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THURSDAY, DECEMBER 1, 1870

MEDICAL SCHOOLS IN ENGLAND AND GERMANY

III.

THERE is something elevating in the thought that the hospital, while it provides care for the sick, at the same time makes them useful for the purposes of instruction in the service of mankind. Still it is painful, on the other hand, to think that the patient enjoys this attention only in order to be made as profitable as possible to others; that he is given over to a crowd of curious seekers for knowledge, who feel his painful spots, percuss and auscultate his weakened body, and, in short, by proceedings of various kinds, disturb the rest he so heartily longs for. Such evils are not indeed of any account, so long as the number of students does not exceed a certain limit. Experience teaches that a patient is pleased to see a certain number of doctors about him; he soon gives them his confidence, he looks upon them as his friends, and willingly allows himself to be examined, partly with the idea that it will be better for him if the examination is several times repeated, partly in acknowledgment of kind attentions which are shown him. The difficulty arises when the number of students is so large that the patient can no longer feel at home with them. Apart from the fact that the disturbance of the patient increases with the increase of the crowd about him, it is also of the utmost importance to consider that the greater number of students must remain total strangers to him, that they are for him only intruders, who are learning from his body without offering him anything in return.

The situation becomes, however, actually distressing in the neighbourhood of a distinguished teacher in one of the large schools. An inquisitive throng crowds about the sick bed, utterly regardless of the patient's comfort. The hospital becomes a market, the sick merchandise.

Inasmuch as it is important for us to give the relations of hospital and school a basis as humane as possible, we must inquire what those large schools have to offer in the way of medical instruction, and whether we may take the advantages as a set-off to the evils.

As far as the art of medicine is concerned, we can maintain, without fear of opposition, that, *cæteris paribus*, clinical medicine will be taught with less success, the greater the number of students who crowd about the bed. The technical part of medicine should be taught orally. The student should be drilled in it as in a trade. In general, however, and to a certain point, the capability of the person so drilled increases in proportion to the time which the teacher devotes to him personally, and it must therefore be in inverse proportion to the number of pupils whom a teacher has to instruct.

Experience, in fact, teaches us that with large numbers of pupils the standard of individual capability is apt to be extremely low. In Vienna, where about 300 students are instructed in clinical medicine around each bed, the amount of drill which each receives comes so near zero that for practical life it is scarcely to be taken into account. The students discover this as soon as they have left their studies behind them and begin to look forward to practice. They distribute themselves then among the hospitals where there are no students' tasks, and there, with great loss of time, and under discouragements of many sorts, they are instructed in subjects which, according to their diplomas, they should have learned long before. Between such extreme unsuitableness and a really healthy state of instruction, there is, however, a long road leading us through many schools of various grades.

Instruction in medical science is good only where it happens to be endurable by the patient — that is, in smaller schools, where a few students only collect about his bed. Medical instruction has still another side. The student must not only learn the handicraft, he must also, through well-instructed teachers, be made familiar with the delicate processes which require delicate instruments, and experiments, and methodical thought for their elucidation; for it is only by intercourse with thinking men that he can learn how careful observation and earnest thought on the phenomena with which his whole life will be occupied, can be made available for practical ends.

In this connection, also, the question of the value of instruction in hospital wards must be answered. In point of fact the *practising physician* has always hitherto been regarded as the high school of medical science. Famous systems of medicine—that is, theories upon the nature of disease—took their start in hospital wards, and were taught at the bedside by distinguished masters.

In this country, however, a total revolution has taken place, speculations upon systems of medicine are supplanted by exact medical science, and this, for the moment, has fled from the ward and taken up its abode in the laboratory—here, at the present time, the most distinguished medical men and working pupils of superior gifts attach themselves to the laboratory, and remain there to study. Hence the phenomenon that, in Germany, in the department of pure clinical medicine, there is a want of worthy representatives. We are not to conclude on this account that our generation is inferior; it is only that the distribution of its scholars is different.

The modern students of clinical medicine seek to found their reputation by work in the laboratory, and they are unable to stand competition with those workers who are at home in it. First, because in general their ranks are not recruited by thinkers of the first order; secondly, because their profitable business coming in the way hinders their thorough education. They leave, therefore, the firm ground which the morphology of disease affords, and trust themselves to the weak ship of experimentation, which they are not able to steer.

Even if the results of such investigations are taught at the bedside, the highest school of medical science is not to be sought there. The patient ought not to be disturbed, and the drill of the student neglected, for the sake of a science which is, or ought to be, better studied in the laboratory. It is the morphology of disease which belongs directly to the sick bed. One must have seen distinguished teachers of this kind, one must have observed how they bring to light latent indications of disease, in order to concede that to such men, even a little misusage of the sick might be allowed in the interests of education. This bringing to light and grouping of phenomena for the purposes of diagnosis, is nowadays, indeed, not called science. It is an intellectual act, such as everyone performs when he defines a plant, a mineral, or a commodity. The combination of the symptoms and definition of the disease is, in fact, an important element of the physician's art, and must be included in every course of instruction. Still "Eines schickt sich nicht für Alle."

When Cuvier determined to what primeval animal a bone found buried in the earth must have belonged, it was only a definition; yet no one will hesitate to rank it amongst the noblest exercises of the human intellect. So, too, the diagnoses of distinguished masters may be ranked with the greatest achievements of medical investigators. To forbid such masters to teach at the bedside because many students crowd about them and disturb the patient, would be absurd. The instruction of such masters is, however, fit only for connoisseurs. For the uncultivated taste of the beginner it is unsuited. If it is besides so diluted as, with a large number of students, must invariably be the case, according to an unalterable law, it entirely loses its value. If, then, the founders of hospitals or their administrators would diminish to a certain definite and very low standard the number of students who, at one time, should be allowed to enjoy the privilege of instruction in a hospital ward, they would certainly do no injury to the interests of education. On the contrary, if liberal in other respects, they would, by such limitation, further these interests.

In what shall this liberality consist? Simply in this, that all available space in the hospital shall be open for the purposes of instruction, and that as many teachers shall be admitted as is consistent with a proper classification of the patients. We say *admitted*, and this is really the fittest expression, for there is not the smallest doubt that always and everywhere the ablest men would be eager to accept the place of teacher. The intellectual stimulus which an able physician derives from association with a school and intercourse with young men, and the moral support which a situation as instructor gives him in regard to his patients, are sufficient compensations for the required expenditure of time.

The physicians and surgeons who at present have large amounts of material at their disposal in large hospitals, would certainly not be willing to support such a system of division. They would complain that they are deprived of the possibility of attaining great skill. There is some truth in this objection. It is a benefit to mankind when a surgeon, by extensive practice, attains superior skill in the performance of certain operations. But the advantage which a few derive from his technical skill is counterbalanced by the loss which the rest sustain from the superficial manner in which they are passed over with a mere glance. In such a hospital more things are overlooked than one would imagine; and the pupils profit far less from the skill of the teacher than they lose from the superficiality of the teaching. It is, moreover, untrue that skill is likely to suffer from a further division of material. We have examples of great operators who never occupied a position in a hospital. Nor can it be doubted that the greater the number of those to whom the opportunity is given of proving their skill, the greater will be the number of skilful surgeons.

In the plan here proposed, many readers may see only what is already introduced in England, that is, small schools for the special study of medicine. The writer of this article is, however, far from speaking in behalf of schools of this kind. Such schools are fit only for the production of craftsmen; and medical men, though they must be craftsmen, must not be mere craftsmen. Such small special schools can serve only as preparatory appendages to the larger educational bodies, that is to say, to the Universities, whose function it is to foster the sciences for their own sakes.

Every medical student ought to obtain his education in general physical science and special medical science at the University; and while doing so he must acquire the art of medicine in a hospital, just as the young botanist must study in the field at the same time that he attends lectures.

The small Universities in Germany answer both purposes to an approximate degree. There one finds excellent schools with so few students that they can obtain both the higher scientific education and drill at once. To approach towards such circumstances, without giving up the great advantages which large schools offer for the development of science, is the end towards which we must strive. S. STRICKER

POLARISATION OF THE CORONA

As this forms one of the most important questions to be settled during the coming Eclipse, it becomes desirable to reconsider the observations already made on this subject. Arago first suggested that the polariscope should be used on the corona, but apparently did not anticipate any decided results. The principal observations since made are the following:—

1842.—Arago at Perpignan. Used a polariscope *a lunulis*, that is, a double-image prism and crystal. He found the two images of complementary tints, the colour extending over the sky around the corona, the corona itself, and even over the disc of the moon.

1842.—Mauvais at Perpignan. Used a Savart's polariscope. He saw the bands very distinctly on the corona, and faintly on the moon itself. Their maximum of intensity corresponded with the horizontal position of the bands. Evidently he should have found another maximum when the bands were vertical.

Both this and the preceding observation show the existence of atmospheric polarisation extending even over the disc of the moon. Its plane must have been the same throughout, or Arago would have seen the different parts varying in tint. The maximum noticed by Mauvais shows that the plane must have been either vertical or horizontal, that is, not oblique.

1851.—Abbadie at Trocdesecksevérk. Inserted a plate of quartz between the object glass and eye-piece of his telescope, and applied a double refracting prism to the eye-piece as an analysis. He found the light of the corona strongly polarised, but saw no traces of colour on the moon. He was, however, troubled by clouds.

1851.—Dunkin at Christiania. Found no traces of polarisation, but was troubled by clouds.

1851.—Carrington at Lilla-Idel. Used a Nicol's prism, but found no polarisation.

1858.—Liais at Paranagua. Instrument used, a Savart's polariscope. He found the plane normal to the limb of the sun, and the intensity small, but greater than that of the moon.

He also remarks that the neutral point of sky polarisation was in the neighbourhood of the sun, a statement difficult to comprehend, as the neutral points are commonly defined by their distance from the sun.

1860.—Secchi on Mont St. Michel. Used an Arago's polariscope, and found that the images were not of equal colour, and that one was elongated in one direction, the other in a direction perpendicular to it. This last appearance was probably imaginary, as the crystal in the polariscope would prevent the extinction of any polarised rays.

1860.—Pragmowski at Briviesca. Used a plate of right and left-handed quartz at the common focus of the object glass and eye-piece, and a Nicol's prism in the eye-piece. This combination should give two semicircles of complementary tints when the plane of polarisation is oblique to the line of junction of the quartz. Using a power of 22, placing the line of junction vertical, and bisecting the sun, he found the top and bottom alone of uniform tint, the two semicircles being very strongly coloured, one red, the other green. He thence inferred a radial polarisation. In reality, in this case, he should have found the sides alike, as well as the top and bottom, only faint yellow instead of purple, and the colours most strongly marked at angles of 45°.

1868.—Campbell used a Savart's polariscope and found the bands strongly marked, having a maximum at 140° from the vertex.

1868.—Winter used a similar instrument, and, as a result, found the polarisation very strong, especially close to the sun.

1869.—Pickering at Mount Pleasant. The writer used an Arago's polariscope, and found the sky polarised close to the corona, the plane being the same on all sides of the sun.

1869.—Smith, at Eden Ridge, records a similar result obtained by a member of his party.

We see, therefore, that the results are very variable, the polarisation of the corona, if any, is obscured by that of the sky, probably due to secondary reflection. It is therefore very desirable to use some means of neutralising this effect. One remedy is to place a double-image prism in front of the telescope, which thus superposes two images of the sky polarised at right angles. For observations on the sky no telescope should be used, or the light will be too much enfeebled. A Savart's polariscope is the most delicate instrument, but such a one as the Arago is more useful to determine just what portion of the light is polarised. The Nicol's prism and the double-image prism give such indefinite results, that little could be expected of them, and they have been tried by several observers without success. The best instrument to measure the intensity of the polarisation is the polarimeter, consisting of several glass plates, which can be set at an angle in front of a Savart, and the point of disappearance of the bands recorded. The absence of polarisation of the protuberances has been observed by Abbadie, Pragmowski, and others, and seems so well determined, their further examination is unnecessary.

EDWARD C. PICKERING

THE RESOURCES OF LA PLATA

The Mineral and other Resources of the Argentine Republic. Published by special authority of the National Government by Major I. Rickard, F.G.S. (London: Longmans and Co., 1870.)

MAJOR RICKARD has executed in a very creditable manner the task which the National Government deputed him to perform, and his volume will give its readers considerable insight into the vast material resources of the Argentine Republic. Hitherto La Plata, in spite of its name, has been regarded rather as a field for agricultural enterprise than as a source of mineral wealth; and the stories which were once current of mountains rich in precious metals have been forgotten in the details given by our countrymen of their successful farming in the pampas of the south. Various causes have combined to divert attention from the mineral riches of the country. The tedious contest with Lopez, only just concluded, and the turbulent character of the gauchos and Indian tribes, have checked the growth of confidence in the minds of emigrants or capitalists; and though the vigorous administration of President Sarmiento has already done much to remove these obstacles, some time must still elapse before investors will be persuaded that "the great Republic of the South" is likely to realise all the favourable vaticinations of which Major Rickard makes it the subject. What, however, has most retarded the progress of mining, and, indeed, of all industrial enterprise, in the Argentine Republic, has been the deficient population and the want of means of intercommunication and transport. Buenos Ayres and the other riverine provinces, where grazing is a pleasant and profitable pursuit, absorb nearly all the working power of the Republic, and at the present time not more than 2,687 persons are engaged upon any form of mining industry. If the reader will cast his eye over a map of the country (the absence of which in Major Rickard's book is a serious defect), he will see what a mere scratching of the soil can be effected by such a handful of men. Hence it is that very trifling results have hitherto been obtained from the few mines yet in operation, and that the processes for reducing the ore have remained defective and costly. The supplies drawn from the Argentine Republic produce no appreciable effect upon the metal markets of the world, and in popular estimation its exports solely consist of tallow, wool, and hides.

President Sarmiento, a man of no common discernment, is convinced that the substantial wealth of his country is to be found in its mineral resources, and, moreover, that they will provide the surest means for promoting rapid and extensive immigration. In their development is bound up the extension of commerce and the progress of agriculture. Had it not been for the discovery of gold, California might still have remained a vast cattle range to this day; and what is there to prevent La Plata, which can boast of the riches of Copiapo, Potosi, and Famatina, from rivalling her neighbour in wealth, population, and national importance?

The Argentine Republic is divided into fourteen provinces, and extends southwards from the Tropic of Capricorn to the 40th parallel of latitude. Roughly speaking, the characteristics of the country admit of a two-fold division; the northern and eastern provinces being metalliferous, and the southern and littoral provinces agricultural. The latter term must, however, be accepted with some qualification, inasmuch as the cereals hitherto produced have been very scanty, and to this day the Republic is an importer of flour. In truth, the natural pasture is so abundant, and alfalfa or lucerne thrives so luxuriantly, that stock-farming is practised almost to the exclusion of all other branches of agriculture.

Major Rickard remarks that mining in La Plata is at once in its infancy and in its old age. An instance of this anomalous state of things may be seen in Mendoza, where the ancient silver mines, El Rosario and San Rumaldo, which were discovered as early as 1638, are again in active operation. The old miners dealt merely or chiefly with what Spaniards term "warm metals" (*metales calidos*), that is, those which could be reduced directly by means of mercury, and this, therefore, left for a later generation the "cold metal" (*metal frio*) which required for its reduction the process of smelting. Silver mines are not by any means confined to the province of Mendoza. In San Juan (where civil war and revolution have long been fatal barriers to all industrial progress) the district of Tontal, on the slopes of the great Andine range, is peculiarly rich in argentiferous lodes; ordinary samples from the Mine Señor containing not less than 160 ounces to the ton, and first class samples yielding 400 ounces. But, in the opinion of Major Rickard, the silver mines of Famatina, in the more northerly province of La Rioja, are the richest in the whole country. That the difficulties in working them are formidable may be gathered from the fact that some are situated 13,000 feet above the sea-level, and that the whole district is deficient in fuel of any sort, and exposed during three months of the year to a rainfall so heavy as to compel the miners to suspend their labours. In the province of Catamarca copper is the predominant metal, and in union with it an appreciable percentage of gold and silver has been found. During the year 1868 the Restauradora mine produced 2,639 tons of ore, containing by assay 506 tons of fine copper; but it must be remembered that the prevailing systems of smelting are by no means perfect.

More than one auriferous district exists within the limits of the Republic, and those which are respectively named Gualilan and Guachi (from *Gua*, which in the Huerpe tongue signifies gold) have for many years enjoyed considerable celebrity. Both of them largely enriched their first workers, and there can be little doubt that thousands of tons of ore still exist in the old workings which have been abandoned, partly from natural difficulties, but principally from the want of skilled labour.

