAN, Clerk to Trustees. Morpeth, 1st November, 1871." Seriously, we had thought the days gone by when it was deemed possible to teach "botany, chemistry, and geology," to say nothing of the other branches of "elementary science," in sixty hours in each half year, and to remunerate the teacher who is competent to instruct in all these subjects, at the rate of 12*s.* 6*d.* per diem and 2*s.* 6*d.* extra for travelling expenses!

THE new edition of Gauss's "Motus Corporum Cœlestium," which has just been published by Perthes, in Gotha, and which is designated as the seventh volume of Gauss's works, and is accompanied by a copy of the original vignette, might easily seem to be a part of the edition of Gauss's works, prepared by the Royal Society of Sciences in Göttingen. We are informed by that Society that the designation of this book as "Gauss's Works, vol. vii.," was chosen without their consent, and that it forms no part of the complete edition of Gauss's works, edited by the Royal Society, and now in the press.

WE are requested by Mr. R. A. Proctor to correct a slight error in the description of Mr. Brothers' negative of his star-chart given last week. The 8-inch negatives, like the 11-inch pictures, are copies of a chart containing upwards of 324,000 stars (not 50,000 only). Prof. Airy, at the last meeting of the Astronomical Society, remarked that the constellations in this chart are not conspicuous. They could not be so, without spoiling the chart; but the lithographic key-map practically removes the objection. The chart is a contribution to physical astronomy—not intended to aid the search for individual stars, though useful in the Observatory, as showing where the richer star-fields are.

[illegible] THOS. J. BOYD has reprinted his paper, "Educational H[illegible] Reform: The Scheme of the Edinburgh Merchant [illegible]y," presented to the Statistical Section at the recent [illegible] of the British Association.

[illegible]ES of "penny lectures for working men" in connection [illegible] Museum of the Folkestone Natural History Society [illegible] commenced last week. The series is intended to illustrate the specimens in the museum—the subject of the first by the hon. secretary, Mr. Ullyett, being "Our Chalk Hills and their Fossils." If the experiment succeeds the lectures will be continued fortnightly during the winter months. Classes in botany and geology, also under the direction of the energetic secretary, were commenced on Wednesday, the 8th inst.

W[illegible] are glad to learn that the interesting series of popular science lectures, to which the charge of admission is only one penny, have been recommenced this winter session in Manchester. The opening lecture was delivered by Prof. Huxley on "Yeast," before a large and attentive audience.

THE *Echo Agricole* complains of the neglect of instruction in science in France. "Why," says this journal, complaining of the importance attached in most schools to a semi-mythological teaching, "when an intelligence is just opening to the light, should it be led through the delusive labyrinths of the marvellous, instead of showing it the truth in all its splendour? Let the young intelligence be accustomed to the observation of natural phenomena, and it will be seen to develop itself normally, because to all the branches of activity which it is called upon to exercise it will bring the spirit of methodical order which it will have been obliged to employ in the study of nature. We therefore demand that the Minister of Public Instruction should introduce into our primary schools the elementary teaching of natural science applied to what children see daily in the country. M. Jules Simon has ordered that a geographical class should be held every fortnight in the colleges and lycées; now we would have the Minister complete this measure by requiring the students not only to mention the principal products of such and such a country, but, as regards France especially, to take account of the natural produce of the land, and to know what sort of soil these different products affect. This would be geography applied to agriculture. . . . Further, we would require that all sciences relating to agriculture taught in the lycées and colleges should be followed by practical application to the soil, such experiments to form the basis of special examinations."

A DISCOVERY has been made by several farmers on the Loddon River in South Australia, that kangaroo rats are good thistle eradicators. "It has been found," says the *Bendigo Independent*, "that these animals dig down under the thistles, and eat the roots of the plants, which thus necessarily die. One farmer has issued orders that no kangaroo rats are to be killed on his land, in consequence of their having been of much service to him in destroying the obnoxious thistles."

AT a meeting of the Philosophical Society of Christchurch, New Zealand, in August last, the President, Dr. Haast, made a few observations on some moa eggs recently received from the Colonial Museum, in comparison with those of living birds. The various models of eggs were displayed on the table. The President said that the first egg to which he would desire to draw their attention was one the pieces of which had been discovered by the Hon. Walter Mantell, and by him reunited after much labour. The original egg, from which a model had been taken by Dr. Hector, was in the British Museum. The second model was that of the largest egg found. It had been discovered in the Kaikoras Peninsula, between the legs of a human skeleton, which had been buried in a sitting position, and, from the fact of it having been so found, he argued that the moa was of great antiquity, as there was no mention in the very earliest Maori traditions of such a mode of burial being adopted. The egg was afterwards exhibited at the Otago Exhibition, and the model had been made by Dr. Hector from measurements taken by him. The third and last model was that of a small egg now in the Colonial Museum, which had been found in Otago, and which had in it the bones of a moa chick.

ON the 17th of September the installation of the Academy of Natural Science took place at Bogota, in Columbia or New Granada, with much ceremony. As yet not much can be expected from it, but it is another sign of the progress taking place in the country. The orator gratefully commemorated what had been done for Columbia by Humboldt, Boussingault, and Acosta in geology, and by Mutis and Caldes in botany.

THE first meeting of the Eastbourne Natural History Society for the present season was held on Friday, October 20. A paper "On the species of Hepaticæ found in the Eastbourne District" was read by Mr. F. C. S. Roper, F.L.S., containing notes on the structure and development of the group, with a list of the species (fifteen in number) occurring in the neighbourhood. It was followed by a paper "On the Bones of Red Deer, &c., found in Eastbourne," by Mr. S. Eveshed. We are glad to observe that active local work is a prominent feature of this young society.

THE Whitechapel Foundation School Literary and Scientific Society held its first annual public meeting last week in the School-room, Leman Street. The Chairman, Mr. Edmund Hay Currie, member of the London School Board, having briefly referred to the importance of the work, and to the dissemination of scientific knowledge by the society's agency, called upon the hon. secretary to read the report; from this we gleaned that the association had made good progress during its first year of existence, and that the interest in the undertaking was rapidly increasing. Twenty-six lectures had been delivered, amongs the principal subjects were "Oxygen and Hydrogen," by Mr. Joseph Loane, M.R.C.S., L.S.A., &c., &c., "Blood and its Constituents," and "Respiration, with its Mechanism," by Mr. H. A. A. Nicholls, of St. Bartholomew Hospital; "The Solar System," "Heat," "Coal and its products," "Electricity," "Chemical affinity," "Water," &c., &c. The evening's proceedings were brought to a termination with a lecture on "Light," illustrated by experiments, by the President, Mr. Charles Judd. We are glad to find that this society has received considerable recognition from gentlemen interested in science and in education generally.