As to the other productions of the Republic, it is impossible in this brief notice of a copious volume to do more than mention them by name. Mendoza and San Juan possess silver-lead mines of considerable extent; and in the former province petroleum springs have been recently discovered. In Santiago del Estero large tracts of land are covered with indigenous indigo; rice and tobacco are cultivated in Tucuman, and in the most northerly provinces of Salta and Jujuy are thriving plantations of coffee.

After making every allowance for Major Rickard's natural enthusiasm, it must be admitted that the country whose resources he has so minutely described, offers many and great inducements to the British emigrant to give it a fair trial. President Sarmiento desires especially to attract a further immigration of our fellow countrymen, for he infers from the success they have already achieved in the cultivation of the Pampas, that their energy and enterprise will be invaluable in developing the mineral resources of the Republic, and that Anglo-Saxon coolness and perseverance will form a favourable counterpoise to the opposite characteristics of the Hispano-American race.

C. J. ROBINSON

OUR BOOK SHELF

An Elementary Course of Hydrostatics and Sound. By Richard Wormell, M.A., B.Sc. Fcap. 8vo, pp. viii. and 146. (London: Groombridge and Sons, 1870.)

THIS little book is "designed for the use of schools, colleges, and candidates for University and other examinations." In such a work it would of course be out of the question to look for novelty of matter: by the nature of the case, to praise the author's originality would be to cast a doubt on his accuracy; and, while inaccuracy would be inexcusable, no merit can be claimed for its opposite. Hence, in trying to form an estimate of a book like this, we are inevitably led to consider whether the subjects treated are arranged in a simple and natural order; whether the exposition of principles is clear and logical, the really fundamental matters being kept constantly and prominently before the student's mind, and special consequences and applications grouped about them in such a manner as to show distinctly their mutual connection and dependence: whether, in short, the book is scientific in treatment as well as in subject. We are sorry to say that, in these respects, our judgment of the work before us is by no means favourable. We should expect a student, instead of acquiring from it ideas which are capable of growth and expansion within his own mind, and being led towards the conception of the organic connection of all scientific truth, to conclude that science—or at least hydrostatics and acoustics—consists of a series of propositions which it is his duty to "get up" and write out on the first opportunity in answer to examination-questions. The least satisfactory parts of the book are the explanatory and descriptive portions, and especially the twenty-two pages at the end devoted to sound. The author says in the preface that "the whole contains all that is required on these subjects [hydrostatics and sound] for the B.A. and B.Sc. degrees of the University of London." If this is true as regards the latter subject, it is more to the discredit of the University than to the credit of his book.

Studien über das Central Nerven-System der Wirbelthiere. Von Dr. Ludwig Stieda, Prosector in Dorpat. (London: Williams & Norgate.)

DR. LUDWIG STIEDA is already well-known for his admirable papers on the central nervous system of osseous fishes, birds, and some mammals. The present work embraces a description of the central nervous system of the frog, rabbit, dog, cat, mole, and mouse; an account of the course of the fibres in the spinal cord of Vertebrata generally; a comparison of the brain of the various classes of Vertebrata with that of man, and finally, a comparison of the cerebral with the spinal nerves. Of the description of the brain and spinal cord of the several mammals mentioned above we need say nothing here, except to remark that the account is full and carefully drawn up; the minutest structure of the several parts being given as well as their coarser anatomical features. In regard to the brain of the frog, the parts of which have received such different names, Dr. Stieda gives the following description of the organ as it appears when viewed from the upper surface. The cerebrum presents the following parts in successive order:—1, the medulla oblongata; 2, the cerebellum; 3, the lobus opticus, with its median furrow; 4, the lobus ventriculi tertii (thalami optici of authors); 5, the lobi hemisphærici, each of which terminates anteriorly in a knob constituting the tuberculum olfactorium. On the under surface of the brain there appear successively from before backwards:—(1) the bases of the lobi hemisphærici; (2) the chiasma of the optic nerves, which last proceed from (3) the lobus opticus, and between which is situated (4) the hypophysis cerebri, and behind this (5) the base of the medulla oblongata. M. Stieda then gives a full description of these parts, and of the various cerebral nerves in the frog. To this succeeds a very good general view or *résumé* of the anatomy of the brain in mammals. We may draw attention to some remarks made in the section where a comparison is made between the brain of man and that of the several classes of Vertebrata. It may be premised that little difficulty is experienced in discovering the homologous parts of the central nervous system of man and the more highly organised mammals. In the birds, however, there are several parts that are difficult to decipher; whilst in Amphibia, and still more in fishes, the nature of the several parts has given rise to much discrepancy of opinion between different observers. Dr. Stieda refers to his former work for the brain of fishes. In regard to Amphibia and reptiles, he considers that the lobi hemisphærici, or anterior lobes, being hollow, and containing a ventricle, are clearly the analogues of the cerebral hemispheres of man. The azygous portion of the central cavity, between the posterior parts of the hemispherical lobes (or ventriculus communis) in the frog, is the indication of the primordial single cavity of the first cerebral vesicle, and consequently establishes the transitional stage between the osseous fishes and the higher Vertebrata. The succeeding segment constituting the lobus ventriculi tertii, (or Zwischenhirn) corresponds in its upper part to the thalami optici; in its lower to the tuber cinereum and lamina cinerea. The third segment, or lobus opticus, agrees exactly with that of fishes, both in its external and internal relations, whilst reptiles exhibit the intermediate type between fishes and birds. Of the nature of the cerebellum there can be no doubt. In regard to birds, he observes, that the great club-like segment of the cerebrum of birds corresponds to the hemisphere of man, the bodies enclosed in them to the corpora striata, the radiated septum to the septum pellucidum. He considers the existence of parts analogous to the corpus callosum and fornix of man to be doubtful. The succeeding segment corresponds to the optic thalami; the large spheroidal body of the lobus opticus to the corpora quadrigemina. Two plates accompany the treatise, which are devoted to the histology of the parts described. H. P.

LETTERS TO THE EDITOR

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

The Difficulties of Natural Selection

As Mr. Bennett complains that I have charged him with errors he has not committed (which I should much regret to have done), I must ask permission to justify my statements by a reference to his own words.

1. Mr. Bennett says that he is unable to discover where he has led his readers to understand that there is only one completely mimicking species of *Leptalis*. I will therefore show him where he has done so. In the third column of his article (p. 31) he says: "Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ* in their size, shape, colour, and manner of flying, being nearly pure white. There is, however, *one particular species* of *Leptalis*, which departs widely in external facies from *all its allies*, and so closely resembles a species of *Ithomia* as to deceive," &c. &c. Then comes the argument and the mathematical calculations always referring to "the *Leptalis*," and it is at the end of this, at the bottom of the next column, that we have the following passage (of which Mr. Bennett in his reply has only quoted a line and a half): "For supposing the chance is reduced from one in ten million to one in ten thousand, and it is said that the world has existed quite long enough to give a fair chance of this having occurred once, it is not a solitary instance that we have. Mr. Bates states that in a comparatively small area several distinct instances of such perfect mimicry occur, Mr. Wallace has a store in the Malay Archipelago, Mr. Trimen records several of wonderful completeness in South Africa," &c. Now, as there is not a word here about other species of *Leptalis*, but only about other cases of mimicry, as *Leptalis* is unknown in Africa or the East, as mimicry occurs in other genera and families of Lepidoptera, and other orders of insects, and as Mr. Bennett has himself stated, that the "*one particular species of Leptalis* departs widely in external facies from *all* its allies," I think it will be admitted that I was justified in asserting that Mr. Bennett's readers would be "led to understand," that there was only one species of completely mimicking *Leptalis*. If I was not so justified I confess my ignorance of the English language, and beg Mr. Bennett's pardon.

2. I leave your readers to judge for themselves whether the fact of a *Leptalis* having twenty offspring does or does not affect the mathematical argument as set forth by Mr. Bennett; but when, in answer to my statement, that the right variation has, by the hypothesis, a greater chance of surviving than the rest, he asks: "By what hypothesis? The hypothesis that these small variations are useful to the individual, the very hypothesis against which I am contending as unproved,"—I must protest against his denying his own words. For, at p. 31, col. 1, he says: "The next step in *my argument* is, that the smallest change in the direction of the *Ithomia* which we can conceive, on any hypothesis, to be beneficial to the *Leptalis* is, at the very lowest, one-fiftieth of the change required to produce perfect resemblance;" and six lines farther on, "For the sake of argument, however, *I will suppose* that a change to the extent of one-fiftieth *is beneficial*," and then comes the calculation. Again, I must acknowledge my ignorance of the meaning of words if Mr. Bennett does not here directly contradict himself. I never said the hypothesis was proved, but only that Mr. Bennett's argument, founded on it, was unsound, and for the sake of the argument he had admitted the hypothesis.

Mr. Bennett goes on to say: "The new factor, of which I take no account, is, again, entirely dependent on the admission of the natural selectionist premiss." This new factor is the principle of *heredity*. As he acknowledges that he takes no account of it, we must presume that he denies its existence; and as the whole of Mr. Darwin's theories and my own fall to the ground without it, he might have spared himself the trouble of his "mathematical demonstration."

3. I do not consider, as Mr. Bennett seems to do, that the distinction between "protective resemblance" and "mimicry" is a subtle one. Anyone who reads his paragraph on this subject (p. 32, col. 2) will, I think, be under the impression, as I was, that he alluded to mimicry, or mimetism, properly so called, as being strongly developed in birds. It seems, however, that he means only protective resemblance; but this, I believe, to be equally common among the very lowest forms of life. Transparency, for example, is a great protection to aquatic animals, and it is very prevalent in low organisms. Fishes are all, or almost all, protectively coloured, by the back being dark and the belly light, so that, whether looked at from above on the dark background, or from below on the light one, they are equally difficult to see. In many fishes, too, we have a specific protective resemblance as perfect as in any birds (see "Contributions to the Theory of Natural Selection," p. 55), and this is as much opposed to Mr. Bennett's theory as the absence of true mimicry in birds and mammals.

4. Mr. Bennett says, I have "brought no evidence to show that *extremely small variations* afford any immunity from the attacks of enemies,"—but this was quite unnecessary, because I show that the variations which continually occur in insects are by no means "extremely small." He also says that I "give no explanation of the tendency of the *Leptalis*, referred to by Mr. Bates, to produce naturally varieties of a nature to resemble *Ithomiæ*." But Mr. Bates introduces this remark with—"It would seem as if;" and though I think that the fact may be so,

and that it is not difficult to explain, yet I do not feel bound to explain every supposed fact as if it were a well-established one. As to the "parallelism of the development of protective resemblance and of instinct in the animal world," which I am also asked to explain, I deny that it has been proved to exist.

In conclusion, I will observe that the theory of Natural Selection, and its subordinate theory, Mimicry—have now been so fully developed by Mr. Darwin, Mr. Bates, Mr. Trimen, and myself, that I conceive it to be a full and sufficient answer to any opponent if we can show that his particular objections are unsound. This, I believe, I have done in the case of Mr. Bennett, although I am sorry to find that he cannot see it, and it is therefore unnecessary to go fully into the collateral points on which he has touched, and which have already been sufficiently explained by Mr. Darwin or myself.

ALFRED R. WALLACE

I AM forcibly reminded of Pope's lines,

> A little knowledge is a dangerous thing:
> Drink deep, or taste not, the Pierian spring,

by the argument used by Mr. Bennett in the P.S. to his letter in NATURE, of the 24th November, in which he says, after quoting a passage from a paper by Mr. Jenner Weir: "Here at least it would seem as if *imperfect* mimicry was anything but beneficial to the individual; how can the principle of natural selection account for its propagation in these instances?" He considers that a little mimicry is a dangerous thing. I would rather agree with Lord Brougham in his remark on the above lines, that as a little knowledge is better than great ignorance, so a little mimicry is better than great dissemblance.

But the case referred to by Mr. Jenner Weir is plain, and the argument, instead of being against the theory of natural selection, is really in its favour.

Some of the larvæ in question, for some reason of which we are unaware, are not so palatable to birds, and they, therefore, are not eaten by them to the same extent. These larvæ have not so much need of the aid of protective resemblance, and indeed their hair, spines, and gay colouring are advantageous to them instead of a drawback. The smooth-skinned larvæ require the aid of protective resemblance for their preservation, but no one would for a moment expect that because an insect has a protective resemblance to the place on which it rests, that every individual is to escape destruction by its enemies.

Mr. Bennett again asks for an explanation of the tendency of the South American *Leptalidæ* to resemble *Ithomiæ*. I think the reason is clear. Mr. Bates, in his paper, read before the Linnean Society in 1862 (Trans., vol. 23), states that the *Leptalidæ* are exceedingly rare compared to the *Heliconidæ*, and that the proportion is about 1 to 1,000, and also that none of the *Leptalidæ* are found in any other locality than those of the *Heliconidæ* they mimic. From this I should judge that the *Leptalidæ* cannot make head against their enemies, and require the assistance of mimicking some better protected species to be able to maintain itself.

November 25 S. N. CARVALHO, JUN.

PROFESSOR HUXLEY has referred Mr. Bennett to the highest authority for an answer to his reasoning on a difficulty in the theory of natural selection. Meanwhile, Mr. Wallace has replied on his own account. Upon the biological question I do not presume to touch, but I wish to say a word upon the mathematical one, especially as I cannot think Mr. Wallace has really met this part of the argument.

Mr. Bennett's argument is shortly this. A modification must be advantageous before natural selection can take hold of it. In order to be advantageous, it must not be too small; it must be so great as to be attainable only in the course of many generations, during which, in the absence of natural selection, we must see whether chance will carry us over the ground. As an extreme concession, he supposes that an advantageous *amount* of change might be accumulated in twenty steps; and, assuming that the required *direction* of change is only one out of twenty directions equally probable, he easily shows it to be violently improbable that a stationary population of one million should produce a single instance of even ten such steps in successive generations.

But why is it necessary to suppose the steps made in successive generations? Provided that the required number are made within reasonable time, it may surely be immaterial what intervals of merely unprogressive variation may elapse between them. In 200 generations, the first, fifteenth, fiftieth, for instance, and seventeen more, might make steps in the right direction, and all the rest might make steps in some or all of the other nineteen possible directions. Ten would in fact be the most probable number of steps in the right direction, and it would be about an even chance that there were ten at least.

However, as soon as we suppose steps in other directions, we must allow for the possibility of steps which shall actually reverse such progress as might be made in the right direction. If one change out of twenty equally likely is in the right direction, there will be on an average one in the opposite direction, and eighteen in indifferent directions. If we assumed that, in 200 generations, 180 were neutral, while twenty made steps forward or steps backward, these twenty *might* be all forward, and the chance that they were so would be one in 2^{20}, or one in little more than a million. Generally, the number of neutral steps would be a little more or a little less than 180, and if we allow for this the resulting chance will be considerably increased. Several instances would probably be produced by a population of a million; and I presume it is easy to allow much more than 200 generations of butterflies.

Nov. 23 C. J. MONRO

Dr. Nicholson's "Zoology"

I NOTICE in NATURE for Oct. 20, a review by Mr. E. Ray Lankester, of a Manual of Zoology recently published by me, and I crave a small portion of your space to say a few words thereon. Upon Mr. Lankester's zoological strictures on my work I will not enter, partly because the public verdict on the merits of my work has already been very emphatically and decisively expressed; partly because the sins laid to my charge are chiefly of *omission* and not of *commission*, and are, therefore, more or less inevitable in a work of such limited compass; and partly because it must be patent to everyone how much more admirably the work, unfortunately left to me, would have been discharged by Mr. Lankester himself.

In the matter of *Greek*, however, Mr. Lankester really must excuse me if I decline to bow to his superior knowledge. I am well aware that he probably entertains a fresher recollection of his school days than I can boast of, and I might, therefore, without shame, have pleaded guilty to some obliviousness of Greek roots. Mr. Lankester, however, has been singularly unlucky in the point of attack chosen by him. He takes upon himself to condemn the whole of the glossary to my work, because he finds the *twelfth* word of the same ("actinomeres") derived from the Greek word *aktin*, and he is good enough to add the information that "there is no such Greek word as *aktin*." Now, any decent lexicon would have informed Mr. Lankester that *aktin* is not only good Greek, but that it is the original form of the word, and that *aktis* was employed for the first time by Pindar, not, therefore, till about 450 B.C.