COLDING ON THE LAWS OF CURRENTS IN ORDINARY CONDUITS AND IN THE SEA

[I SEND to NATURE for translation the abstract (in French) appended, according to a most excellent custom, to Colding's great paper in the Copenhagen Transactions. The subject is one of especial interest at the present time, though, of course, everything written by such a man is deserving of careful attention. Those in particular who met the genial Dane at the British Association will be glad to have in a compact form his views on a question which has given rise to much discussion, and which is of very great practical importance.—P. G. TAIT]

I PRESENTED in 1863 to the Scientific Society, and some months later to the Congress of Scandinavian Naturalists at Stockholm, a short exposition of my researches on the motion of fluid bodies, on which I had been occupied for several years, and the results of which appeared to me worthy of being submitted to the Society.

The characteristic of this work is that it does not suppose, like previous works of the same kind, that all the parts of a current are endowed with the same rapidity; for it owes, in fact, its existence to my conviction that this mode of looking at the subject can only lead in exceptional cases to exact results. My researches are based on the different motions assumed by the liquid threads or elements of the currents, and are supported by the well-known fact that any body, and consequently any portion of a fluid, can only move with a constant rapidity when the accelerating force is equal to the resistance.

In the case of a fluid flowing by virtue of its own weight over a plane surface which opposes a resistance to the motion of the water, it was easy to determine how this motion varies with the depth, when the rapidity of the current is constant in all its parts; and, by pursuing this train of thought, I was led to the law of the variation of the rapidity with the depth, when the current moves in a cylindrical conduit with circular section, completely filled with the liquid. These researches are of greater interest from the circumstance that the laws at which I have in this manner arrived from theoretical considerations, are confirmed by the experiments which have recently been carried out in France by Capt. Boileau and Inspector-General Darcy. These laws of the motion of water may be expressed by the formula

$$(V - v)^2 = K_0^2 \frac{h}{l} x^3;$$

where V is the rapidity of the first elements of the current, the motion of which is the most rapid, v the rapidity at the depth x, $\frac{h}{l}$ the fall per foot of the water, and K_0^2 a magnitude which depends entirely on the nature and dimensions of the conduit, on the depth of the current, &c. The theory shows besides that the laws of the motion of water on a level surface are included in the general law of the motion of water on a cylindrical surface, when the radius of the cylinder is supposed infinite.

Darcy, who has experimentally established the formula given above for cylindrical conduits, endeavoured, at the same time, to determine K_0^2 by means of certain experiments performed with four different kinds of pipes, and found that K_0^2 was inversely proportional to the square of the radius of the conduit. It resulted, according to the theory, that, for level conduits, K_0^2 should be in the same manner in an inverse ratio to the square of the depth of the current. But two series of experiments performed by Boileau with level conduits led, on the contrary, to the supposition that K_0^2 was inversely proportional simply to the depth of the current. There was thus a want of agreement between the results of the two experiments, and the point was to discover which of these two hypotheses was correct. Several circumstances leading me to believe that Darcy's theory could not be exact, I took as my starting point the experiments of Boileau, and considered K_0^2 as inversely proportional to the depth of the current, which I did with the less scruple since this hypothesis agreed almost as well with Darcy's experiments as with his own. I pursued, therefore, my researches on this basis, and, after many difficulties, arrived at results which, on the whole, were so entirely in accordance with experiments that I could not suppose the possibility of Boileau's hypothesis being inexact. It was only afterwards, when I approached the study of marine currents, that new difficulties constantly arose, which I endeavoured at first to overcome, but which became day by day more insurmountable, until at last there was nothing left but to doubt the correctness of my calculations, since they led to results which were in obvious contradiction to facts.

The theory then was shown to be inexact; but since in so large a number of cases it was evidently in agreement with experiment, I attempted by a variety of means to discover the error which I must have committed; still all my attempts were unattended with result, and I was on the point of abandoning the resolution of the problem to which I had already devoted so much time, when the idea struck me of examining what would happen if I rejected Boileau's determination of K_0^2, and adopted Darcy's hypothesis, although it still appeared to me impossible; when I found, with as much delight as surprise, that it removed not only the great difficulties which I had up to that time encountered, but also all the contradictions which had occurred to me as an inevitable consequence of that hypothesis, and from that moment the results of the calculations showed themselves to be entirely in the most perfect accordance with what exists in nature.

The circumstance that the experiments of Darcy are almost as satisfactory whether $\frac{1}{K_0^2}$ is supposed to be proportional to the first or to the second power of the depth of the currents, made me think that the reality would be still more nearly approached by expressing this magnitude by a binomial of the first and second degree, and this was completely confirmed by facts. In determining the constants of the binomial according to the results of Darcy's experiments, I found the law of the motion of the water in cylindrical pipes with a radius R, with a coefficient of resistance m, and a rapidity v_0 at the surface of the conduit, may be represented by the formula

$$V - v = 6{\cdot}8\sqrt{m} \times v_0 \times \left(\frac{x}{R}\right)^{\frac{3}{2}} \sqrt{\frac{117{\cdot}7R}{62{\cdot}5 + 117{\cdot}7R}}$$

V being the rapidity next the axis, to which corresponds $x = 0$. This formula may be applied equally to the movement of water in level conduits, if by R is designated the depth of the current; only the coefficient then becomes $\frac{6{\cdot}8}{\sqrt{2}} = 4{\cdot}8$, instead of $6{\cdot}8$.

This formula shows, among other things, that the ratio $\frac{v}{V}$ which corresponds to any point in a given conduit entirely filled by the current, is entirely independent of the rapidity of the current, a fact which Darcy's experiments confirm in a remarkable manner. This relation furnishes us besides with the means of determining the value of the coefficient of resistance m for different kinds of pipes which were employed by Darcy, and it is thus found that for

Old pipes . .	m =	from 0·0120 to 0·0080
New pipes . .	m =	from 0·0050 to 0·0033
New varnished pipes.	m =	from 0.0033 to 0·0025

values which are altogether independent of the diameter of the conduit. For level wooden conduits, it is found, according to the experiments of Boileau, that m = 0·0160 to 0·0090, while the resistance of the air, according to the same author, corresponds to m = 0·0003 to 0·0002

Inspector-General Darcy has unfortunately died, but the researches on currents which he commenced were continued by the French engineer Bazin, who published in 1865 a great work on the results of a considerable number of experiments carried on with conduits of very different kinds.

However interesting otherwise these researches may be, they do not display either the powers of observation or the grandeur of conception which distinguish the works of Darcy. Among those experiments which are of the greatest interest, there are some begun by Darcy and finished by Bazin, such as researches into the motion of water in rectangular conduits, where the rapidity is determined in 45 points symmetrically distributed. The result for these, as for circular conduits, is that the ratio $\frac{v}{V}$ is independent of the absolute rapidity of the current, and if the results of experiment on the motion of water in level conduits are compared with those given by the theoretic formulæ, it will be found that these last agree so completely with experiment, that the difference between the calculated and observed rapidities, in each of the 45 points mentioned above, falls within the limit of errors of observation. This agreement is especially remarkable in the case of the conduit which Darcy employed in 1857 for the carrying out of his researches. In 1859 Bazin undertook similar experiments with a small rectangular conduit; but he did not make so great a number of experiments, and his errors of observation are larger than those of Darcy. In determining the coefficients of resistance of these conduits, it was found that for those of Darcy m = ·0104, while for those of Bazin it rose to 0·0180.

Bazin performed a considerable number of experiments on the motion of water in open conduits, and thought himself compelled to admit that the laws of this motion are essentially different from those which relate to perfectly full conduits; but he is certainly in error.

The results of a considerable number of ancient measures of currents are in existence, which Bruning undertook, towards the close of last century, in different rivers—namely, the Rhine, Waal, &c. They were performed with much care; but, as might be foreseen, are nevertheless very defective. They deserve, however, to attract attention, partly because the rapidity was determined, for every section of the current, at distances of six inches from the surface to the bottom, in a series of perpendiculars, the imperfections which the measurement of the rapidity presents losing thus much of their importance; partly and especially because the currents examined by Bruning were of a depth which reached 23 feet. In applying this theory to these currents, and especially in determining the constants of the formulæ with the aid of Bruning's measurements, it was found that the observed and calculated rapidities are for all depths as accordant as could be desired; and this agreement furnishes a new proof of the exactness of the theory. The coefficient of resistance m, calculated according to Bruning's measurements, varies between 0·0250 and 0·0080, with a mean of 0·0160; and as the resistance at the depth of these currents must doubtless approach that which a marine current experiences in flowing over a mass of water placed beneath it, and which does not participate in the motion, I have a right to believe that the extreme value $m = 0·0250$ corresponds nearly to the resistance which currents meet with when flowing freely in the sea.

After having in this manner assured myself that the preceding theory agrees with experiment wherever it has been tried, I endeavoured to determine the laws of the motion of water in currents of variable rapidity. In considering the simplest case of this kind, that, namely, in which the conduit is a level surface (I had already treated this case by the old theory), it was found that the laws of currents, according to the new theory, are entirely in agreement with the facts observed in nature; and consequently this theory may be regarded as giving the explanation of all permanent currents.

Having thus shown that this theory of the movement of fluid bodies accounts satisfactorily for all the phenomena, I shall now, from this as a stand-point, give a review of my recent researches upon ocean currents. The currents which more particularly demand our attention here are those of the North Atlantic, especially the Gulf Stream and the Polar Currents.

The Gulf Stream issues, as we know, from the Gulf of Mexico, but it is possible to follow its course across the Carribean Sea, where passing between the Antilles, it arrives from the Atlantic, and afterwards flows to the north-west at the rate of ¼ mile an hour until it enters the Gulf of Mexico. From this Gulf the Gulf Stream takes an easterly course towards the Bahamas along the north coast of Cuba; but, after rounding Florida, it bends northwards, and passes between the latter and the Bahamas, in the channel which separates Florida Cape from the Islets of Bemini; here the current has a speed of 1 mile per hour, a breadth of 8 miles, and a depth of 250 fathoms. From the channel of Bemini the Gulf Stream proceeds directly northwards at a rate which decreases gradually from 6⅓ feet per second at Bemini to 4 feet at St Augustine; the distance between these two points being about 70 miles, during which the breadth of the current increases from 8 miles to 11¾. From St. Augustine to the Bay of New York the Gulf Stream takes a north-easterly course, parallel with the land, and conterminous with a cold current which flows from the north to the south between the stream and the American coast. In this part of its course it continues to increase in breadth from 11¾ miles at St. Augustine to 31¾ at New York; meanwhile its speed decreases from 4 feet to 2⅔ per second. The depth of the sea along the course of the current is many hundred fathoms, and the distance between St. Augustine and New York is 180 miles. On quitting the Bay of New York, the Gulf Stream takes an E.N.E. direction to the south of Newfoundland, skirting the cold current, which goes down to southwest as far as New York, following the east coast of Newfoundland. By the time the Gulf Stream, after a course of 200 miles, reaches the south of Newfoundland, it has attained a breadth of about 80 miles, while its speed is only 2 feet per second; but the current continues to run in the same direction towards Europe for other 300 miles, with a speed which is from 2 feet to 0·6 feet, and a breadth increasing from 80 up to 200 miles. The Gulf Stream, when it has attained a distance of 750 miles from Bemini, separates into two branches, the one proceeding southwards towards the coast of Africa, at a speed of 0·6 feet per second, the other taking a northerly course towards Iceland, along the shores of the British Islands, and running about 200 miles, at a rate which decreases from 0·6 to 0·3 feet per second, the breadth of the current meanwhile increasing from 100 to 105 miles. When the stream reaches the neighbourhood of Iceland, it sends off a branch which skirts the south coast of that island, afterwards taking a direction north-west towards the Polar current of the east coast of Greenland, which it seems partly to follow in its march southwards. As to the main stream, it inclines to the east after passing the extreme north of Scotland, and then runs to the north-east, along the west coast of Norway, until it ends its wanderings in the Icy Sea.