In conclusion, if I may be permitted to make a suggestion, I would recommend Mr. Lankester, in his capacity as critic and appraiser of the work of other men, not to judge in future of the value of a haystack by the first straw that he may happen to pull out of it; or, if he must do this, to be very sure before giving his opinion to the public, that it *is* a straw that he has succeeded in laying hold of.

Newhaven, Edinburgh H. ALLEYNE NICHOLSON

DR. NICHOLSON's extraordinary assertions as to the supposed word "aktin" really demand no serious discussion, which, indeed, would be out of place in NATURE. A reference to Liddell and Scott's Lexicon will conclusively demonstrate to any person interested in the matter that he is entirely wrong. The following additional blunders in Dr. Nicholson's glossary will enable your readers more fully to judge of his accuracy, and it will require considerable boldness to attempt to justify them by reference to imaginary archaic forms:—1. In several places we find Dr. Nicholson giving "poda" as the Greek for "feet," a gross grammatical fault. 2. "Pseud*os*" is given as the adjective corresponding to the English word "false." 3. "Enchuma" is said to be a Greek word meaning "tissue." It has not this meaning. Dr. Nicholson's mistake arises from ignorance of the origin of the signification of the word "parenchyma." 4. "Laima" is given in several places in the glossary for "throat," in place of "laimos."

It is improbable that these are anything but a fraction of Dr. Nicholson's etymological misrepresentations. Mistakes in the glossary of a zoological work are not of very great importance, and would not in this case have demanded notice had they not been fair samples of the general character of the book in which they occur.

I much regret that the fact of the writer's name being appended to the notice of Dr. Nicholson's work should have led him into the region of personalities, whither I do not intend to follow him. E. RAY LANKESTER

Glass Floats off the Isle of Lewis

IT would be of great importance if the *fact* could be ascertained whether the floats are from the Norwegian or from the Canadian fisheries. Your note of November 10 says, "They are hermetically sealed, and have certain characters, such as IV. or VI., impressed on the sealed part." Doubtless your columns are read in Norway as well as in Canada, and possibly a correspondent, from these characters or from other evidence, may claim the floats for one or for the other side of the Atlantic. In favour of the *west* side, but with the utmost deference to the opinion of Mr. Gwyn Jeffreys, I suggest that a north-*east* wind is an unlikely conveyance to "the *west* side of the Island of Lewis," or to "the *western* coast of Shetland."

A writer in the *Athenæum* of this week (Nov. 19, p. 659) thinks that these "net floats" are carried to Nova Zembla, and "still farther to the north and east" by the Gulf Stream. Dr. Carpenter supposes a constant warm surface current from each tropic to each pole, and a constant cold current below from each pole to each tropic, caused as we cause the currents of water to warm our houses. Suppose this grand theory to be true. The surface current should warm east coasts as well as west coasts. The same parallel touches England, Newfoundland, and Vancouver's Island. The climate of England and of Vancouver's Island on west coasts is "insular." The climate of the *island* of Newfoundland on an east coast is "excessive." This difference of climate in islands, on the same parallel, at the same levels, results from currents of *air*, not from currents of *water*—namely, from the prevalence of south-west winds in the north temperate regions. In winter with a south-west wind we hunt, with a north-east wind we skate.

GEORGE GREENWOOD, Colonel

Brookwood Park, November 19.

ENGLISH GOVERNMENT ECLIPSE EXPEDITION

THESE arrangements and instructions are not yet finally completed, but it is thought that the latter may be useful to members of other Expeditions, though they are not yet by any means complete.

A. SPANISH AND ALGERIAN PARTY.

NOTE.—*This party leaves Portsmouth in the "Urgent," on the 6th proximo. Observers to be on board by 5 P.M. on the 5th.*

1. *Cadiz Detachment.*—In charge, the Rev. S. J. Perry. Spectroscope, the Rev. S. J. Perry and assistant (Mr. Hostage), Mr. Abbay; Polariscope, Mr. Moulton, Mr. Hudson: Sketches of Corona, Mr. Naftel, Mr. Smyth, Mr Penrose, Mr. Collins; Time and General Observations, Captain Toynbee.

2. *Gibraltar Detachment.*—In charge, Captain Parsons. Spectroscope, Mr. Carpmael, Mr. Gordon; Polariscope, Mr. Lewis, Mr. Ladd; Photography, Mr. Buckingham, and assistant (Mr. Spiller); Sketches of Corona, Mr. Hunter, two Oxford men; Saturn in the Corona, Mr. Talmage, Mr. Maclear; Chemical Intensity, Mr. Thorpe.

3. *Oran Detachment*—In charge, Mr. Huggins. Mr. Huggins, Admiral Ommanney, Rev. F. Howlett, Mr. Carpenter, Mr. Crookes, Captain Noble, Dr. Gladstone, Prof. Tyndall.

B. SICILIAN PARTY.

NOTE.—*This Party leaves London, overland, at 8.45 P.M. from Charing Cross, on the 7th proximo.*

In charge, Mr. Lockyer. Spectroscope, Mr. Lockyer and assistant (Mrs. Lockyer), Prof. Roscoe and assistant (Mrs. Bowen), Mr. Seabroke and assistant (Mr. Burton), Mr. Pedlar; Polariscope, Mr. Raynard, Mr. Griffith, Mr. Clifford; Sketches of Corona, Mr. Brett, Mr. Darwin; Photography, Mr. Brothers, Herr Vogel, Mr. Harris; Time and General Observations, Mr. Vignoles, Sen., Mr. Vignolles, Jun.

INSTRUCTIONS TO OBSERVERS.

Instructions for the Polariscopic Observations of the Corona, including Beams and Streamers.—It is recommended that the polariscopic examination of the Corona be carried on as follows:—

1. To examine a detached and selected part of the Corona about 6′ from the limb of the sun, and say about 8′ in diameter. 2. A field extending from the limb of the sun outwards should be examined either with a Nicol's prism, or a double image-prism. 3. The light of the streamers at some distance from the sun should be examined with a Nicol and a crystal. 4. The polarisation of the Corona should be examined in such a manner as to eliminate atmospheric polarisation. *Note.*—The most suitable instruments for ascertaining the plane of polarisation, and the proportion of polarised to unpolarised light are (1) a double-image prism; (2) Savart's polariscope; (3) a plate of quartz, consisting of two compensation wedges turned through an angle of 180°; (4) a plate of arragonite, or calc spar, cut perpendicular to an optic axis, and affixed to an analysing prism; (5) a polarimeter, consisting of four plates of glass, movable on an axis perpendicular to the plane of polarisation.

First Observation.—The object of this observation is to observe the polarisation (if any) of the Corona without having the observer's attention distracted by the chromosphere. A Savart's polariscope is recommended by preference. The Nicol's prism of the polariscope should be set beforehand with its principal plane (or plane of symmetry) radial, *i e.* perpendicular to the sun's limb, and the observer must note whether bands are visible, and if so, whether they are black-centred or white-centred. Should the bands be feeble, it will be well to rotate the polariscope, prism and plates of course moving together, and quickly restore it to its primitive azimuth, after having noted the estimated azimuth of the Nicol when the bands are strongest and black-centred. Should no bands, or only dilute bands be seen, it may be that the Corona, though polarised, is overpowered by other light; and the observer will move the telescope from the sun, radially if it may be, if in any other direction rotating the polariscope so as to render its principal plane radial in the new position. He will then note whether, though the light becomes feebler, the bands become less dilute. Should, on the other hand, the bands be strong, the observer, after having satisfied himself as to the plane of polarisation, will endeavour to make out whether by means of the polarisation of its light the Corona can be detected superposed on the chromosphere. He will, therefore, move the telescope *towards* the sun, keeping the principal plane of the polariscope radial, and note how near the sun the bands can still be seen. To provide for the contingency of this observation, it will be well to point in the first instance to the side of the sun which will be first uncovered. If time permit he should try whether there is any sensible quantity of polarised light on the dark disk of the moon, rotating the analyser and determinating the plane of polarisation.

Second Observation.—The special object of this observation is to differentiate, if possible, between the Corona, on the one hand, and the Chromosphere, or whatever else may be self-luminous (be it even a portion of the Corona itself), on the other. This will be possible if the light of the Corona be strongly polarised, so as to admit of comparative extinction by an analyser. The observer should turn the analyser so as to extinguish, as far as may be, the light of the Corona in the neighbourhood of a radius depending on the angular position of the analyser. He should notice the form, colour, and general appearance of any residual luminosity other than the well-known protuberances; should contrast the appearance, especially as to colour, with that seen when the light is analysed so as to retain light from the same region polarised in the perpendicular direction, and should ask himself whether the luminosity is such as could be accounted for by the superior brightness near the sun of the unanalysed light, even though it were to suffer the same proportionate loss by analysis as the Corona at a greater distance. Of the instruments named the Nicol's prism is better adapted for a general survey, not requiring any limitation of field; the double-image prism is better adapted for a comparison of the oppositely polarised images, since the observer can compare them *directly*, not one with *his memory* of the other. The double-image prism will require a diaphragm with a long and moderately broad aperture in the focus of the eye-piece, rotating with the prism, and in the observation the length should be placed in a radial direction.

Third Observation.—The observation of the streamers as to polarisation might throw much light on their nature, and the observer who undertakes this observation, by means of a Savart's polariscope, or else of a Nicol's prism capped with a plate of calc spar or arragonite, will be in a condition to determine with advantage the plan of polarisation, if there be polarisation, of the Corona generally. But the streamers must be his first object.

Fourth Observation.—It has been supposed by some that the polarisation observed in the Corona was really due to the *secondary* illumination of the intervening portion of the Earth's atmosphere, in other words, to the illumination produced by reflection from clouds, &c., towards the horizon. This effect may be eliminated by using a Savart's polariscope, or, better, a polariscope with quartz wedges, and turning the instrument till the bands (if any) seen on the moon's disc disappear. The corona can then be scrutinised as to polarisation, and the polarisation examined in different azimuths of the Nicol's prism relative to the radius drawn from the sun's centre, by pointing the telescope instead of rotating the analyser. In this observation the observer has the choice of two rectangular azimuths of the polariscope, for each of which the bands (if any) on the moon disappear, and if no bands be seen on the moon he is free to scrutinise the polarisation of the Corona, by turning the polariscope.

General Remarks.—The object-glasses of all telescopes intended to be used in polariscopic observations should be examined before departure as to their freedom from defects of annealing. All polariscopes including a Nicol's prism, or tourmaline, should be marked, so that the principal plane may be readily known *by feeling*, as sight-marks might fail for want of light. Double-image prisms should have one side of the aperture in the diaphragm marked so as to distinguish the two images.

Instructions for the Spectroscopic Observation of the Chromosphere.

Note: The objects to be obtained are:—1. To determine the actual height of the chromosphere as seen with an eclipsed sun; that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted. 2. To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences. To do this the band of the spectrum just above the stratum which gives the hydrogen lines before totality and during totality, should be carefully examined, to notice (*a*) if any traces of the hydrogen spectrum exist above the region which before totality gave the hydrogen lines, and (*b*) what lines extend outside the hydrogen spectrum and whether they also exist with it in the lower strata. 3. To test the American observations of last year as to the existence of a line at 1474 in the corona spectrum, by seeing if it be visible above the region which gives the hydrogen spectrum. 4 To determine whether any other gases or vapours are ordinarily mixed up with hydrogen, but remain invisible with the uneclipsed sun in consequence of the absence of saliently brilliant lines in their spectra. The observations should be conducted as follows:—1. Work with a horizontal slit, or a slit in a parallel of declination, according to the instrument used, whether altazimuth or equatoreal. 2. See that the spectroscope telescope works easily, so that sweeping along the spectrum is easy. 3. Find before totality an average plain-topped region of the chromosphere, where some motion on either side does not brighten, or thicken, or lengthen the lines near the part covered at the commencement of totality. 4. Observe this before and during part of totality, the telescope being driven by the clock if an equatoreal is used. 5. Just before totality sweep from red to violet; note the lines and their lengths; mark the 1474 line and the lines between D and E carefully. 6. Immediately after the commencement of totality sweep back; note new lines—their heights; especially the comparative heights of C, near D, F, and near G, with these former heights; and especially whether band over *b*, and the thickness of F. Note also the lines between D and 1474. 7. Just before the end of totality unclamp and bring back the slit to the *following* limb of the Moon; and note the extinction of the spectrum by the reappearance of the Sun, if possible by a rapid sweep; if this is not possible, then watch the behaviour of F; sweep back again to see if there be any variations from (5) in the new region now observed. 8. Carefully note position angle where slit cuts limb. 9. Record the impressions of facts, and facts not already noted, as soon as possible.

Instructions for the Spectroscopic Observation of the Corona

Note.—*The word Corona is here used for convenience, to include all the light above the prominences. It therefore includes that part of the chromosphere which is generally veiled to us when observations are made by the Janssen-Lockyer method.*

The principal object to be obtained is:—To determine whether it is possible to differentiate the outer layers of irregular outline and streamers from a stratum say some 5′ or 6′ high round the sun, which may possibly be the limit of the gaseous envelopes above the photosphere. To attack this question, we require a long slit, a large aperture, and long collimator, and small dispersive power. The slit must be adjusted for a faint cloud before totality, and on no account is it to be touched before observations of a similar cloud can be made after totality, by the heads of the party. The most important observation to make is, whether there are any dark lines in the spectrum at any distance from the sun; and if so, at what distance? Next, whether there are any bright lines; if any, their positions must be noted, especially if the lines recorded by the American observers are again visible. The observations should be conducted as follows:—1. Arrange the instrument so that the image of the following limb of the moon, at the point of its first contact, will fall on the left-hand side of the slit, placed nearly horizontally. 2. See if Corona is visible before totality, and note its spectrum with utmost care, moving the slit in azimuth, so that perhaps, at the instant during totality, while possibly with a long slit, the spectrum of the sun or prominences on the preceding limb is visible in the same field of view.

Some Particulars to be Especially Noticed by those Observers who make Drawings of the Corona

1. Its extent, and the boundary-line *if any;* if no definite boundary, this *should be stated.* 2. Whether

there is any change, all changes must be most carefully shown in any manner the artist may prefer. 3. Especially note all long streamers. 4. All tints and change of tint, and whether the colour is distributed in patches or in layers concentric with the moon, or in connection with the prominences. 5. Whether it consists of a level patch of luminous haze or radiating beams of light, or of bundles of hyperbolic rays. 6. If of radiating or hyperbolic beams, whether they are evenly distributed all round, or in groups only. 7. Whether the dark intervals between such radiating beams are constant or fluctuating. 8. Whether it is concentric with the moon. 9. Whether it is equally intense all round the moon. 10. Whether the outer border exhibits any coruscations, or whether its definition is permanent and equally pronounced all round. 11. Whether the light of the Corona is more intense or less so in the immediate neighbourhood of the prominences. 12. How much darker the moon's disc is than the sky.

ENERGY, AND PROF. BAIN'S LOGIC

[EXTRACT FROM PROF. TAIT'S OPENING ADDRESS TO THE UNIVERSITY OF EDINBURGH, NOV. 2, 1870]

THE so-called *Laws of Motion* first explicitly stated, as we now employ them, by Newton in the Principia, are partly due to Galileo, partly to his immediate successors. Like all great physical discoveries, they were more or less clearly seen by many philosophers about the time in which Newton threw them into the simple, and yet comprehensive, form in which we now use them. As ordinarily understood, they embrace the results of observation and experiment as to the action of force on matter. The first tells us how matter behaves when not acted on by force, and therefore shows us how to *detect* the action of a force. The second tells us how to measure the force by its effects, and how to calculate the action of a force or forces acting on a mere *particle* of matter. The third, as directly interpreted, shows how to apply the other two to the motion of masses or of groups of particles. With these alone we have the foundation of an enormous portion of the science of Dynamics, and we require merely a sufficiently powerful mathematical process to enable us to develop to their utmost the calculations necessary for the determination of equilibrium or motion of any set of masses whatever, so long as the motion is visible, or capable of being rendered visible by a microscope.

But we require something more before we can extend mathematical calculations—which, be it ever remembered, are necessary in physics solely on account of the imperfections of our intellect; merely saving us an intolerable amount of thought which would otherwise be wasted on petty details—something more, I say, is required before we can apply our mathematics to Heat, Electricity, Chemical Action, &c., &c.

Curiously enough, that something was foreseen and provided for by the keen intellect of Newton. He gave it in the form of a *second* mode of interpreting his third law, quite distinct from the ordinary one, which is the well-known assertion that "Action and Reaction are equal and opposite." Instead of using the terms Action and Reaction in the sense of mere pressures or tensions, he shows that the law will equally hold if they stand for what are now called rates of *spending* or of *receiving* energy; or, in more familiar language, rates of doing work. So that whenever there is transference of energy from one body to another, the one gains *exactly* as much as the other loses. This is at present the grandest physical law known. That we may understand it better, let us take first a simple physical fact, but one of a somewhat analogous nature. It is a comparatively recent discovery that *matter is indestructible*, yet so important that without it we may be certain that chemistry could never have become a science. If a chemist were not assured by experiment that no quantity of matter, however small, is ever put out of existence, submit it to what ordeals he may, what confidence could he have in the results of an analysis? Or again, where would his science be if new matter could suddenly make its appearance? The balance is his most important instrument, but without the confidence (derived from experiment) that matter cannot change in quantity, its indications would be of no value to him.

So it is, but in a more extended sphere, with the Natural Philosopher, and it is a source of legitimate pride to us, that as Newton first hinted at this grand modern generalisation, and first gave the mathematical method naturally fitted for its development, so it is to this country again, and mainly to Dr. Joule of Manchester, that we owe the proof (which must, of course, be *experimental* to be valid) that energy is, like matter, *indestructible*. It is, therefore, in the usual sense of the word, as REAL as matter. In fact, the physical phenomena of the Universe (excluding in the meantime, on account of our utter ignorance, some of those connected with life) depend upon matter and energy alone. Different combinations of matter constitute the subject of our chemistry; different groupings of molecules as well as of masses, and different distribution of Energy, form the rest of our Natural Philosophy. Hence the overwhelming importance of this real *something*, Energy, in the whole of Physical Science.

I shall devote the rest of my time this morning to very elementary notions connected with energy and this grand law of Nature. But before I do so I have a few words to say about another work in which the principles of Natural Philosophy are discussed; a book infinitely more likely than that of Hegel (whose absurdities I have already pointed out to you) to fall into your hands. It is now not a dreamy and dogmatic German, evolving everything from himself, and railing at physical facts as well as at exquisite methods in mathematics, with whom we have to do—it is on the contrary, a hard-headed Scotsman, and a Professor in one of our Universities. We have here no evolution from consciousness to laugh at, no sneering at experimental science; we have to guard against dangerous misconceptions of the truths discovered by physicists; mistakes all the more dangerous that they are honestly held, and that they have been assigned a prominent place in a text-book which many of you may have at some time to read; and especially because, as students, you are peculiarly liable to be led away by *ex cathedrâ* statements. For obvious reasons I cannot take many examples now; in the more abstruse, the statement itself, and the exposition of its error, would be alike unintelligible to you; in the simpler ones you may be trusted to see the error for yourselves.

The first I quote is from what is called the Logic of Physics, and is, to a certain extent, personal. "*Volume* and *mass* rightly precede *density* in order of definition. Messrs. Thomson and Tait make *density* precede *mass*." And we do so, we think, very logically, because density is a *specific* property of matter, unalterable in general, except to a very small extent, by physical processes, while volume and mass are absolutely indefinite, depending as they do upon the quantity of matter spoken of.

Again, "In the transfer of force, *nothing is lost*. The mechanical momentum transmuted into heat is fully accounted for in the heat produced: by proper arrangements it could all be gained back." The last nine words, however they may be interpreted, are essentially false: in fact they contain an explicit denial of the second law of thermodynamics upon which Sir W. Thomson based his grand law of Dissipation of Energy, one of the most important of his many splendid contributions to physics, and one having the most direct bearing upon the future of the physical universe. The rest of the statement, as it stands, is also false. It may be made correct by writing one of the words, *work*, *power*, *potency*, or preferably *energy*, in place of *force*, and also in place of *momentum*. What would be thought of a man who should say—"I paid six weeks for it," meaning "pounds" by "weeks." For "momentum" cannot be transformed or "transmuted" at all, it remains for ever unchanged.

Again, when bodies impinge on one another, "the rise of temperature is exactly proportioned to the visible momentum destroyed." Let us put the correct word "energy" in the place of momentum, and we find that this asserts the startling physical fact that the specific heat of every body is the same at all temperatures. If we take the statement as it stands without correction, it is simply nonsense.

Again, "the foot pound, meaning the force expended in raising one pound weight one foot, which is the same as a (*sic*) momentum of one pound moving at eight feet per second."

Raising one pound weight one foot is a feat which, by proper combinations of machinery, may be effected by any given force whatever, be it the weight of a grain or of a million tons. But a "foot pound," and the "momentum of one pound moving at eight feet per second" cannot possibly be compared with each other, any more than a cubic yard can be expressed as a number of square yards, or the height of a mountain in acres, roods, and poles.

You will see that the error in the examples I have just chosen (excepting of course the fatal one about restoration of energy) is in great part due to the misuse of words. Yet it is from a treatise on Logic that I have quoted!

The essence of the lesson taught by all this is simply the conviction that scientific knowledge has reached such an immense development that no one man can now possibly master thoroughly more than one or two of its many branches. There can be no "Admirable Crichtons" in our days. The greatest logician the world has produced, or is likely to produce, for many a long day, the lamented George Boole, more than once expressed his regret that a systematic logical treatment of human knowledge, even in moderate compass, and going little farther than the elements in each branch, had become absolutely impossible as the work of one man—impossible, that is, for a man who revolted at the idea of publishing anything he knew to be defective.

MOUNTAIN CLIMBING

IN the number of NATURE for June 23, 1870, I described an ascent of Mount Etna which I made on March 4 of this year, with an excellent guide, Pietro Cravagna.

I now propose to make some remarks on a few points of interest with regard to mountain climbing. One of the most important of these is the alleged lowering of the internal temperature of the body under such conditions.

During two ascents of Mont Blanc made on the 17th and 26th of August, 1863, by Dr. Lortet, of Lyons, and Dr. Marcet, of London, and described by Dr. Lortet in the *Lyon Médical* of September 26, 1869, experiments made apparently with great precaution on Dr. Lortet himself with a registering maximum thermometer (of Walferdin), by which (between + 30° C. and + 40° C.) hundredths of a degree could be appreciated, showed that the internal temperature of the body under such conditions is lowered to a very remarkable extent.

I will quote Dr. Lortet's own words: "A jeun et exactement dans les mêmes conditions, *pendant la marche*, la décroissance de la température intérieure du corps est très-remarquable, *elle est à peu près proportionelle* à l'altitude à laquelle on se trouve."

In effect, from the table given in the paper referred to, I find that during the first ascents the internal temperature descended *gradually* from 36·3° C. (that *during exercise* at Chamounix, 1,050 metres above the sea) to 32° C. at the summit of Mont Blanc (4,810 metres above the sea); while during the second ascent the difference was from 35·3° C. to 31·8° C.

Dr. Lortet found that as soon as he stopped for a few minutes, the temperature of his body rose briskly to the normal standard, except on the summit itself, where "il a fallu près d'une demi-heure pour que le thermomètre atteignît sa hauteur habituelle."

During digestion, in spite of the exercise being taken, the temperature remains normal, or even rises; but this does not last long: "Une heure à peine après avoir mangé, le corps se refroidit de nouveau par les efforts." The descent of the temperature of the body under such conditions, then, amounts sometimes to more than 4° C.; and if we take the difference between the normal temperature of the body *at rest*, and that observed by these experimenters on the top of Mont Blanc, the difference amounts in one case to 5° C., "abaîssement énorme pour les mammifères dont la température était reputé constante!" as Dr. Lortet justly exclaims.

Now Mount Etna is particularly suitable for such experiments; one begins to walk either at Nicolosi, or preferably at the Casa del Bosco, and one has nothing to do but to go straight up; there is nothing in the way, it is simply a long "grind" of some five or six hours or more, according to the state of the snow. A series of misfortunes with my maximum thermometers prevented my repeating the above-described observations, and I have referred to them at such length in the hope that some one may be induced to take the excellent opportunity afforded by the expedition to Sicily of deciding so important a point.

The state of the circulation is hardly less important than that of the internal temperature.

Dr. Lortet found that the pulse increased in frequency from 64 per minute at Chamounix to 172 on the summit of Mont Blanc, and he was further enabled, by means of the sphygmograph, to make some observations as to the *state* of the pulse at various altitudes. In ascending Etna I made some comparative observations on the frequency of my guide's pulse as compared with my own, which show some points of interest.

At the Casa del Bosco my pulse was 68, my guide's 74; we had both *ridden* to that point, and the difference is probably an illustration of the established fact that the circulation of persons living in mountainous districts is quicker than that of those living in plains. On arriving at the summit of Etna after a ride of an hour and a quarter (from the Casa del Bosco), a rest of fifteen minutes, and a stiff walk of three and a quarter hours over dry, hard, snow (an exceptionally easy ascent),* my pulse was very irregular, and about 114 or 115 to the minute, while the guide's was only 89—that is to say, that mine had increased 46 beats in frequency, his only 15; or mine was about ⅔rds, his only about ⅕th, as fast again; showing the slight effect of such ascents on those who are used to them, and who live habitually in mountainous countries.

But this was still more forcibly illustrated by the state of our pulses after a very quick descent, a regular trot all the way (we had stayed two hours at the summit, and eaten a hearty breakfast); at the place where we left the mules, I found that while my pulse, after a minute or two's rest, was at 106 or 107, the guide's was at 99 or 100; mine being 8 beats *less* than it was on arriving at the top, his 10 beats *more*; his circulation was *less* disturbed by

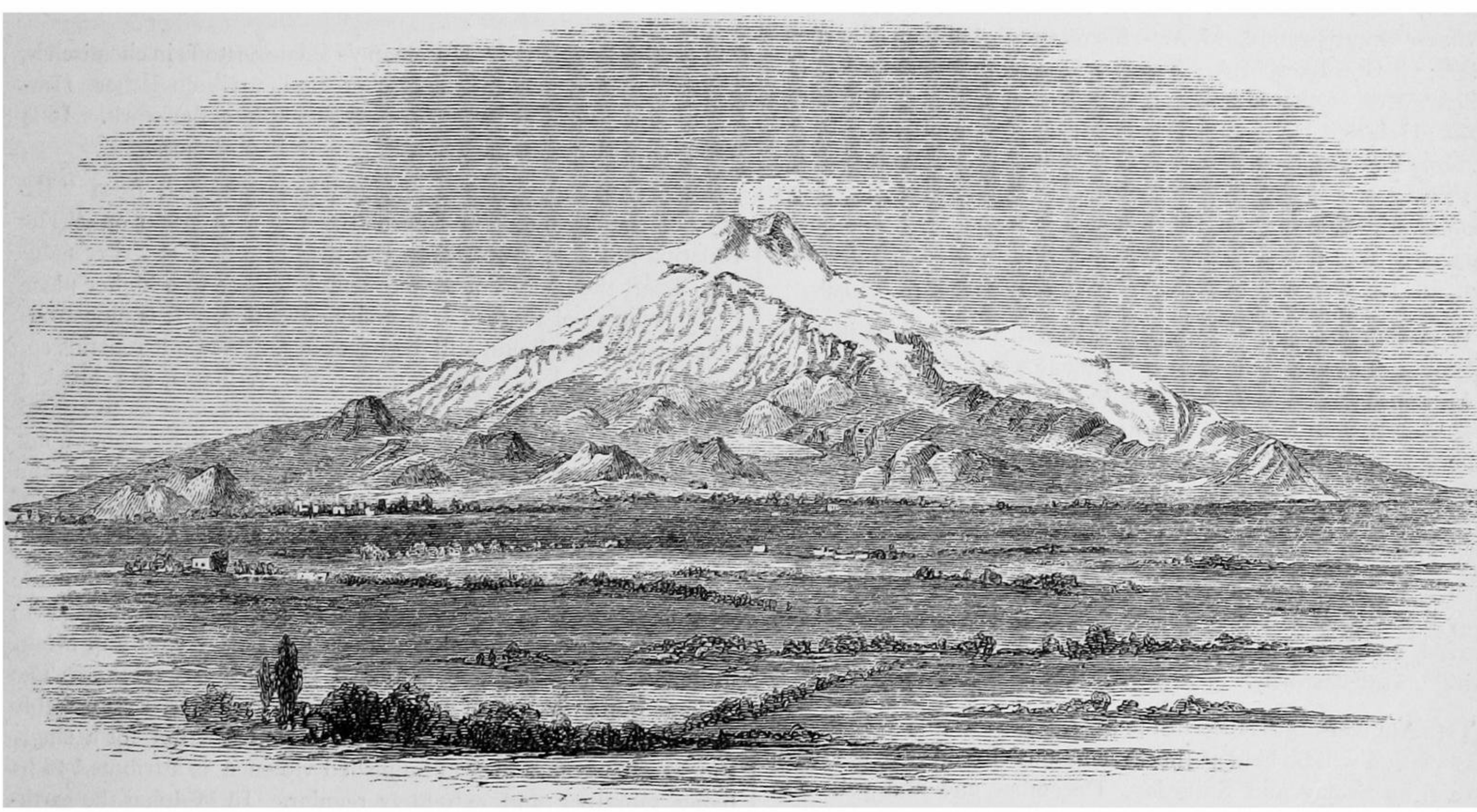
* Later on in the month of March, when much snow had recently fallen the Rev. A. G. Girdlestone and I, with two other friends, made an ascent: it took us nearly eight hours to walk from the Casa del Bosco to the Casa degli Inglesi, and we saw nothing but a very heavy snow-storm.

walking fast up the mountain, carrying a load of provisions, than by running down it with nothing to carry; mine, on the contrary, as one would expect, much more so by walking up than by running down. (Of course I could get nothing in the shape of sphygmograph at Catania.)

Dr. Lortet has recorded some interesting observations, made with the aid of the anapnograph of MM. Bergeon and Kastus, on the state of the respiration. It is much quickened, as is well known; the expiration is much prolonged, its amplitude much lessened, the inspiration shortened and quickened; much less air being inspired and expired than is normally the case.

These effects are partly due to the rarefaction of the air, less oxygen by weight being taken in at each inspiration, and partly to the excessive muscular exertion, which demands a corresponding increase in the animal heat, and so a corresponding increase in the amount of oxidation taking place in the blood; this not being always obtainable and the exercise being continued all the same, the normal temperature of the body cannot be maintained, and so it falls, and one becomes miserably cold while walking, and has to stop to get warm again.

Now as to the amount of work done; that is very much greater than is commonly supposed: leaving out of consideration the difficulties encountered in walking either over soft snow or over slippery places, especially when very steep, I find that in climbing Etna, starting as I did, on the occasion that I have already described, from some distance above the Casa del Bosco, I lifted about 150lb. to the vertical height of at least 5,700 ft., or about 380 tons one foot; adding 10 foot-tons (little enough) for the horizontal distance traversed, we have 390 foot-tons as representing work done in 3¼ hours: now 390 foot-tons is about the work done by a person of the weight above mentioned in walking 22 miles on level ground; that is to say that, without making any allowance for the increased difficulty of breathing due to the rarefaction of the air or for any of the consequences of this (increased action of heart, &c.), one has walked 22 miles in the time ordinarily taken to walk 13.



A VIEW OF ETNA

To put it in another way: 390 foot-tons is a hard day's work, as it is found that something over 300 foot-tons is the average day's work of strong labourers. One therefore does a hard day's work in 3¼ hours, and this after an ordinary day's work, *plus* a fatiguing ride of some four hours on a mule, over lava currents and cinders, in the middle of the night and without any chance of sleep.

Taking everything into consideration, it is difficult to believe that the fatigue is, as it is often stated, out of all proportion to the work done; we must not only consider the amount of work, but *the time in which it is done*, and this is what I have especially wished to point out, as one can easily understand that the fatigue must increase very fast as the time in which the work is done decreases.

At the summit of Etna (the accompanying woodcut, from a rough sketch taken when a good deal of the snow had melted, can give but a feeble idea of the exquisite effect produced by the dazzling whiteness of the snow against the perfectly clear blue Sicilian sky) the range of temperature within a few feet of vertical distance is very remarkable. Just after sunrise I found that the temperature of the air at the height of four feet or so above the ground was $-2°$ C.; on the ashes where the snow had melted it was $+9°$ C.; just under the surface it was 20° C., and a few inches under it was higher than 36° C. (my minimum thermometer, the only one I had left, not allowing me to register a higher temperature than this).

This high temperature of the ashes only occurs where they are mixed with sulphur, which is continually undergoing oxidation; the other parts of the cone and crater are in the winter covered with snow, and it is very strange to see snow a foot deep or more, quite close to ashes that really feel uncomfortably hot.