As to the Polar Current we feel authorised to mention the following statements :—From the region of the Icy Sea, the most northerly of which we have any knowledge, from the neighbourhood of Spitzbergen about the 80th degree of N. latitude, there descends to the south-west a great polar current loaded with floating ice. It reaches the coast of Greenland at about 70° N. latitude, and follows it as far as Cape Farewell; its breadth being nearly 40 miles and its speed ⅘ of a foot per second. After passing Cape Farewell, it curves round to northward and follows the west coast of Greenland for some distance into Davis Strait. After having run for a few degrees in this direction it bends to the south-west, towards the coast of Labrador, along the whole length of which it runs, then proceeding to the south-east, enlarged by the polar current which comes from Baffin's Bay. On quitting Labrador, where its speed is ⅘ of a foot per second, and its breadth 50 miles, the polar current on rounding the east coast of Newfoundland makes for the Gulf Stream, and, after doubling Cape Race, sends a branch to the southwest between the Gulf Stream and the American coast, which branch can be traced as far as Florida. As to the part of the polar current which does not take this route, it is generally admitted that it flows underneath the Gulf Stream on the east of Newfoundland, and that it runs uninterruptedly to the south-east, towards the African coast, where the waters of the ocean are of a temperature comparatively low.

In order to explain the causes of these immense ocean currents by the aid of the laws of the movement of water in ordinary conduits, it is necessary first of all to know the forces which produce and maintain the movement of these currents. Captain Maury, who has made a special study of this question, has given it as his opinion that these ocean currents are due to the differences caused by the changes of temperature and of saltness in the specific gravity of the water of the sea. In order to make this theory more easy of comprehension, Maury imagines a globe like the earth covered over the whole of its surface with a sea 200 fathoms in depth, the water throughout being of the same density; at the same time he supposes the surrounding circumstances to be the same at all points, and that there being neither evaporation nor precipitation, there can of course be neither winds nor currents upon the imaginary globe. He next supposed the water contained between the tropics suddenly transformed into oil to a depth of 100 fathoms. From this moment the equilibrium is destroyed, and there results a general system of currents and counter-currents; for the oil, being lighter than the water, will rush along the surface towards the poles, while the water of these regions makes for the equator in the shape of a submarine current. As the oil reaches the polar sea, it is supposed to be transformed into water, which returns to the equator, where it is changed anew into oil that again rises to the surface and again makes its way to the poles, and so on. If then this globe turns, like the earth, on its axis once in the twenty-four hours from west to east, each particle of oil, according to Maury, will proceed towards the pole in a spiral course with a speed towards the east always increasing; on reaching the pole it will turn at a rate equal to that at which the earth revolves at the equator, viz., 225 miles an hour. But, says Maury, when the oil has been changed into water, it will return towards the equator describing a curve in a westerly direction. If the sea in question should be bounded by land, as is the case on the surface of the earth, the uniformity of these currents will be broken up by different local circumstances; and the speed of the currents will vary at various places, but there will always be a system of equatorial and polar currents. Is it not admissible then to suppose, asks Maury, that the cold waters coming from the north and the warm waters issuing from the Gulf of Mexico and made lighter by the heat of the tropics, will act relatively to each other in the same way as the water and the oil in the preceding example?

The Gulf Stream was at one time regarded as a branch of the Mississippi; but this notion must be abandoned since it has been proved that the volume of the Gulf Stream is many thousand times greater than that of the river, and that its water is salt, while the water of the Mississippi is fresh. Next, Benjamin Franklin's idea was generally adopted, viz., that the trade-winds drive the waters before them into the Carribean Sea, whence they issue more slowly in forming the Gulf Stream. Maury, however, refuses to accept this explanation; he admits that the trade-winds may increase the speed of the stream in the strait of Florida, but he maintains that it is impossible for these winds to give such an impetus to the Gulf Stream as would make it traverse the whole of the Atlantic as a markedly distinct current. He caps his objections to the theory of Franklin by remarking, that as surely as a river flows along its bed only under the influence of gravity, so the course of the Gulf Stream in the midst of the ocean necessitates the existence of a never-ceasing moving force; in short, he says, if gravity did not exist, the waters of the Mississippi would never leave their source, and, were it not for a difference of specific weight, those of the Gulf Stream would remain for ever in the tropical regions of the Atlantic. But as Maury disputes the correctness of Franklin's statement, viz., that the surface of the sea is above the normal level in the Gulf of Mexico, and that the water tends by virtue of its weight to rush towards the north, and as observation has proved that along the western edge of the Gulf Stream there flows a current of those cold waters which descend southwards as far as Florida Strait, he can no longer maintain his first opinion as to the cause of the Gulf Stream. He is forced to resign the hypothesis that the water of the Gulf Stream, on account of its greater degree of saltness, has a specific gravity greater than the water of the polar seas, to which it flows in virtue of its great density, causing a current in a direction contrary to the lighter waters of these colder regions. But from the moment that Maury supposes that the ocean currents have their origin at the time when the water of the tropics is lighter, and that of the Gulf Stream heavier than the water of the Polar seas, his point of view becomes uncertain and difficult to sustain; and he fails all the more signally in presenting the question of the currents in its true light, from the fact that at that time there existed no exact method of obtaining the specific gravity of the water of the ocean, the degree of saltness of the different seas being then unknown.

(*To be continued.*)

*PHYSIOLOGY FOR WOMEN**

BY Physiology we should understand a knowledge of the functions of the human body, and of the laws which regulate and maintain its various actions. The physiology of plants and of the lower tribes of animals (Botany and Zoology) are described by two other Professors in the University, and there will be little enough time for me to condense and give an account of what is now known of the subject, even as I have limited it. Whatever useful information, however, can throw light on human physiology, derived from every collateral science, will be made use of to assist inquiry. After some preliminary lectures on the histology, chemistry, the physical and vital properties of the tissues, I shall more especially dwell on the two great functions of nutrition and innervation. The former involves an acquaintance with what constitutes a proper food for man—how it is prepared by mastication, insalivation, digestion, chymification, sanguification, and respiration, to form the blood; how out of this blood the tissues are formed; and how, after these have fulfilled their proper uses, they are separated from the body in the act of excretion. The latter comprehends a description of the functions of mind, including the mental acts, sensibility, sensation, volition, and the varied kinds of motion; of the functions of the nerves; of the special senses, such as smell, taste, touch, sight, hearing, and the muscular sense of voice and speech; and lastly, of sleep, dreams, somnambulism, catalepsy, trance, witchcraft, animal magnetism, &c. &c. Of the subjects included under these heads it is impossible to overrate the importance in reference to their relation to the health and happiness of man, his physical and moral welfare, his social relations, his national resources, and the prosperity of his race. I have long formed the opinion that physiology, besides being essential to the medical student, should be introduced as an elementary subject of education in all our schools—should be taught to all classes of society. It is an ascertained fact that 100,000 individuals perish annually in this country from causes which are easily preventible, and that a large amount of misery is caused by an ignorance of the laws of health. The clergy should especially study it—first, with a view of diminishing the difference in thought existing between literary and scientific men; and, secondly, because their influence on the people from the pulpit, and as parish ministers, is so important. All other professions and trades, however, might beneficially study physiology, especially newspaper editors and reporters, who diffuse a knowledge of useful things among the public; and architects, who have not yet learnt to build dwelling-houses and public halls properly ventilated. But women, in all classes and degrees of society, have more to do with the preservation and duration of human life even than men. It has been argued that, inasmuch as even the brutes know instinctively how to take care of their young, so must women be able to do the same. But the human infant is the most helpless of creatures, and nothing is more lamentable than to witness the anxieties and agony of the young mother as to how she should manage her first-born. In no system of education are women taught the structure and requirements of the offspring which will be committed to their charge; and certainly no error can be greater than to suppose that the senses and instincts are sufficient for teaching man as to his physical, vital, and intellectual wants. The enormous loss of life among infants has struck all who have paid attention to the subject, and there can be no question that this is mainly owing to neglect, want of proper food or clothing, of cleanliness, of fresh air, and other preventible causes. Dr. Lankester tells us, when ably writing on this topic, that, as coroner for Central Middlesex, he holds one hundred inquests annually on children found suffocated in bed by the side of their mothers, and he calculates that in this way 3,000 infants are destroyed in Great Britain annually alone, attributable in nine cases out of ten to the gross ignorance of those mothers of the laws which govern the life of the child.* But women are the wives and regulators of the domestic households. They also constitute the great mass of our domestic servants. On them depends the proper ventilation of the rooms, and especially the sleeping rooms, in which all mankind on an average spend one-third of their lives. Children are too often shut up all day in crowded nurseries, and when ill, are subjected to numerous absurd remedies before medical assistance is sent for. Their clothing is often useless or neglected, the dictation of fashion rather than of comfort and warmth being too frequently attended to. The cleanliness of the house also depends on women, and the removal of organic matter from furniture and linen, the decomposition of which is so productive of disease. Further, the proper choice and preparation of food is entrusted to them,—all these are physiological subjects, the ignorance of which is constantly leading to the greatest unhappiness, ill health, and death. Among the working classes it is too frequently the improvidence and ignorance of the women which lead to the intemperance and brutality of the men, from which originate half the vice and crime known to our police offices and courts of justice. Additional arguments for the study of physiology by women may be derived from the consideration of—(1) the effects of fashionable clothing—the tight lacing, naked shoulders, thin shoes, high-heeled boots—often subversive of health; (2) the great objects of marriage—the production of healthy offspring—and all the foresight, care, and provision required, but too often neglected through ignorance, to the danger both of mother and child; (3) the proper employment of women, which should be regulated with regard to their conformation and constitutions; and (4) nursing the sick, which is one of the most holy occupations of women, and which would be much more intelligently done if

* Abstract of the Opening Lecture of the Ladies' Course of Physiology, delivered in the University of Edinburgh, Nov. 2, by Prof. Bennett.

* See his excellent pamphlet, "What shall we Teach; or Physiology in Schools." London: Groombridge & Sons, 1870.

they possessed physiological knowledge. Doubtless those who regard this study as too difficult and technical for young men, will decry it also for women ; yet it so happens that for them nothing is so truly interesting as this science. The examination-papers of school-girls of the Ewart Institution, Newton-Stewart, contain an amount of information in physiology perfectly astonishing. Seldom have medical students given better answers. And yet it has been argued that physiology was far too difficult and technical a subject to be studied even by the students in Arts of our University. Hence women in all ranks of society should have physiology taught to them. It should be an essential subject in their primary, secondary, and higher schools. So strong are my convictions on this subject, that I esteem it a special duty to lecture on physiology to women, and whenever I have done so, have found them most attentive and interested in the subject, possessing indeed a peculiar aptitude for the study, and an instinctive feeling, whether as servants or mistresses, wives or mothers, that that science contains for them, more than any other, the elements of real and useful knowledge.