Those who wish to "get up" the history of Mount Etna and its structure, should refer to Sir Charles Lyell's "Principles of Geology," tenth edition, vol. ii.; or for a much more detailed account of the eruptions, its present and past conditions, &c., to "La Vulcanologia dell' Etna," by Carlo Gemmellaro, published at Catania in 1858.

W. H. CORFIELD

NOTES

As will be seen from another column, the Eclipse Expedition is now fully organised, and all the parties will be on their way before our next number appears. The Organising Committee, who have done about three months' work in a fortnight, deserve all praise for their untiring efforts, and we may hope that they will be adequately rewarded by the results obtained. We may mention that on the representation of the Organising Committee, the Government have communicated with the French and German Governments with a view of securing the services of M. Janssen, who, if he can be got out of Paris, will accompany Mr. Lockyer to Sicily.

It is stated that Prof. H. J. S. Smith, F.R.S., has been appointed to succeed the late Dr. W. A. Miller as a member of the Royal Commission on Scientific Instruction and the Advancement of Science.

The annual election of the Council and officers of the Royal Society took place yesterday, when the following gentlemen were elected, viz.:—President: Gen. E. Sabine, K.C.B. Treasurer: William Spottiswoode, M.A. Secretaries: William Sharpey, M.D.; G. G. Stokes, M.A. Foreign Secretery: Professor W. H. Miller, M.A. Other Members of the Council: George Burrows, M.D.; Heinrich Debus, Ph.D.; P. M. Duncan, M.B.; Sir P. de M. Grey Egerton, Bart.; Prof. G. C. Foster, B.A.; Francis Galton; J. P. Gassiot; J. D. Hooker, C.B, M.D.; William Huggins; Prof. G. M. Humphry, M.D.; J. Gwyn Jeffreys; Sir J. Lubbock, Bart.; C. W. Siemens; Prof. A. J. S. Smith, M.A.; Prof. John Tyndall, LL.D.; and Prof. A. W. Williamson, M.D.

It is with very great regret that we have to announce the disablement of two of our most prominent scientific men. Sir R. I. Murchison has been stricken with an attack of paralysis, and Professor Balfour Stewart was among the sufferers by the collision on the London and North-Western Railway on Saturday evening last. In the latter case, though the injuries are severe, one thigh broken, and a great shock to the nervous system, we may hope that Professor Stewart will shortly be restored to his friends and to science. At the moment of going to press, we hear that Sir R. Murchison's state is considered somewhat improved, and that Sir W. Ferguson reports Prof. Stewart to be going on satisfactorily.

The Anniversary Session of the St. Andrews Medical Graduates Association will be held at the Freemasons' Tavern, Great Queen Street, on Friday and Saturday, December 2nd and 3rd. On Friday, at 7.30 P.M., Dr. Whitmore will read a paper "On the results of Sanitary Legislation on the Health of the Metropolis and our present urgent Sanitary requirements." On Saturday, at 5 P.M., the President, Dr. Richardson, F.R.S., will deliver the Anniversary Address "For the Future of Physic."

At the first ordinary meeting for the session of the Society of Arts, held on the 16th ult., the following silver medals were awarded:—To Mr. Thomas Dickins for his paper "On Silk Supply;" to Mr. James Collins for his paper "On Indiarubber, its History, Commerce, and Supply;" and to Mr. William Bridge Adams for his paper "On Tramways for Streets and Roads and their sequences;" and the Prince Consort's Prize of twenty-five guineas to Mr. Edward Turner Sim, who, at the Society's examination, had obtained during the last four years the largest number of first-class certificates.

Counts H. Wilcjek and G. Wurmbrand have been engaged, at the instance of the Viennese Anthropological Society, in an examination of the Austrian lakes, and have found remains of pile-dwellings in the Attersee. One of them belongs to the Stone period.

At the remote city of Indianapolis an Academy of Sciences has just been formed under the presidency of Professor E. T. Cox. The exclusive object of the association is the cultivation and imparting of knowledge of the natural and physical sciences. Though the number of members at present does not appear to be large, it will, no doubt, like most other things in the Far West, increase rapidly. We wish all success and a prosperous future to this the youngest society for the advancement of natural history.

The recently published volume of Bentham's "Flora Australiensis" (vol. v.) includes the natural orders from Myoporineæ to Proteaceæ.

Messrs. Macmillan will shortly publish, as one of their series of school class-books, Lessons in Elementary Physics, by Prof. Balfour Stewart.

We have just received the first part of the second edition of Schellen's Spectral-analyse. The preface states that it has been carefully revised, and considerable additions made, in accordance with the progress of science since the publication of the first edition, especially in the application of spectrum-analysis to the sun. It is copiously and beautifully illustrated.

The third section of "Husemann's Pflanzenstoffe in chemischer, physiologischer, pharmakologischer, und toxikologischer Hinsicht," does not, as was expected, complete the work. It is occupied with vegetable acids and neutral substances.

Mr. Matthew Williams, jun., has reprinted, from "Essays of the Birmingham Speculative Club," a paper on "The Relation of the Universities to Practical Life." The writer's aim is to assert the claims of experimental science to a higher place in the scheme of English University education. It is monstrous that such a plea should still be necessary. While it remains so, however, we welcome all who, like Mr. Williams, can break a lance in the cause of true progress in such a manner as to appeal even to the most "practical" sympathies. Why should not others in the great centres of English industry take up the subject? Agitation works wonders in the political world; who knows what it might not in time effect for science?

On the 7th of August the most severe shock of earthquake since April 1868, has been felt in the Hawaian Archipelago. The time was $4^h\ 10^m$ A.M., and the shock was felt generally in Maui, Molokai, and Hawaii, but not so much in Honolulu. The damage done was not so great, but it was considered remarkable that the shock lasted ten seconds—a long period for a single shock. The cause of its diminished violence is attributed to its being vibratory and extremely regular. In Molokai the earthquake was accompanied by a tremendous roar, and appeared to have a circular motion, so that no precise direction could be noticed. It is stated that the fissures on the Kau side of Mauna-loa are constantly steaming, and that smoke is issuing from the crater of Makeaweoweo in great volume.

H.M.S. *Zealous*, bearing the broad pennant of Admiral Farquhar, having visited St. Charles, one of the Galapagos Islands, has found sufficient objects of interest to induce the Admiral to make another trip there in October, to examine the natural history of the place.

On Thursday, the 29th September, two violent shocks of earthquake occurred at Lima, in Peru, about 10 P.M., causing much alarm.

A passenger reports to the *Rangoon Times* a waterspout seen on the Irawaddy River on the 31st of August in the Chwaygeen Creek. He saw a dense mass of clouds, and then a whitish cloud, which resolved itself into a large waterspout. It soon partially dispersed, but formed again, and remained about ten minutes, sometimes straight, at others curved. The convex side was to the wind. The spout appeared to be rapidly skimming the surface of the river, the water rising like spray round it, and having a spiral appearance along the shaft. The phenomenon was not accompanied by noise or thunder and lightning.

WE have received the reports of the Mining Surveyors and Registrars, Victoria, for the quarter ending 30th June, 1870. In some instances steady improvement is reported, but there are also numerous complaints of a considerable falling off in the yield of gold. The total number of miners employed in the colony during the quarter was 60,267. Of these, 28,227 Europeans and 15,478 Chinese were employed as alluvial miners, 16,500 Europeans and 62 Chinese as quartz miners. In alluvial mining 399 steam-engines of 9,657 aggregate horse-power were used in winding, pumping, &c.; and in quartz mining 701 steam-engines, of 13,283 aggregate horse-power. The approximate value of the mining plant was 2,144,727*l*. 1,021½ square miles of auriferous ground were actually worked upon, and 2,782 distinct quartz reefs were actually proved to be auriferous. The Mining Surveyors and Registrars are unable to obtain perfectly full information respecting the quantities of quartz, &c., crushed or operated upon; but the following is a summary of the leading facts which they detail:—The total quantity of quartz crushed was 223,285 tons 14 cwt. 36,909 tons 7 cwt. of quartz tailings, cement, and mullock were crushed, and 868 tons 15 cwt. 2 qrs. of pyrites and blanketings were operated on. The average yield of gold per ton from quartz was 9 dwt. 11·38 gr.; from quartz tailings, cement, and mullock, 3 dwt. 0.39 gr.; from pyrites and blanketings, 2 oz. 6 dwt. 13·37 gr. The total yield of gold from quartz was 105,775 oz. 18 dwt. 19 gr.; from quartz tailings, cement, and mullock, 5,566 oz. 9 dwt. 1 gr.; from pyrites and blanketings, 2,022 oz. 7 dwt. 18 gr. Appended to the reports is a careful geological map of portion of the Durham Lead, with a paper on the subject by Mr. Reginald A. F. Murray. Mr. Murray describes the physical and geological aspects of the district traversed by the Durham Lead, discusses the question whether it is or is not the main outlet of the Golden Point Gutter, and gives a brief account of the claims lying within the portion of the lead immediately under consideration.

IF we may judge from its last annual report, the proceedings of the Bombay Geographical Society are not much enlivened by the efforts of its members. During the entire session not a single member had favoured the Society with any original communication. For some time the Society has been considering the propriety of getting compiled an Anglo-Vernacular Index of Indian Geographical names. The scheme would supply a want that has long been felt, but it does not appear to have received much encouragement from the authorities, to whom an appeal for aid in the matter was addressed. The "Transactions" of the Society for the year ending December 1869, are made up of some interesting notes on Annesley Bay, by Mr. Edwin Dawes, and a brief paper by Mr. J. U. Yajnik, on the Hot Springs of Lasúndrá, in the Kairá Zillá.

ANOTHER proof of the desirability of earnest experiments in the widespread cultivation of economic plants is shown in the success which has attended the introduction of tobacco in some of our own colonies. Samples of Latakia tobacco grown in Jamaica have been submitted for approval in London, and reported upon favourably; and from India we hear that the seeds of the best varieties are being distributed in the districts most suited to the cultivation of the plants. From Natal, a sample has just been received, and submitted to an eminent firm of tobacco brokers in the City, who report that it is a very near approach to what colonial tobacco should be. It is of good substance, and of a fair light brown colour, and would, if carefully packed, in all probability fetch a price of from 5*d*. to 5½*d*. per lb. in bond, and would meet with a ready sale to a rather large extent in the London market. The principal requirements of a good tobacco are brightness of colour and dryness of condition, the latter being most essential in consequence of the high rate of duty. Green and imperfect leaves should be excluded, and great care should be taken in packing, so as not to injure or crush the leaves. An important point in the curing of tobacco is to ensure its burning well, and holding fire. A good opportunity presents itself at the present time for the introduction into our markets of colonial tobacco, owing to the general scarcity both of the continental supplies and of other kinds usually substituted for the American product in the manufacture of cut tobaccos.

AS an instance of the rapidity with which introduced plants spread, when soil and climate are congenial to their habits, we may point to the *Euphorbia prostrata*, Ait, a little annual weed in Jamaica and Trinidad, which became introduced by chance about ten years since into a garden in Madeira, situated some 400 feet above the sea; from this spot it has rapidly spread down the steep road to the town; while up the other hills, separated by deep ravines from that down which it came, it has scarcely crawled at all, a downward course apparently being far easier for it than an upward one. It has, however, slowly crept up another hill at the rate of about ten feet a year. The seeds, which are described as ecarunculate, with sharply tetragonal palæ, transversely keeled, are well adapted for sticking to the clothes of travellers, and to be carried about, so that we might well expect the plant to crop up in all directions. Mr. Lowe says that it is now to be found everywhere in Funchal below 500 feet.

THE pods of the Ground Nut (*Arachis hypogæa*) are frequently to be seen in the windows of small shops in the poor neighbourhoods of our large towns, where they are chiefly purchased by children, and are known to them as "monkey nuts." Their chief use, however, is for the expression from the seeds of a light coloured bland oil, said to be extensively used for mixing with olive oil; and we have even heard of the seeds being parched and used as a substitute for coffee, but we now read that in America they are used for making chocolate (so called); for this purpose they are beaten up in a mortar and the mass compressed into cakes, and it is said to form a most agreeable chocolate without a particle of true cocoa. More than this, the Americans prepare the seeds as a dessert sweetmeat by parching them and beating them up with sugar.

THE disasters of a ship have made the Peruvians acquainted with the situation of a new guano island in the South seas. This is called "Baker's Island," in 12° N. lat. and 176° E. long. It has some smaller islands near, and is surrounded by coral reefs, on which thirty wrecks have been counted. The island has been taken possession of by a North American company, and is peopled by three Americans, of whom one is the governor, and about a hundred Kanakas. The cargo of the English barque *Borneo*, bound to London, consisted of a yellowish earth, which the Peruvians say has no smell of ammonia, but may sell at a profit to mix with Peruvian guano.

THE *Boston Journal of Chemistry* says that large factories have been established in New Orleans, Buffalo, and Brooklyn, or making grape sugar from corn. The latter is steeped in weak soda lye, for the purpose of softening the husk and gluten; and is then ground wet, and run through revolving sieves to separate impurities. It is afterwards made to flow through ways or troughs, in which the starch gradually settles as a white powder. The wash water is run into a large cistern, and allowed to ferment and produce a weak vinegar. The starch from the troughs is put wet into the mash-tub, and treated with water containing one per cent. of sulphuric acid, for eight hours. The acid is neutralised with chalk or carbonate of lime, and the liquid evaporated to get rid of the gypsum, and afterwards further evaporated in vacuum pans, and run into barrels ready for crystallisation.

MATERIAL changes are officially reported from the Bay of Talcahuano, in Chile, which was surveyed by Captain Fitzroy. The commander of the Chilean war steamer *Ancud* now reports that the water throughout the entire extent of the bay has increased from two to two-and-a-half fathoms. This represents great geological change. The rock Fraile, in the Gulf of Arauco, represented on Fitzroy's chart as a sunken rock, is now, however, a small island united to the mainland, opposite to the western bank of the river Tubul, and has rather the shape of a quadrangular pyramid.

THE *Times of India* states that the coal-beds discovered in Bellary are excellent in quality and abundant in quantity. Great anxiety is felt for Dr. Oldham, who said he would eat all the coal found in the Madras Presidency, for the doctor is a man of honour.

FROM a considerable number of observations on the temperatures of the two sides of the body, Mr. Blake draws the following conclusions, which are recorded in the *Medical Times and Gazette* of October 8, 1870:—1. That the temperature of the sides of the trunk under usual circumstances, *i.e.* in health and at rest in a temperate climate, is equal. 2. That under certain conditions the temperature of the left side of the trunk may exceed that of the right. 3. That that excess during exertion in a cool atmosphere averages half a degree F. 4. That that excess reaches its maximum of about one degree F. during exertion under a powerful sun.

THE GEOGRAPHICAL DISTRIBUTION OF DEER*

IN his excellent "Geographische Mittheilungen," Dr. Petermann has lately given us several *zoo-geographical* articles, as we may call them – such as those of Dr. Finsch on the distribution of Parrots, and of Freiherr von Heuglin on the Bird-fauna of North-eastern Africa. Both of these memoirs are the products of the highest authorities on the subjects to which they respectively relate, and deserve our warmest commendation. We cannot, however, say so much as to the merit of the paper upon the Geographical Distribution of Deer, which appears in a recent number of Dr. Petermann's journal. The authors of this memoir, which, if properly treated, is on a subject of very great interest, have, we fear, commenced to indulge in "generals" before having sufficiently got up their "particulars." In the first part of their essay they point out the present distribution of the different genera and species of *Cervidæ* over the world's surface, and endeavour to show how they have descended from a common ancestral form. This form they imagine must have been the *Moschidæ*, upon the ground that in order to obtain a deer with horns we must pre-suppose the existence of a deer without horns, and the *Moschidæ* answer this definition. Unfortunately, however, the authors have not yet discovered that their so-called group *Moschidæ* is composed of two forms of animal life that have very little to do with one another. It has been shown most conclusively by the researches of M. Alphonse Milne-Edwards† in Paris, and Prof. Flower in our own country,‡ that the *Chevrotains* (*Tragulus* and *Hyomoschus*), one of the constituents of the *Moschidæ* of MM. Jaeger and Bessels, constitute a family *per se*, quite distinct from the rest of the ruminants, and connecting them with the pigs, and consequently quite distinct from the musk-deer (*Moschus*). In the same way our authors base certain arguments upon the fact of all the typical deer being spotted in the immature state. But, as Dr. Jaeger at least—having been, if we are not misinformed, custos of a zoological garden—ought to know, this is not quite the case, all the *Rusine* deer having their young spotless. Again, arguments are founded upon *Cervus pudu* of Chili living in the Cordilleras, and the other allied species with simple unbranched horns in the plains of South America. But exactly the contrary is the case. *Cervus pudu* is from the low maritime coast of Chili, and one, if not more, of the so-called "*Subulones*" (*C. rufus*) lives high in the Andes of Venezuela and New Granada. From these and other similar instances of erroneous statements which it would be easy to point out, it is, in fact, quite obvious that the authors of this essay have no very special acquaintance with the group upon the distribution of which they treat. We leave it to naturalists to decide whether, under these circumstances, the results arrived at are worthy of much attention. Their theory seems to be that the deer-family reached the New World by an Arctic continent which formerly connected northern Europe with eastern America, and which Dr. Jaeger, in a former paper, has proposed to call "*Arctis*." There are, however, if we are not mistaken, equally good grounds for believing that the numerous, undoubtedly Old-world forms in North America reached it by immigration from North-western Asia.