SOCIETIES AND ACADEMIES

London

Geological Society, November 8.—Joseph Prestwich, F.R.S., president, in the chair. Mr. Henry Hicks was elected a Fellow, Dr. Franz Ritter von Hauer, of Vienna, a Foreign Member; and M. Henri Coquand, of Marseilles, a Foreign Correspondent of the Society. The following communications were read:—1. A letter from the Embassy at Copenhagen, transmitted by Earl Granville, mentioning that a Swedish scientific expedition, just returned from the coast of Greenland, had brought home a number of masses of meteoric iron found there upon the surface of the ground. These masses varied greatly in size ; the largest was said to weigh 25 tons. Mr. David Forbes, having recently returned from Stockholm, where he had the opportunity of examining these remarkable masses of native iron, took the opportunity of stating that they had been first discovered last year by the Swedish Arctic expedition, which brought back several blocks of considerable size, which had been found on the coast of Greenland. The expedition of this year, however, has just succeeded in bringing back more than twenty additional specimens, amongst which were two of enormous size. The largest, weighing more than 49,000 Swedish pounds, or about 21 tons English, with a maximum sectional area of about 42 square feet, is now placed in the hall of the Royal Academy of Stockholm ; whilst, as a compliment to Denmark, on whose territory they were found, the second largest, weighing 20,000 lbs., or about 9 tons, has been presented to the Museum of Copenhagen. Several of these specimens have been submitted to chemical analysis, which proved them to contain nearly 5 per cent. of nickel, with from 1 to 2 per cent. of carbon, and to be quite identical, in chemical composition, with many aërolites of known meteoric origin. When polished and etched by acids, the surface of these masses of metallic iron shows the peculiar figures or markings usually considered characteristic of native iron of meteoric origin. The masses themselves were discovered lying loose on the shore, but immediately resting upon basaltic rocks (probably of Miocene age), in which they appeared to have been originally imbedded ; and not only have fragments of similar iron been met with in the basalt, but the basalt itself, upon being examined, is found to contain minute particles of metallic iron, identical in chemical composition with that of the large masses themselves, whilst some of the masses of native iron are observed to enclose fragments of the basalt. As the chemical composition and mineralogical character of these masses of native iron are quite different from those of any iron of terrestrial origin, and altogether identical with those of undoubted meteoric iron, Prof. Nordensckjold regards them as aërolites, and accounts for their occurrence in the basalt by supposing that they proceeded from a shower of meteorites which had fallen down and buried themselves in the molten basalt during an eruption in the Miocene period. Notwithstanding that these masses of metallic iron were found lying on the shore between the ebb and flow of tide, it has been found, upon their removal to Stockholm, that they perish with extraordinary rapidity, breaking up rapidly and falling to a fine powder. Attempts to preserve them by covering them with a coat of varnish have as yet proved unsuccessful ; and it is actually proposed to preserve them from destruction by keeping them in a tank of alcohol. Mr. Maskelyne stated that the British Museum already possessed a specimen of this native iron, and accounted for its rapid destruction on exposure by the absorption of chlorine from terrestrial sources, which brought about the formation of ferrous chloride. This was particularly marked in the case of the great Melbourne meteorite in the British Museum ; he had succeeded in protecting this, as well as the Greenland specimen, by coating them externally, after previously heating them gently, with a varnish made of shellac dissolved in nearly absolute alcohol. He considered it probable that a meteoric mass falling with immense velocity might so shatter itself as to cause some of its fragments to enclose fragments of basalt, and even to impregnate the neighbouring mass of basalt with minute particles of the metallic iron ; but he considered the question of meteoric origin could only be decided by examining the same mass of basalt at some greater distance from the stones themselves, so as to prove whether the presence of such metallic iron was actually characteristic of the entire mass of the rock. Prof. Ramsay referred to the general nature of meteorites and to their mineral relationship to the planetary bodies, and remarked that, supposing the earth to have in part an elementary metallic core, eruptive igneous matter might occasionally bring native iron to the surface. Mr. Daintree mentioned that he had been present at the exhumation of the Melbourne meteorite, and that at that time there was little or no trace of any exudation of ferrous chloride, the external crust on the meteorite being not above $\frac{1}{32}$inch in thickness. 2. "On the Geology of the Diamond-fields of South Africa." By Dr. J. Shaw, of Colesberg. Communicated by Dr. Hooker, F.R.S. The author described the general structure of the region in which diamonds have been found. He considered that the diamonds originally belonged to some metamorphic rock, probably a talcose slate, which occupied the heights during a late period of the "trappean upheaval," to which he ascribed the origin of the chief physical features of the country. This upheaval was followed by a period of lakes, the traces of which still exist in the so-called "pans" of the region ; the Vaal river probably connected a chain of these lakes ; and it is in the valley of the Vaal and the soil of the dried up "pans" that the diamonds are found. The author referred also to the frequent disturbance and removal of the diamentiferous gravels by the floods which prevail in these districts after thunder-storms. 3. "On the Diamond-gravels of the Vaal River, South Africa." By Mr. G. W. Stow, of Queenstown, Cape Colony. Communicated by Prof. T. Rupert Jones. The author described the general geographical features of the country in which diamonds have been found, from Mamusa on the south-west to the headwaters of the Vaal and Orange Rivers. He then indicated the mode of occurrence of the diamonds in the gravels, gravelly clays, and boulder-drifts of the Vaal Valley, near Pniel, including Hebron, Diamondia, Cawood's Hope, Gong Gong, Klip Drift, Du Toit's Pan, and other diggings. By means of sections he showed the successive deepenings of the Vaal Valley and the gradual accumulation of gravel-banks and terraces, and illustrated the enormous catchment area of the river-system, with indications of the geological structure of the mountains at the headwaters. The specimens sent by Mr. Stow, as interpreted by Prof. T. R. Jones, showed that both igneous and metamorphic rocks had supplied the material of these gravels. The author concluded that a large proportion of these materials have travelled long distances, probably from the Draakensberg range ; but whether the original matrix of the diamonds is to be found in the distant mountains or at intermediate spots in the valleys, the worn and crushed condition of some of the diamonds indicates long travel, probably with ice-action. Polished rock-surfaces and striated boulders, seen by Mr. Gilfillan, were quoted in corroboration of this view. Mr. Woodward mentioned that Mr. Griesbach and M. Hübner had been over the country described in these papers, and had communicated a map of it to Petermann's Journal. Mr. Griesbach stated that the rock described as metamorphic in the paper was by M. Hübner regarded as melaphyre, and that in some parts of the Vaal Valley the beds of the Karoo formation might be seen *in situ*. He disputed the possibility of any of the gravels being of glacial origin. He was convinced that there were no metamorphic rocks on the western side of the Draakensberg ; those regarded as such probably belonged to the Karoo formation. Prof. Tennant commented on the large size of the diamonds from the Cape, of which he had within the last few months seen at least 10,000, many of them from 30 to 90 carats each. Some broken specimens must, when perfect, have been as large as the Koh-i-Noor. Mr. Tobin corroborated the information given by Mr. Stow, and stated that the source of the Vaal was in sandstone, and that it was not until it had traversed some distance that agates, peridot, and spinel were met with. The large diamonds, in his view, occurred principally in old high-level gravels, at a considerable elevation above the river, which had much deepened its valley since the time of their deposit. At Du Toit's Pan, however, none of the diamonds, nor indeed any of the other stones, showed any signs of wear; and he considered that at that spot was one of the centres at which diamonds had been found in their original matrix. Mr. Daintree stated that in Australia there were agate-bearing beds of amygdaloid greenstone similar to those in South Africa, and that he had called attention to their existence in the neighbourhood of the Burnett River, where since then a diamond of the value of 80*l.* had been discovered. Mr. Maskelyne commented on the dissimilarity of the minerals found in the diamond-bearing beds of Brazil from those of Du Toit's Pan or of South Africa generally. He thought that possibly the minerals described as peridot and spinel might be bronzite and garnet, which, however, came from igneous rocks; and the remarkable fact was that with them occurred unrolled natrolite and diamonds in an equally unrolled condition, which was suggestive of their having been due to a common origin. Mr. Ward gave an account of an examination of some of the rock from Du Toit's Pan, with a view of discovering microscopic diamonds, none of which, however, had been found. Prof. Rupert Jones had been equally unsuccessful in the search for minute diamonds, both in sand from Du Toit's and in the ochreous gravel from Klip drift. He pointed out the waterworn condition of the agates from Du Toit's Pan, which showed aqueous action, though there were also several other minerals present in a perfectly fresh and unrolled condition. He thought a careful examination of the constituent parts of the gravel might ultimately throw light on their origin. That fluviatile action was sufficient to account for their presence had already been shown by Dr. Rubidge and others, who had treated of the grand plateaux and denudations of the district under notice.