* 'Die Geographische Verbreitung der Hirsche mit bezug auf die Geschichte der Polar-länder." Von Gustav Jaeger und Emil Bessels. (Petermann's Geographische Mittheilungen, 1870.)

† "Recherches Anatomiques et Palæontologiques sur la famille des Chevrotains," Paris, 1864.

‡ "Notes on the Visceral Anatomy of *Hyomoschus aquaticus*." Proc. Zool. Soc., 1867.

HENDERSON'S PATENT STEEL PROCESS

TWO articles written by Mr. W. Mattieu Williams, called "Papers on Iron and Steel—A Costly and Vexatious Fallacy," were published a short time since in NATURE. These papers are considered in this country to be the clearest and ablest that have ever appeared on this subject, setting forth the reason why all efforts heretofore made to produce steel from English cast-iron by partial decarbonisation have failed; that all manipulations have been directed to removing as much as possible the impurities contained in pig iron by oxidation. He was not aware that new agents have been used, combined with oxygen, and that patents had been granted therefor in England, Nos. 318, 1,051, A.D. 1870 (which were not then published) for combining fluorine with oxygen, and fluorine combined with titanic acid, or with titanium and oxygen.

The new patent process for the production of steel by the partial decarbonisation of cast-iron consists in the combined use of fluorspar or other fluorides and titanic acid, applied to cast-iron at the melting temperatures, preferably in reverberatory furnaces. Fluorine is given off from the fluorspar, and is a more powerful agent for the removal of silicon than oxygen, and removes it almost entirely from the cast-iron before the reactions with the carbon begin; the phosphorus and sulphur are next acted upon and removed in the order they are named by means of the combined action of fluorine and titanic acid or fluorine, titanium, and oxygen, and lastly the carbon is removed. The fluorine is derived from fluorspar combined with iron ores containing titanic acid in such wise as ensures simultaneous action of the fluorine, titanium, and oxygen upon the cast-iron; and by reason of the affinities of these substances for silicon, phosphorus, sulphur, manganese, arsenic and carbon, these substances are taken from the iron in the form of vapour and slag, leaving the purified metal in the condition to be hammered or rolled as merchantable steel.

English pig-iron may be made direct into steel by the new process; and with the large class of irons smelted from hæmatites and specular ores with good fuel, *pure* steel may be made, that will be entirely desiliconised, dephosphorised, and desulphurised, and contain but the required amount of carbon to form steel of any particular degree of hardness, by arresting the decarbonisation of the metal whilst undergoing conversion.

Experiments were made in May last on a large scale at Messrs. Park, Brothers and Co.'s Steel Works, in Pittsburg, in boiling puddling furnaces fettled with such oxides, but without puddling or labour of stirring the iron during conversion; the only labour was that of "balling" and removing it from the furnace after the conversion was completed. The mode of application in the above-named establishment was 125lb. of titaniferous iron ore (similar to the Norwegian) and 50lb. of fluorspar, both in a powdered state, and then charging them evenly over the sole of the furnace. 475lb. of No. 3 charcoal pig-iron, similar in quality to English hæmatite of Barrow, was then charged upon them, and, when melted, was allowed to remain without stirring or puddling. As soon as the pig-iron melted, reactions began between the fluorspar and the titaniferous iron ore, and the silicon and phosphorus contained in the pig-iron. To ascertain exactly the conditions upon which the changes are made in the cast-iron whilst under treatment, samples were taken from the bath of liquid iron. The analysis of the first sample shows that the operation of the new process is entirely different from any other process, inasmuch as the silicon is entirely removed at the early stages of the process; and, with the silicon, phosphorus is also taken from the iron, and the carbon is changed from the graphitic to the combined form. Other samples were taken from the bath at intervals of ten minutes. The analysis of the first sample made it evident that most of the later ones were steel. This has since been confirmed by analysing a specimen taken thirty minutes later from the bath, and by the treatment of them as steel, it having been found that they possess the properties of steel, forging well, and tempering and hardening according to the various degrees of carbon contained in them. At the end of the operation, the charge became wrought-iron, by removal of *all* the carbon. This iron forges, welds, and is neither red-short nor cold-short.

The analyses of the first and fourth samples taken from the bath have been made by Mr. W. M. Habirshaw, analytical chemist, of 36, New Street, New York, and are annexed. Also, analyses of Messrs. Sanderson's and Krupp's cast-steel, and Hoop L Dannemura-Swedish bar iron are annexed for comparison, taken from Dr. Percy's "Iron Metallurgy."

First Sample. Refined Cast-iron, taken 40 minutes after fusion.		Fourth Sample. Steel, taken 30 minutes later.	
Carbon, combined	2·7144	Carbon, combined	0·2172
do graphite	traces	do graphite	none
Slags (silicates)	none	Slags (silicates)	none
Silicon	0·0046	Silicon	none
Phosphorus	0·0349	Phosphorus	none
Sulphur	0·1073	Sulphur	very minute trace
Titanium	trace	Titanium	none
Fluorine	none	Fluorine	none

The presence of sulphur in the refined cast-iron is mostly due to sulphurets mixed with the fluorspar, which was used in the condition in which it was taken from the mine, there being no convenience at hand for dressing it.

Later experiments with other kinds of pig-iron, with impure or fluorspar not dressed, show that at the stage of the process where it becomes refined cast-iron, the increase of sulphur from this cause amounts to 0·1051 per cent.; which with "dressed" or pure fluorspar, would be 0·0022 per cent. of sulphur for the refined cast-iron of the foregoing analysis, instead of 0·1073 per cent.

It will be evident to the practical metallurgist that the refined cast-iron, when treated with *pure* fluorspar, becomes steel of superior quality when decarbonised below 1·90 per cent of carbon.

Hoop L Bar-iron.		Sanderson's Cast-steel.		Krupp's Cast-steel.	
Carbon	0·087	Carbon	not deter.	Carbon	1·18
Silicon	0·115	Silicon	0·24	Silicon	0·33
Phosphorus	0·034	Phosphorus	0·02	Phosphorus	0·02
Sulphur	0·220	Sulphur	0·05	Sulphur	none
Manganese	none	Manganese	0·03	Manganese	trace
Arsenic	trace	Cobalt, nickel	none	Cobalt, nickel	0·12
Cobalt, nickel	none	Copper	none	Copper	0·30
Copper	none	Aluminium	0·12	Aluminium	none
Aluminium	none				

From the foregoing analysis it will be seen that the *refined cast-iron* of the patent process contains but $\frac{1}{25}$ part of the silicon, less than one half the sulphur, and about the same amount of phosphorus, as compared with Hoop L bar-iron. This is the most celebrated wrought-iron made in the world, and is used exclusively for making steel, and sells at 24*l.* per ton at Sheffield.

The analysis of the steel of the patent process shows, as compared with Sanderson's and Krupp's, that while the latter are alloyed with carbon, silicon, sulphur, phosphorus, &c., the *steel* of the new process is practically *pure* iron and carbon.

In the articles before referred to, written by Mr. Williams (and from his intimate practical and scientific knowledge no better authority can be had), he says:—"To make *perfect* steel they take out *all* these latter, and leave nothing but pure iron and carbon. Absolute perfection is not, of course, practically attainable in steel making, but it is approximated in the same degree as the purification of iron from everything except carbon is effected."

Persons wishing to satisfy themselves of the value of the process, by a test, should use the ordinary boiling puddling furnace, fettled with the purest red hæmatite or specular ores, containing the least silica, ground and applied wet, with the ordinary "bottom" made in the usual way: and when perfect steel is required, that contains neither silicon, phosphorus, nor sulphur, good pig-iron should be taken, such as Nos. 1 and 2 Barrow, or West Cumberland hæmatite; and treated with pure fluorspar, and titaniferous iron ore from the Bay of St. Paul, in Canada, containing over 43 per cent. of titanic acid, or from Norway, containing over 40 per cent. of titanic acid; using 48lb. of fluorspar to 118lb. of the ore. They should be ground to fine powder, and mixed, and used dry, and charged evenly over the sole or bottom of the furnace; and 475lb. of pig-iron should be charged upon them, and the furnace closed tight so as to exclude all air, for about 70 minutes. The fire should be kept to the highest temperature. After the metal has been in the furnace this period, samples should be taken from the bath at intervals of five minutes. It will be found that a little experience will soon determine the proper time to stop the process.

It is best *not* to allow the workman attending the furnace to stir or puddle the metal during the conversion, as the fluorspar and titaniferous ore become viscid by the heat of the furnace by the time the pig-iron melts, and, if left alone, will remain on the bottom of the furnace until decomposed by the reactions of the process, when they pass through the iron as vapour and slag, and purify it more effectively than can be done by stirring or puddling; and the slag serves to protect the surface of the metal from the effects of the sulphur in the fuel. The only labour that should be *allowed* is that of removing the steel from the furnace at the required stage of conversion.

The time in conversion of the steel, from the charging of the pig-iron, is one hour and 30 to 40 minutes; this time may be shortened 30 to 40 minutes, by previously refining the cast-iron of all its silicon and most of its phosphorus, by a shorter and more economical process (Patent, No. 1,051) with hæmatites and specular ores with fluorspar, which will become the subject of a future article. JAMES HENDERSON

New York

SOCIETIES AND ACADEMIES

London

Geological Society, November 9.—Mr. Joseph Prestwich, F.R.S., President, in the chair. Lieutenant Reginald Clare Hart, R.E., Brompton Barracks, Chatham; Lieutenant James Frederick Lewis, R.E., Brompton Barracks, Chatham; and Mr. M. F. Maury, jun., 1300 Main Street, Richmond, Virginia, U.S., were elected Fellows of the Society.—(1.) "On the Carboniferous Flora of Bear Island (lat. 74° 30′ N.)," by Professor Oswald Heer, F.M.G.S. The author described the sequence of the strata supposed to belong to the Carboniferous and Devonian series in Bear Island, and indicated that the plant-bearing beds occurred immediately below those which, from their fossil contents, were to be referred to the mountain limestone. He enumerated eighteen species of plants, and stated that these indicated a close approximation of the flora to those of Tallowbridge and Kiltorkan in Ireland, the greywacke of the Vosges and the southern Black Forest, and the *Verneuilii*-shales of Aix and St. John's, New Brunswick. These concordant floras he considered to mark a peculiar set of beds, which he proposed to denominate the "Ursa-stage." The author remarked that the flora of Bear Island has nothing to do with any Devonian flora, and that consequently it and the other floras, which he regards as contemporaneous, must be referred to the Lower Carboniferous. Hence he argued that the line of separation between the Carboniferous and Devonian formations must be drawn below the yellow sandstones. The presence of fishes of Old Red Sandstone type in the overlying slates he regarded as furnishing no argument to invalidate this conclusion. The sandstones of Parry Island and Melville Island are also regarded by the author as belonging to the "Ursa-stage," which, by these additions, presents us with a flora of seventy-seven species of plants. The author remarked upon the singularity of plants of the same species having lived in regions so widely separated as to give them a range of 26½° of latitude, and indicated the relations of such a luxuriant and abundant vegetation in high northern latitudes to necessary changes in climate and in the distribution of land and water.—Sir Charles Lyell remarked that the Yellow Sandstones of Dura Den in Fife, and of the county of Cork in Ireland, contain *Glyptolepis* and *Asterolepis*, genera of fish exclusively Devonian, or belonging to the middle parts of the Old Red Sandstone—also the genus *Coccosteus*, which is abundantly represented in the Middle Old Red Sandstone, and sparingly, or only by one species, in the Carboniferous formation. The evidence derived from these fishes inclined him to the belief that the Yellow Sandstone, whether in Ireland or Fife, should be referred to the Upper Devonian, and not to the Lower Carboniferous, as Sir Richard Griffiths contended, and as Heer now thinks. As to the argument founded on the plants, he considered it an important and truly wonderful announcement, that many well-known Carboniferous species are common to Bear Island (in lat. 74° 30′ N.), in the Arctic regions and to Ireland and other parts of Europe (26° of latitude farther south). But fossil plants are supposed to have a wider range in space and time than fossil fish; and we know that the cryptogamic flora of the ancient coal is remarkable for the wide horizontal spread of the same species, extending from North America to Europe, so that we need not be surprised if many species should extend vertically from the Devonian into the Carboniferous strata. Mr. Carruthers remarked on the bearing of the paper on the Kiltorkan beds, and considered that Dr. Heer had completely established the correlation of the deposits. He differed, however, as to the numerical proportions of the species. He could not recognise *Cyclostigma* as a genus, but considered it founded on insufficient grounds, in which view Prof. Haughton now agreed. It was, in fact, founded on fragments of the bark of *Lepidodendron Griffithsii*, Brongniart, to which species the *Lepidodendron* indicated by Prof. Heer as *L. veltheimianum* really belonged. Other detached portions of this same plant had been described by various authors under no less than seven different specific names, and referred to nearly an equal number of distinct genera, and Prof. Heer had reckoned these as species in his comparison of the Bear Island and Irish floras. Prof. Heer had been led, chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists, to refer these beds to the Carboniferous rather than to the Devonian formation, the Kiltorkan fossil having been established as a very distinct species by Brongniart and Schimper. Mr. Carruthers considered that both the Irish and Bear Island deposits belonged to the Devonian. Mr. Boyd Dawkins pointed out that the proximity of land was exhibited by the presence of terrestrial plants in the deposits, and believed that this might have much to do with the difference in the proportion in the beds. As the marine fauna decayed more rapidly than the terrestrial, it was preferable for classificatory purposes. He mentioned forms of vegetable life which had been recently discovered in America in beds of Cretaceous age. He did not believe that corals could have existed in those high latitudes under anything approaching to the present condition. Prof. Nordenskjöld had failed to discover any traces of glacial action in these beds; and the question arose whether there had been any change in the position of the Pole or in the radiated heat of the sun.—(2.) "On the Evidence afforded by the Detrital beds without and within the North-eastern part of the Valley of the Weald as to the mode and date of the Denudation of that Valley." By Mr. S. V Wood, jun., F.G.S. The author commenced by discussing the various hypotheses that have been proposed to explain the denudation of the Weald Valley. In his opinion the upheaval of the district took place in Post-glacial times, and subsequently to the deposition of the gravels of the Thames Valley, of East Essex, and of the Canterbury heights; and the denudation was effected chiefly by tidal erosion during gradual upheaval in an inlet of the sea, aided by the action of fresh water flowing into this inlet from the north by streams draining the land which now constitutes the counties of Middlesex and Essex. The chief evidence in favour of his views is as follows:—1. The absence from the glacial beds of Essex of any débris representing a considerable denudation of the Weald during the glacial period, and the probability that the Wealden area was beneath the sea during the deposition of the Boulder Clay. 2. The comparative absence of Lower Cretaceous or Hastings-sand materials from the Post-glacial gravel-sheets outside the north of the Weald. 3. The impossibility of reconciling the presence of Tertiary pebbles in certain Weald-gravels with an origin by means of streams flowing in the direction of the present rivers. 4. The antagonism between the character of the major valley of the Weald and that of any excavation producible by the agency of rivers. 5. The persistence of the old coast contour with the river-drainage entering it from the north. 6. The existence of a cause, in the shape of an isthmus at Dover, sufficient to induce a strong tidal scour. Mr. Godwin-Austen thought that the author had done his theory injustice in presenting only a portion of the Wealden area for consideration. He remarked that phenomena similar to those of the Weald were to be found in various parts of Western Europe. He was glad to find that Mr. Searles Wood did not regard the escarpment as representing marine cliffs; but he did not attach sufficient weight to the absence of any material of marine origin at their base, so that there was no evidence of the presence of the sea within the Wealden area. He differed wholly from the author as to the age of the gravels, for beneath the gravels were silty beds containing elephant remains. These gravels he was inclined to refer to a glacial period, as they contain blocks such as could only have been transported by the agency of ice. The elephants found in the valley of the Wey are of the species (*E. primigenius*) which also occurs in the Selsea beds; and he believed both to be of glacial age. As to the theory of the denudation of the Weald, he professed himself a convert to the views of Messrs. Foster and Topley, and cited what was now going on in Heligoland in illustration of atmospheric denudation.—Mr. Whitaker observed that the present absence of gravels along parts of the valley of the Thames affords no proof of their not having formerly existed. He pointed out the soft and friable nature of most of the rocks of the Wealden, which would account for their absence in the gravels. The only really hard rock was the Chert of the Lower Greensand, which was abundant in the gravels of West Kent. Angular flints occurred at the base of the chalk escarpment wherever it had been carried back by denudation. The major valley of the Weald had been spoken of, but he denied that any such valley existed; it was merely a series of numerous small valleys. He could not conceive the rivers flowing against the dip of the strata, as supposed by Mr. Wood. He did not agree in the view of the denudation of the Weald being such an enormous affair, but thought that it might be due to comparatively small causes.—The President pointed out that beyond Southend there was a section precisely similar to that of Grays. It was a mistake to suppose pebbles from the Wealden area did not occur in the Thames gravels.