Royal Geographical Society, November 13.—Major-General Sir H. C. Rawlinson, K.C.B., president, in the chair. The president, on opening the session, delivered an address, in which, after paying an eloquent tribute to the worth of the late president, Sir Roderick Murchison, and expressing his sense of the loss which the Society had sustained in his death, he reviewed the progress of geography since the last meeting of the previous Session. He congratulated the Fellows on being again permitted to meet in the handsome and commodious hall of the London University; and stated that the Council felt that the Senate of that body, in granting the use of the hall, conferred an obligation not only on the Society but on the public at large, whose instruction and education in geography formed the especial objects of their study. He also announced that the Society had, during the recess, taken up its permanent quarters in Savile Row, where it was now located on its own freehold estate. In Physical Geography the important subject of Oceanic Circulation, and Dr. Carpenter's researches thereupon, was prominently noticed; and he stated that Dr. Carpenter, during his Mediterranean voyage of the past summer, had met the objections of his critics in so far as related to the under-current outwards at the Straits of Gibraltar by experimentally proving that such a current really does exist. In Arctic exploration the recent German expeditions were noticed, particularly the voyage of Messrs. Payer and Weyprecht, who, last summer, had found an open sea, in lat. 70°, between Spitzbergen and Nova Zembla. In Central Asia and Eastern Persia much accurate information had recently been obtained by English travellers and surveyors; and in Syria their medallist, Captain Burton, had recently, in company with Mr. Drake, examined the Anti-Libanus and the little-known district east of Damascus,—subjects on which this indefatigable traveller would read papers at a subsequent meeting. An excellent descriptive paper had been received from the well-known and able traveller Captain Blakiston, on the subject of the island of Yezo, the circuit of which he had recently explored in the capacity of an official of the Japanese Government. No direct news had been recently received either from Dr. Livingstone or Sir Samuel Baker; but authentic intelligence of Livingstone could not be much further delayed, as an able and adventurous American gentleman, Mr. Stanley, left Zanzibar for the shores of Lake Tanganyika in February last, taking with him "Bombay," one of Speke and Grant's "faithfuls," as guide. He (the president) added that if Mr. Stanley succeeded in restoring Livingstone to us, or in assisting him to solve the great problem of the upper drainage into the Nile or Congo, he would be welcomed by the Society as heartily and warmly as if he were acting under their own immediate auspices.—A paper was then read "On the Exploration of the Limpopo River," by Captain Frederic Elton. This remarkable journey was performed between July 6 and August 8, 1870, the author starting from the Tati gold-fields and proceeding by an easterly route to the junction of the Tuli River with the Limpopo, and thence descending the great stream or marching along its banks to beyond the junction of the Lipalule, whence he struck across to Lorenço Marques, in Delagoa Bay. The middle part of the Limpopo, between the Tuli and Lipalule, was found to be encumbered with rapids and waterfalls, some of which, especially the cataracts called Tolo-Azime, were truly magnificent, the river, after a series of rapids five miles in length, here plunging over a ledge into a deep chasm. These falls mark the spot where the Limpopo leaves the great interior plateau of Africa and descends abruptly into the plains which extend henceforth to the sea. The paper described the country traversed as rich and abundant in game of all descriptions.

Mathematical Society, November 9.—Dr. Spottiswoode, president, in the chair. The following gentlemen were elected to form the council for the ensuing session:—President: Dr. Spottiswoode. Vice-Presidents: Profs. Cayley, Henrici, H. J. S. Smith, and Mr. S. Roberts. Treasurer: Dr. Hirst. Honorary Secretaries: Messrs. M. Jenkins and R. Tucker. Other members: Profs. Clifford and Crofton, Dr. Sylvester, Hon. J. W. Strutt, Messrs. T. Cotterill, Merrifield, Stirling, and Walker. Mr. A. Freeman was proposed for election. It being unanimously agreed upon that the number of honorary foreign members should be increased to six, the president read out the names which the council recommended for nomination, viz.: Dr. Clebsch, M. Hermite, Prof. Cremona, Dr. Hesse, and Prof. Betti. The only foreign member at present is M. Chasles. Dr. Sylvester then gave an account of his communication "On the partition of an even number into two primes." In one of his minor papers Euler has enunciated as a theorem, resting entirely on intuition from a comparatively small number of instances, that every even number may be decomposed into a sum of two primes. The object of Dr. Sylvester's communication was to obtain some measure of the probable number of ways in which such decomposition can be effected for any given number; if it can be shown to be probably greater than the square root of the number itself, it will follow from generally admitted principles of the theory of chances, that the probability of the theorem being universally true above any assigned limit, if *proved* to be true up to that limit, may be represented by an infinite product of terms, which will approach as near as we please to unity the higher the limit in question is taken. The mere fact of the theorem, as Euler gave it, being proved up to 100,000,000, or any other number however great, would leave the probability of its being universally true, absolutely zero, just as the fact of the sun having risen 100,000,000 times would not contribute an atom of probability to the supposition that it would continue to rise for all time to come. In the case before us, on the contrary, the probability of the theorem being universally true by a sufficiently copious induction, may be made to approach as near as we please to absolute certitude. The author considers that he has established beyond the reach of reasonable doubt that the magnitude which represents the mean probable value of the number of modes of effecting the resolution of a very large even number into two prime numbers is that of the square of the number of primes inferior to the given number divided by the number itself, or which (thanks to the discoveries of Legendre and Tchebicheff) we know to be the same thing, the number of the decompositions in question bears a finite ratio (assignable within limits) to the number to be decomposed, divided by the square of its Napierian logarithm. If we agree provisionally to call preter-primes in respect to n, those numbers which are prime themselves, and also when subtracted from n leave prime remainders, the author shows that the probable number of such preter-primes (*i.e.*, the most probable value attainable under our present conditions of knowledge) may be found approximately by multiplying the number of ordinary primes inferior to n by the product of a set of fractions, depending in part on the magnitude and in part on the constitution of the number n. If n is the double of a prime, the product in question is got by multiplying together all the quantities $\frac{p-2}{p-1}$ where p is every odd prime between unity and the square root of n; but if n itself contains any such primes among its factors, then the corresponding factors are to be omitted out of the product. We thus see that if two even numbers of considerable magnitude lie adjacent or tolerably near to each other, one of which is the double of a prime, but the other six times a prime, the number of preter-primes relative to the latter will be about twice as many as those relative to the former. For the purpose of greater simplicity of explanation, the formula of approximation has been stated above with less accuracy than it admits of being stated with. Instead of the total number of odd primes being multiplied by the product of factors last described, those only should have been taken which are not intermediate between 2 and $\sqrt{n}$, and the result so modified should have been stated to be the probable value not of the total number of preter-primes, but only of such of them (by far the larger number) as are not of the excluded class above described, nor subtracted from n, give rise to remainders belonging to such class. The author has found by actual trial on an extensive scale, that the estimated values of the number of decompositions never differ by more than a moderate, and in some cases exceedingly slight, percentage from their actual values determined by the use of Borchardt's tables. The same methods enable him also to assign a probable value to the number of modes of resolving an odd number into the sum of one prime and the double of another, and in general lead to an approximate representation of the number of solutions in prime numbers of any system of linear equations of which the total number of solutions is limited, and even to resolve approximately such questions as that of determining how many prime numbers there are inferior to a given limit, which are followed by prime numbers differing from them by any assigned interval. Since the communication made to the Mathematical Society, the secretaries have been favoured with a note from which they understand that Dr. Sylvester has verified his results by quite a different method. The exact number of the solutions of the equation $x + y = n$ in prime numbers may be expressed algebraically by means of the method of generating functions in terms of the inferior primes to n. The expression will be found to consist of two parts, one a constant multiple of n, the other, a function of the roots of unity corresponding to the several inferior primes and their combinations. The former non-periodic part may obviously be regarded as the even value of the expression, and Dr. Sylvester has found that it is identical with the value obtained by the method of averages previously employed. In order to prove strictly Euler's theorem, it only remains to show that the entire expression can never become zero. This Dr. Sylvester believes he has the means of doing, and at the same time of assigning exact limits to the number of solutions in question; but in a matter of so much moment, and of such singular interest, does not wish to express himself in a more decided manner, until he has had the opportunity of subjecting his method to a further rigorous examination.