He thought that much of the denudation of the Wealden area might have taken place before the glacial period. The presence of Tertiary pebbles in the Wealden area might readily be accounted for by their presence at the edge of the escarpment. Mr. Searles V. Wood, jun., in reply, justified himself for having limited his observations to the northern part of the Weald, as it was there only that it could be brought into juxtaposition with the glacial beds. He maintained that, under certain circumstances, no beaches or marine beds were formed at the base of sea-cliffs. He pointed out that in Post-glacial gravels large blocks of rock were frequently found, and protested against limiting all ice-transport to the glacial period. He could not recognise the Selsea beds, with 150 living species, some of southern character, and none extinct, as glacial. He did not acknowledge the alleged softness of the Wealden rocks.—The Earl of Enniskillen sent for exhibition a fragment of Lias Limestone from Lyme Regis perforated by *Pholades*.

Entomological Society, November 21.—Mr. Alfred R. Wallace, F.Z.S., &c., President, in the chair. Exhibitions of *Lepidoptera* were made by Mr. Bond; of *Coleoptera* by Mr. Albert Müller and Prof. Westwood; and Mr. F. Smith exhibited *Phora florea*, a Dipterous parasite in the nest of the wasp. The following paper was read:—"Descriptions of some new diurnal *Lepidoptera*, chiefly Hesperiidæ," by Mr. A. G. Butler.

Ethnological Society, November 22.—Prof. Huxley, President, in the chair. Mr. George Macleay was announced as a new member. Mr. Edgar Layard made some remarks upon a collection of stone implements which he has recently brought from the Cape of Good Hope. Some polished celts from the Naga Hills, between Assam and Burmah, were exhibited, and Lieut. Barrow's notes upon them were read.—A paper was then communicated by Dr. Bleek "On the Concord, the Origin of Pronouns, and the Formation of Genders or Classes of Nouns." The author believes that the classes or genders in the sex-denoting languages originally depended, not upon the meaning of the nouns, but upon their representative particles, which, in these languages, were primiarily at the end of the nouns. These genders were, from an originally large number, gradually reduced, until in the Aryan languages they were mainly two—one with the representative element, U, which is called the *masculine* class, and the other with the representative element, A T I, which is named the *feminine* class. The *neuter* appears to be a later development, into which, however, an original common plural gender, with the termination, A N I, may have been incorporated. To these endings the case-terminations were affixed, and through pressure of the latter the original marks of gender have frequently been obscured. The concord was at first due to the presence of these representative elements of the nouns in their pronominal character. Mr. Hyde Clarke, in eulogising this paper, said that he had by independent investigation arrived at some of the results detailed by the author. The speaker insisted upon the necessity of extending our philological studies beyond the Indo-European languages.

MANCHESTER

Literary and Philosophical Society, November 15.—Mr. E. W. Binney, president, in the chair. "On the Temperature Equilibrium of an Enclosure containing a Body in Visible Motion," by Prof. Balfour Stewart, LL.D., F.R.S. It has been established that in an enclosure containing bodies which are all at the same temperature, and at rest, the same amount of heat enters any surface forming part of the walls of the enclosure as leaves it in the same time, so that the body, of which this is the surface, neither gains nor loses heat. It is also known that if we take, not the outer surface of such a body, but any plane passing through its substance, say for instance one parallel to its outer surface, then, as much heat passes across this plane going into the body, as passes across it going out of the body in the opposite direction; and further, this equilibrium of heat is known to hold separately for every one of the individual rays of which the whole heterogeneous radiation is composed. The effect of the motion of a body in altering the wave-length of the radiated light is also well known. In consequence of this, if a cosmical mass, such as a star or nebula, should be formed of incandescent hydrogen, and be at the same time rapidly approaching the earth, the light which strikes the earth will not be the double line D, but a line more refrangible than it, and therefore this light will be able to pass through a mass of ignited sodium vapour at the earth's surface without suffering absorption, while, however, the light emanating from the sodium vapour will still be the double line D. In such a case, even if the star and the terrestrial sodium vapour should both be of the same temperature, yet the light radiated by the latter will not be the same in quality as that absorbed. This instance would appear to show that the equilibrium which holds in an enclosure of uniform temperature when all the substances are at rest does not hold when some of these are in visible motion, and that if in that enclosure there be a body moving towards or from the surface of the enclosure, the heat which enters the surface from the moving body will not be the same as that which the surface gives out. Suppose for instance that the walls of the enclosure are made of glass, and that the temperature of the whole enclosure including that of the moving body is 0° C., then, were the whole at rest, the heat which strikes the glass surface will all be absorbed at a very short distance below the surface, and in like manner the heat radiated by the glass will all emanate from a short distance below the surface. But let us now suppose, to take an extreme case, that the moving body is approaching one of the glass surfaces so rapidly that the heat which it emits has been so much increased in refrangibility as to enter the boundary of the visible spectrum. Then, while the heat radiated by the glass will still continue to proceed from a very short distance beneath the surface, the heat absorbed by the glass from the moving body will be able to penetrate to a very considerable depth beneath the surface of the glass. The outer layer of glass will thus lose, while the inner layer will gain heat. Now, it is possible to conceive an enclosure with a fixed diaphragm, and containing a revolving body, so arranged that the heat which leaves it in the direction of a certain part of the enclosing surface, shall always be given out by that part of the revolving body which is moving towards the surface; while, on the other hand, the heat given out by the revolving body to another surface, shall be given out when the revolving body is moving from that surface. There will thus be a want of temperature equilibrium among the various layers, those near the surface being somewhat different in temperature from those beneath. But when we have a temperature difference of this kind, have we not acquired the power of converting heat into work? It would thus appear at first sight that the mere presence of a moving body has given us the power of obtaining work from an enclosure all of whose particles were originally at the same temperature. This appears however to be opposed to the theory of the dissipation of energy, and in consequence we are induced to think there must be some error in the assumption. Now, does not the unwarranted part of the hypothesis consist in our supposing that the revolving system can continue to revolve without losing part of its visible motion? When two moving bodies approach or recede from each other, is it not possible that each loses a small part of its visible energy, while at the same time there is a surface disturbance produced in both? It might be said that, believing in a medium pervading all space, we were prepared for a stoppage of motion of this nature, and that there is therefore nothing gained by the supposition which has been made; but it might be replied that by looking at the problem in the above light, we appear to connect this stoppage of motion with other facts, besides being made aware of a source of surface disturbance when cosmical bodies approach or recede from each other.—Postscript added 19th November.—If we imagine a stoppage of the motion of cosmical bodies of the nature above described, then if the two approaching bodies be exactly equal and similar, either extremity of the medium between them will be similarly affected by the motion derived from the approaching bodies; but if these bodies are unequal, the two extremities of the medium will be dissimilarly affected.

Microscopical and Natural History Section, October 10.—Mr. Joseph Baxendell, President of the Section, in the chair. Mr. Joseph Sidebotham read the following paper:—"On the Variations of *Abraxas grossulariata*." The variations in animals and plants are of great interest, and each contribution to the store of facts accumulated relative to these variations, their causes and limits, is of value in determining the identity and limits of species, in whatever way we interpret the word *species*. *Abraxas grossulariata* is probably one of the most variable insects we possess in this country in colour and markings, and it would be quite pardonable in any one not well acquainted with it, were he to split it up into four or five species; but although it varies in colour and markings in such a great degree, all these varieties are joined together by gradual steps, and yet no step is found to join it to the next species on our list, *Abraxas ulmata*. The larvæ of this species will feed upon the leaves of most trees and shrubs, and are therefore easily experimented upon, as to whether the changes in food influence the colour or markings. So far as my own experiments, and I believe those of others are concerned, no difference whatever can be detected from the varieties of food, except in size. That long-continued changes of food through many generations might have a perceptible effect, is however more than probable. The type form of this moth is too well known to require description. I will therefore exhibit a drawer of specimens, having the type form in the centre, the various forms radiating from it in steps, in one line ending in white, another in black, another in which the white ground runs gradually into brown, and various other marked varieties. We may divide these into the following seven groups :—1. Variation. White, or the spots very few and distant : this leads up to the type form. 2. Spots joined together, forming curves and lines. 3. A variety of intermediate spots and patches. 4. The spots at the border becoming lines, and running towards the base of wings. 5. Spots confluent, forming solid black patches over nearly the whole of wings. 6. The spots having the type form, but the white ground tinged with a smoky brown or drab colour, sometimes suffusing the whole of the wings. 7. Spots of the type form, but the ground of wings bright yellow. From various experiments with many thousands of larvæ of this species, I have come to the conclusion that these variations are in a great measure hereditary, that one brood of eggs will produce moths of forms in a great measure identical, if the parents be of the ordinary type ; if the eggs be the produce of moths of extreme colouring, varying much from the type, then, although the bulk of moths will be marked dark or light as the parents, there will be others of the ordinary type, and also some of the very opposite character of marking, precisely as in many florists' flowers the seeds from those varying most from the original form are known to produce the most marked and opposite varieties. These experiments can only produce approximate results, unless a great number of years could be devoted to them, and in this and many others of our most variable species, it is almost impossible to rear them in confinement beyond the second generation.

November 7.—Mr. Joseph Sidebotham exhibited a series of specimens of *Limobius dissimilis*, from Llandudno, on which the markings were very distinct and perfect ; he discovered the species in considerable numbers beneath the flowers of *Geranium sanguineum*.—Mr. Spencer H. Bickham, jun., reported occurrence of *Myosurus minimus*, L., in plenty at Vale Royal, near Northwich, which species, he believed, had never previously been noticed in the neighbourhood. — Mr. Bickham then exhibited a series of specimens of *Polygonum minus*, Huds, collected at Mere and the surrounding district; he stated that he had searched for *P. mite*, Schrank, but without success, and believed with Mr. Hunt, that luxuriant specimens of *P. minus* had been mistaken for it : on the other hand he called attention to the fact that in 1859 Mr. John Hardy, to whom Mr. Bailey had previously alluded, distributed specimens of *P. mite* from Mere, through the Thirsk Exchange Club, and on this authority Mr. J. G. Baker, the Curator, remarked in the report, "new to the Mersey Province." It seems doubtful also whether *Alopecurus fulvus*, reported from the same locality, has not been erroneously recorded, peculiar states of *A. geniculatus* having been mistaken for it. As, however, it was found in considerable quantity at Oakmere in 1868, it appears probable that it may occur elsewhere in Cheshire.

LEEDS

Field Naturalists' Club (Young Men's Christian Association), October 24.—The first meeting of the winter session took place this evening, Mr. Coates in the chair. In entomology, Mr. Liversedge exhibited specimens of *Latyrus ægeria*, *Argynnis selene*, *Anthrocharis cardamines*, and *Pamphila sylvanus*, all collected in this neighbourhood.—Mr. Turner exhibited a variety of insects taken near Selby, including *Cerura vinula*, *Triphæna fimbria*, *Argynnis paphia*, and *Saturnia carpini*. In oology, Mr. Coates brought the nest and egg of the ring ousel found at Ilkley.—Mr. Beevers and Mr. Taylor were the principal exhibitors in the conchological branch, Mr. Beevers exhibiting *Unio pictorum* from Went Vale, *Cyclostoma elegans*, Thorparch, and *Limnea palustris* var. *corvus*, Knaresbro.—Mr. Taylor exhibited *Limnea glabra*, *Helix virgata* var. *submaritima*, *Planorbis corneus* var. *albina*, and a small collection from Wisconsin, U.S.

November 7.—Mr. W. Coates in the chair. Mr. Taylor read a short paper describing a conchological visit to Boston Spa during the present month. Amongst the specimens taken were *Cyclostoma elegans*, *Helix lapicida*, *H. cantiana*, and *Pupa marginata*, specimens of which species, and a number of others, were exhibited.—Mr. Wood brought for exhibition a fine collection of shells, illustrating the Pontefract district.—Mr. Roebuck exhibited several species of shells taken in the neighbourhood of Harrogate.—Mr. Scholefield exhibited the American mosquito and a fine specimen of *Bombyx cynthia*.—Mr. Denny brought for inspection a quantity of wheat infested by the wheat weevil, and a specimen of the death's-head moth, *A. atropos*.—Mr. Liversedge exhibited a number of insects taken in the immediate neighbourhood, including *Lasiocampa quercus*, *Smerinthus populi*, *Nemeobius lucina*, and *Lycæna alsus*. The next meeting was to be held November 21st, when a paper was to be read by Mr. Acomb, "On geology as a study."

NORWICH

Norfolk and Norwich Naturalists' Society, October 25.—The President, the Rev. J. Crompton, in the chair. A most elaborate and interesting paper was read by Mr. F. Kitton, "On Diatomacea and the lower forms of vegetable life as revealed by the microscope." The lecture, for such it may more properly be termed, was illustrated by diagrams, showing some of the most familiar as well as most peculiar forms of Desmids and Diatoms ; and at the close Mr. Kitton exhibited a series of very beautiful photomicrographs, of similar objects, executed by Dr. Maddox. The Chairman, in offering to Mr. Kitton the thanks of the Society, and especially of the members present, for the time and labour he had devoted to their instruction, alluded in complimentary terms to the high reputation he had already attained throughout the scientific world, by his persevering researches in this particular branch of natural history; his skill as a microscopist being equalled only by the extreme accuracy of his descriptions of the most intricate and minute organisms. At the request of the meeting Mr. Kitton consented to his paper being published *in extenso* in the Transactions of the Society.

EDINBURGH

Royal Physical Society, November 23.—Prof. Duns in the chair. The retiring president, Professor Duns, delivered an address, in which he referred to the early history and past achievements of the Society. A hundred years ago, eighteen students of nature banded themselves together for mutual profit in the pursuit of natural science, under the name of the Physical Society. Here is the first list of the ordinary members, Session 1770-1771 :—William St. Clair, M.D. ; David Young, M.D. ; Thomas Melville, Thomas Smith, James French, James Wood, Robert Stewart, Alexander Muir, James Dick, Henry W. Tytler, Malcolm Macqueen, Arthur Taafe, Daniel Gibb, Thomas Thorburn, James Webster, George Home, William Manuel, and William Keir. The names deserve to be brought out of the mists of 1770, and set before the Society in the light of 1870. The period was one well fitted to quicken young and ardent students, and to lead them to long to win their spurs in work closely kindred to that in which others were distinguishing themselves. Eight years previously, Black had made public his theory of Latent Heat, and two years before he had been inducted to the Chemistry Chair in Edinburgh. The influences of the day were bearing in on Hutton's mind, in which "The Theory of the Earth" was shaping itself into compactness and symmetry. Ray's *Synopsis*, Willoughby's *Ornithologia*, Lister's *Mollusca*, and Ellis's *Corallines*, were before the public. But these dealt with British forms. Scotland was still in the rear. Nothing had been done to purpose for Scottish forms, except in the *Scotia Illustrata* of Sibbald, most valuable at the time, no doubt, but also most suggestive of how much still remained to be accomplished. It was in such circumstances the Physical Society began, and more than ten years elapsed before the foundation of the Royal Society of Edinburgh. In 1788 the Physical obtained a Royal Charter, and assumed the name it now bears. Its meetings were held for many years in the Royal Physical Society Hall, Richmond Court, a building which stood on a site now occupied by a chapel. From the outset its influence over working naturalists was great and beneficial, and it ultimately absorbed other kindred associations, which had been at different times set up in Edinburgh. The Chirurgo-Medical, its senior by a few years, joined it in 1788; the Hibernian Medical, in 1799; the Chemical, in 1803; the Natural History, in 1812;

the Didactic, in 1813; and the Wernerian, in 1858. Between 1771 and 1788 many well-known names occur among its list of members—Benjamin Bell, Professors Alexander Munro, J. Hope, Joseph Black, Francis Home, James Gregory, Alexander Hamilton, and W. Hamilton (Glasgow). In 1802, Dr. Barclay and Charles Bell; in 1814, David Brewster. By its union with the Natural History Society it enrolled among its members the botanists, James Edward Smith and Robert Brown, and another, great in almost every department of science, literature, and law, Henry Brougham. Brown's papers on the "Botany of Angus," and on the "Sexes of Plants," are models in this department, and Brougham's on "Thunder" and "Combustion" will well repay a careful perusal. One other notice: in 1828 the Plinian approached the Royal Physical with proposals to unite, and both societies appointed influential committees, with powers to form a union. After much consultation, they reported "that the union of the societies had been admirably accomplished." But the Plinians, after the union was consummated, rued the act, and refused to associate with their lawful head. The Plinian lived on for a season in cold estrangement, and gradually passed into the dark. On the list of ordinary members of the Plinian is the name "Charles Darwin, Shrewsbury, Nov. 26, 1826." The history of the Royal Physical Society is substantially that of Scottish zoology. The latter could not be written without the former. I have only to choose these names from the list of our presidents to make good this remark:—Robert Knox, Captain Thomas Brown, Edward Forbes, Robert K. Greville, James Y. Simpson, John Coldstream, George Wilson, John Goodsir, Alexander Bryson, William Dick, Hugh Miller, Sir John Graham Dalyell, and John Fleming. In these men was embodied the great characteristic of our society. They were all practical naturalists. In November, 1849, Professor Fleming delivered the opening address, in which he urged the expediency of steps being taken by the society to bring before the Government and country the great want of a general national museum for the native products of Scotland, and to bring together the other collections in Edinburgh under one roof. Steps were soon taken in these directions by public bodies and by influential individuals. It again fell to Fleming to give the opening address, in 1855, and he could say—"The gratifying intelligence at last reached us that the Board of Trade had resolved to institute an industrial museum for Scotland in Edinburgh." The Society might claim the merit of one of the first agitations for this great national institution. Dr. Duns passed a high eulogium on the researches of Dr. Strethill Wright on the *Cœlenterata* and *Protozoa*, referring to the sensation produced by the deep-sea dredging report, intimating the growth of chalk in one of the dredged localities. But honour to whom honour. In 1861, this note occurs in Dr. Wright's address to this Society. Referring to the oolite and the chalk, he says: "Similar deposits are now in process of formation over vast areas of sea bottom, especially in the Atlantic, Mediterranean, and Australian seas." Since the Society last met it had lost one of its most distinguished members, Sir James Young Simpson. Dr. Simpson was born at Bathgate, Linlithgowshire, on the 7th June, 1811. He sprang from a family long resident in the district, comfortable in worldly circumstances, and noted for their strong mental powers and outstanding individualities. After being educated at the parish school, where for several years he had for a companion the late Prof. John Reid, of St. Andrews, Simpson entered the Arts course of the University of Edinburgh. He commeneed his purely medical studies in 1827, and graduated as M.D. in 1832. Immediately after graduation he was elected President of the Medical Society. In 1833 he petitioned for a seat in this Society, recommended by Edward Forbes. From 1832 to the beginning of 1836, he acted as assistant to Prof. John Thomson, who occupied the Chair of General Pathology in the University. In Session 1839-40 he gave a course of lectures on midwifery, and in 1840 he was elected by the Town Council to the Chair of Midwifery, vacant by the resignation and subsequent demise of Dr. Hamilton. Dr. Simpson died on the evening of the 6th May, 1870. Dr. Duns concluded by some apposite observations on the motives that should incite to natural history studies, and the methods by which they should be pursued.

Botanical Society, July 14.—Sir Walter Elliot, President, in the chair.—1. "On Kashmir Morels." By Mr. M. C. Cooke, India Museum, London. The author remarked, that it has long been known that truffles and morels are found in N. W. India and Kashmir, but no attempt has hitherto been made to determine the species. Some years ago, application was made to the Agricultural and Horticultural Society of the Punjab, and to other sources, for specimens, but without any result. He had, however, lately received, through Dr. J. L. Stewart, a string of dried morels, said to be the morels of Kashmir, and sent by Mr. Baden Powell, of Lahore. This string contains two distinct species, both of them small, and neither of them the *Morchella esculenta* of European markets. The author gave some account of the history of morels as far as known, and concluded by giving scientific descriptions of the supposed two new species from Kashmir. 2. "On the Characters of the Flowers of *Silene maritima* and *Silene inflata*, as regards their Stamens and Pistils." By. Dr. F. Buchanan White. The author had examined 72 plants and 201 flowers of *Silene maritima*; of these, 39 plants were perfectly hermaphrodite, 11 had the stamens abortive, 10 the styles abortive, 11 the styles partly abortive, and 1 with the stamens partly abortive. Of the 201 flowers examined, 122 had three styles and three-celled ovary; 68 had four styles and four-celled ovary; and 11 had five styles and five-celled ovary. 3. "Notes of a Botanical Excursion to the neighbourhood of Perth." By Mr. John Sadler. 4. "Results Obtained from the Cutting and Transplanting of a Plaited Hornbeam Hedge." By Mr. M'Nab. 5. "On the Guachamacan, a poisonous plant growing in the Llanos (plains) of Venezuela." By M. A. Ernst, Caracas.

GLASGOW

Geological Society, November 3.—Mr. John Young, Vice-president, in the chair. Mr. James Thomson, F.G.S., submitted to the Society some remains of fish and molluscan life, which he had recently discovered in the neighbouring coal-fields, and which were new at least to the west of Scotland. These were *Acanthoides Wardii*, from Airdrie; *Athyris pisum*, from Brockley; and *Anomia corrugata*, from Dalry. He pointed out the characteristics of these species, and described the relative position of the beds in which their remains had been found. 1. The *Acanthoides* was a well-preserved specimen, showing the dorsal and anal spines in their natural position. This was of some importance, as these spines had frequently been found singly, and could not be referred to any known genus; but this discovery enabled palæontologists to name and classify these ichthyodorulites. This species had also been discovered in the Staffordshire coal-field by Mr. John Ward, and named by Sir Philip Egerton, F.R.S., after its discoverer. It also occurs in the Edinburgh coal-field. The specimen before them had been found near Airdrie, in the upper members of the Clyde coal-measures. 2. *Athyris*. This little fossil occurs at Brockley, Lesmahagow, and Roughwood, Ayrshire. From the resemblance to *Terebratula Saculus*, it had often been mistaken for that shell; but when placed under the microscope the structural characters indicated that it could not be referred to that genus. It had been submitted to Mr. Thomas Davidson, F.R.S., who named it *A. pisum*, from its pea-like form. 3. *Anomia corrugata*. This is the first well-authenticated specimen of Anomia that has been recorded from the Scottish mountain limestone. It is found in a band of shale which underlies the "Linn" limestone, near Dalry.—Mr. D. C. Glen, C.E., gave some notes on the boulder-clay laid open in the excavation now going on for a new dock at Cartsdyke, near Greenock, and referred to the abundance of arctic marine shells and other organisms found embedded in it. The shell-bed seems to occur in a hollow of the boulder-clay, which has been exposed to view by a deep cutting running parallel to the river, or east and west. On the northern side of this cutting, nearest the river, the bed is several feet in thickness; but on the other side it thins out, and finally disappears as we recede from the shore. In the other direction, from east to west, it is seen to abut suddenly against the boulder-clay, and thus occupies a hollow of no great extent, in which, however, an immense number and variety of marine organisms are crowded together, forming one of the richest beds of such clay yet discovered on our western coast. At the same time there was reason to doubt whether the deposit is now found in its natural position, or has not been dug out from some neighbouring part of the shore, and laid down to improve and level the ground, many years ago, in forming the policies where the excavation is being made. On this point, however, he would not express a decided opinion, and other members who had visited the spot were not unanimous regarding it.

DUBLIN

Royal Irish Academy, November 14.—The Rev. Professor Jellett in the chair. The Rev. Maxwell Close read a paper "On M. Delaunay's Views relating to the condition of the Interior of the Earth." The paper was referred to the Council for publication.—Mr. Samuel Ferguson, LL.D., read portions of a paper "On the difficulties attendant on the Transcription of Ogham Legends, and the means of avoiding them." He presented the Academy with a series of casts of Ogham Legends, and pointed out the advantages of them to students of the subject. Dr. Stokes and Professor Ingram congratulated the Academy on this important addition to its collection, and Dr. Ferguson was invited to consider the expediency of issuing engravings of the casts. The reading of the remainder of the paper was postponed to a future meeting.—At a meeting of the Council of the Academy on the 9th inst., it was resolved to recommend to the Academy that Her Majesty's Government be memorialised to use their good offices in order to prevent, as far as possible, any injury during the present siege to the collections in Paris, which are universally acknowledged to be of inestimable value to science, literature, and art. In pursuance of this resolution the following memorial to the Government was adopted on the motion of Dr. Ingram, seconded by Professor Hennessy:—"We, the president and members of the Royal Irish Academy, desire to call the earnest attention of Her Majesty's Government to the irreparable loss which would be sustained by the whole civilised world if the inestimable scientific, literary, and other collections of Paris should be destroyed or seriously injured during the siege. That city contains galleries stored with treasures of art, libraries rich in every species of literary monument, and scientific museums which are amongst the foremost in their several kinds. These collections represent the accumulated labours of many generations, and are, in truth, the property not of France only but of the whole civilised world. Many of the objects contained in them, if once allowed to perish, no subsequent exertion could ever replace. The fate of the library at Strasburg shows that these priceless collections are in real and imminent peril from the operations of the war. It is not for us to pronounce any opinion on the merits of the present lamentable struggle, or on the conduct of either of the contending parties; but as members of a body having for its object the cultivation of science, literature, and archæology, we protest, in the name of the intellectual interests of humanity, against the destruction of these collections; and we respectfully call upon Her Majesty's Government to use their utmost efforts for their preservation, by impressing on the belligerents the duty of taking every possible precaution for their protection from the dangers to which they are likely to be exposed."

Royal Geological Society of Ireland.—W. Stokes, F.R.S., in the chair. The Rev. Prof. Haughton, F.R.S., read a paper "On the amount of horizontal thrust produced by the secular cooling of the earth, and its effect in producing continents and seas." In the discussion which followed the reading of this paper, Professor Hull, Rev. Maxwell Close, and Mr. William Ogilby, took part. Professor Macalister, hon. sec., exhibited a collection of volcanic rocks and of fossils from South Italy, presented by Prof. Guiscardi and Mr. R. Mallet, also a collection of fossil Devonian Plants from Nova Scotia, presented by Principal Dawson.

BERLIN

Royal Prussian Academy of Sciences, July 14.—Dr. A. W. Hofmann read a memoir on the Aromatic Cyanates, containing investigations on derivatives of the phenyle, tolyle, xylyle, and naphtyle series.

July 25.—M. Kummer read a paper on the Algebraic Systems of the third order—Prof. W. Peters read descriptions of New Species of Shrews from the British Museum. The species were *Crocidura retusa*, from Ceylon, *C. fœtida* and *C. doriæ*, from Borneo, *C. monticola*, from Java, *C. microtis*, from Hong Kong, and *C. gracilipes*, from Madagascar, and belonging to the subgenus *Pachyura*, *C. waldemaris*, from Bengal, *C. ceylanica*, and *C. media*, from Ceylon, *C. sumatrana*, from Sumatra, *C. fuscipes*, from Singapore and Java, and *C. luzoniensis*, from Manilla.—Dr. Hofmann read an account of various investigations relating to the action of cyanogen upon aniline and triphenylguanidine, to a new class of cyanic ethers, to a new mode of formation of the isonitriles, to tests for cyanuric acid and chloroform, to the diagnosis of primary, secondary, and tertiary amines, to the knowledge of phenylxanthogenamide, to the action of acetic acid upon phenylsenföl, to the history of the ethylene bases, to the knowledge of aldehyde-green, and to the molecular volumes of chinone.

PHILADELPHIA

American Philosophical Society, Oct. 21.—Prof. Cope read a paper "On the Osteology of *Megaptera bellicosa*." He stated that this species of whale was one of the few whalebone whales of economic value found within the tropics, being the object of pursuit in the Caribbean Sea. Having received a skeleton from the island of St. Bartholomew, West Indies, he presented a detailed account of its structure. He pointed out important points by which it differed from the known species of *Megaptera*, among others in the form of the mandible and of the nasal bones.—Dr. George Emerson read a paper on the part taken many years ago by the American Philosophical Society and Franklin Institute of Philadelphia in establishing stations for meteorological observations in Pennsylvania, detailing the arrangements adopted by them for procuring a full series of observations at fifty-two points in the State.

BOOKS RECEIVED

ENGLISH.—Odd Showers, or an Explanation of the Rain: Carriber (Kerby).—Our Feathered Companions: Rev. T. Jackson (Partridge).

FOREIGN.—(Through Williams and Norgate)—Steinkohlentheer: A. Pubertz.—Beiträge zur Histologie des Gehör-organes: Dr. Rüdinger.—Die Kleinschmetterlinge der Umgegend Münchens: A. Hartmann.—Biologische Briefe von Dr. G. Jäger.—Die Praxis der Naturgeschichte: P. L. Martin.—Geometrie der räumlichen Erzeugnisse ein-zwei-deutiger Gebilde: Dr. E. Weyr.—Die Geometrie und die Geometer vor Euklides: C. A. Bretschneider.—Die Pflanzenstoffe, 3te Lieferung: Husemann.—Elemente der Mineralogie: C. F. Naumann.—Beiträge zur Biologie der Pflanzen, 1te Lieferung: Dr. F. Cohn.—Die Spectralanalyse: Dr. Schellen; 2te Auflage.

DIARY

THURSDAY, DECEMBER 1.

ROYAL SOCIETY, at 4.—Anniversary Meeting.
LONDON INSTITUTION, at 7.30.—On Gems and Precious Stones: Prof. Morris.
LINNEAN SOCIETY, at 8.
CHEMICAL SOCIETY, at 8.—On some Derivatives of Anthracene: Mr. W. H. Perkin.
SOCIETY OF ANTIQUARIES, at 8.30.—Faliscan Inscription: Padre Garucci.

SUNDAY, DECEMBER 4.

SUNDAY LECTURE SOCIETY, at 3.30.—On the Telescope and its Discoveries: Mr. R. A. Proctor.

MONDAY, DECEMBER 5.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.
LONDON INSTITUTION, at 4.—On Chemical Action: Prof. Odling.

TUESDAY, DECEMBER 6.

ANTHROPOLOGICAL SOCIETY, at 8.—On the Races inhabiting the British Isles: Mr. A. L. Lewis.—On Archaic Structures of Cornwall and Devon: Mr. A. L. Lewis.—On Forms of Ancient Interment in Antrim: Dr. Sinclair Holden.

WEDNESDAY, DECEMBER 7.

SOCIETY OF ARTS, at 8.—On the American System of Associated Dairies, and its bearing on Co-operative Farming: H. M. Jenkins.
GEOLOGICAL SOCIETY, at 8.—On Fossils from Cradock, Cape of Good Hope: Dr. George Gray.—On some points in South-African Geology, Part 2: Mr. G. W. Stow.—On the Geology of Natal: Mr. C. L. Griesbach.—On the Diamond-districts of the Cape of Good Hope: Mr. G. Gilfillan.

THURSDAY, DECEMBER 8.

LONDON MATHEMATICAL SOCIETY, at 8.—Further Remarks on Quartic Surfaces: Prof. Cayley.—On the Polar Correlation of two Planes, and its Connection with their Quadric Correspondence: Dr. Hirst.—On Systems of Tangents to Plane Cubic and Quartic Curves: Mr. L. J. Walker.—On the Order and Singularities of the Parallel of an Algebraical Curve: Mr. S. Roberts.
SOCIETY OF ANTIQUARIES, at 8.30.
LONDON INSTITUTION, at 7.30.—On Count Rumford and his Philosophical Work: Mr. W. Mattieu Williams.

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