Royal Astronomical Society, November 17.—Mr. W. Lassell, president, in the chair. The Astronomer Royal showed a drawing of Encke's comet made by Mr. Carpenter of Greenwich; it gave the impression of a somewhat shuttlecock-shaped nebulous haze, with two wings of much fainter light, extending on either side, giving a flattened appearance to the head of the comet. Dr. Huggins made a drawing which coincided in all essential particulars with that of Mr. Carpenter. He thought that he had detected a very minute but distinctly-marked nucleus in the paraboidal-shaped head of the shuttlecock. The whole light of the comet was very faint, but he had succeeded in ob aining its spectrum, which, as in the case of that of Comet II, 1868, consisted of three bands, apparently identical with the bands in the spe trum of the vapour of carbon. The middle band situated near "little b" was much brighter than than the other two, and he was quite satisfied of its identity with the middle bands of carbon vapour; the two outlying bands were much too faint for him to speak with confidence of their identity, but they appeared to correspond. The Astronomer Royal showed a celestial globe, on which he had fixed a small white wafer in the place occupied by the sun, and a piece of white paper cut out to represent the comet. He pointed out that its longer axis was directed almost exactly to the sun, and that its head and nucleus were turned away from the sun. This appears to be the almost universal rule with the smaller class of comets. Unlike the sheep of little Bo Peep they carry their tails before them, and not until their smaller fan-shaped appendages have been well warmed by the sun's rays, do they begin to shoot out large tails in the other direction.—A paper was read by Prof. Grant, in which he pointed out that as early as the year 1852 he had recognised the continuity of a red envelope enclosing the sun, of which the prominences were merely the more elevated portions; he had come to this conclusion from a comparison of the observations made during the total eclipses of 1842 and 1851.—A discussion then followed as to whether there were any permanent markings upon Venus. Dr. W. De la Rue and Mr. Browning affirmed that they often saw spots and other irregularities of surface. The authority of Mr. Dawes, and many other observers of note, was cited to the contrary.—Some careful drawings of the Zodiacal light as seen by Captain Tupman while cruising in the Mediterranean were handed round. It was pointed out by Mr. Ranyard that the axis of symmetry of the light was in many instances greatly inclined to the ecliptic, and that the distance of the node of the axis from the sun was in some instances more than 40°.

BOOKS RECEIVED

English.—The Geology of Oxford and the Thames Valley: J. Phillips (Macmillan and Co.).—Weale's Treatises: Rudimentary Geology; Historical: R. Tate (Lockwood and Co.).—Profitable and Ornamental Poultry: H Piper (Groombridge and Sons).—Ganot's Elementary Treatise on Physics, Experimental and Applied: Translated by G. Atkinson, 5th edition (Longmans and Co.).—Tables of Velocity, Time of Flight, and Energy of Various Projectiles; Bashforth Chronograph (E. and F. Spon).—The Discovery of a New World: G. Thomson (Longmans and Co.).

Foreign.—(Through Williams and Norgate)—Les Migrations Humaines en Océanie d'après les faits naturelles: Jules Garnier.

DIARY

THURSDAY, November 23.

Royal Society, at 8.30.—On the Behaviour of Supersaturated Saline Solutions when Exposed to the Open Air: C Tomlinson, F.R.S.—On Experimental Determination of the Velocity of Sound: E. J Stone, F.R.S; (1) Second Paper on the Numerical Value of Euler's Constant, &c.; (2) Second Paper on the Numerical Values of ϵ, log ϵ^2, log ϵ^3, log ϵ^5, and log ϵ^{10}, &c.: W. Shanks.

Society of Antiquaries, at 8.30.—On Mediæval Representations of the Months and Seasons: James Fowler, F.S.A.—On some Casts of Ivories from Cologne: Augustus W. Franks

London Institution, at 7.30.—The Influence of Geological Phenomena on the Social Life of the People: Harry G. Seeley, F G.S.

FRIDAY, November 24.

Quekett Microscopical Club, at 8.—On the Minute Structure of Tremelloid Uredines: M. C. Cooke.

MONDAY, November 27.

Royal Geographical Society, at 8.30.—Exploration of the Volcanic Districts East of Damascus: Capt. R. F. Burton.—Journey in Southern Arabia: Baron de Maetzan.

London Institution, at 4.—Smell, Taste, and Touch: Prof. Huxley, LL.D., F.R.S. (Course on Elementary Physiology).

WEDNESDAY, November 29.

Society of Arts, at 8.—On Tramways and their Structure, Vehicles, Haulage, and Uses: W. Bridges Adams.

Archæological Association, at 8.

THURSDAY, November 30.

Royal Society, at 8.30.—President's Address.

Society of Antiquaries, at 8.30.

London Institution, at 7.30.—Science and Commerce, illustrated by the Raw Materials of our Manufactures, (II.) P. L. Simmonds, F.R.C.I.

CONTENTS

Page