THURSDAY, NOVEMBER 24, 1870

THE CLAIMS OF SCIENCE

THE Statistical Society, which held its first meeting for the session 1870-71 on Tuesday, the 15th inst., had the claims of Science brought before it in a paper read to it by Dr. Guy, one of its vice-presidents. The paper was written with the practical aim of commending and furthering a scheme which the Statistical Society has set on foot, and in which it has invited the Institute of Actuaries, the Social Science Association, and several other scientific bodies, to participate. The object these societies have in view is to provide a common home in which they shall enjoy the advantage of fixity of tenure and the sense of permanence, with suitable and economical arrangements for carrying on their scientific work. They wish to provide for themselves a common theatre, convenient offices, spacious libraries, and—in the case of societies requiring moderate museum accommodation—museums. All this the societies aim at accomplishing within moderate limits and at a reasonable cost; for they feel very naturally that when the Government has made provision at Burlington House for six leading societies, and other institutions have provided their own isolated accommodation, there no longer remains any place or pretence for a large and comprehensive scientific centre. The building contemplated by the associated societies would have all the unity of character now practicable, if its principal tenants were to consist of societies having a common aim. Such an aim is to be found in the culture of the sciences now known as "social," or societies which make man himself, as the unit of society, the object of their study.

If we define Science as "knowledge in its most definite, condensed, and exquisite form, dealing with worthy objects, and applied to worthy uses," it may be stated, as a truth worthy of general acceptance, that every branch of knowledge that is, by common consent, stamped with the word *science*, aims at some useful and worthy object, studies a certain defined order of things, which it identifies by accurate descriptions and exact definitions, by expressive words and phrases; which it arranges in lucid order, under classes and sub-classes; on which it brings to bear the most delicate instruments and most refined methods of analysis; to which it applies, as far as practicable, the rules of logic and the figures of arithmetic; crowning the entire edifice, if it proves equal to the burthen, with some comprehensive numerical theory.

Passing from this general view of science, and coming to that branch of it now known as *social*, we may trace the seeds of it back to the parish registers of 1538 and the enactment of Henry VIII., respecting leases for three lives, or twenty-one years, through the London Bills of Mortality and the commentaries of Grount and Petty, through the early attempts of Halley to construct a table of mortality from the death registers of Breslau, through the prison inspections of John Howard, up to the establishment of the Statistical Society in 1834, and the foundation of the Social Science Association in 1857; the Statistical Society having, as is well known, been set on foot with the object of collecting "facts calculated to illustrate the condition and prospects of society," which was what Gottfried Ochenwall, of Göttingen, who coined the word *Statistik*, really meant by that word. The Social Science Association, therefore, was a second development and a modified culture of that branch or division of human knowledge—that science of States—to which had been previously given the name of Statistics. The two societies have a common aim—the improvement of man's condition physical, intellectual, and moral, through the patient heaping up, intelligent sorting, and critical examination of the elements of a knowledge which, properly applied, is power indeed.

This social science, of which the *Institute of Actuaries* cultivates a very important section, differs from most other sciences chiefly in this, that its units are of variable magnitude, and that its truths and principles, gathered from large assemblages of such units, admit of application only to like collections of facts, not to the individual units themselves. The actuary has the function of first establishing truths of this order, and then applying them; the statist must look to the statesman to carry into effect the practical works of justice and benevolence. The association of the Statistical Society and Institute of Actuaries with the Social Science Association and Law Amendment Society is, therefore, one pointed out by the nature of things; and we may hope to see them some day working side by side under one roof with one common aim—"*the improvement of man's estate.*" But this principle of association admits of being carried much farther, so as at length to embrace in one group, under one roof, all the societies or associations that make man himself, as a physical and moral unit, the object of their study.

The section of Dr. Guy's paper that treated of *scientific societies and associations*, consisted of an historical retrospect of the rise and origin of most of the societies now existing, finishing with some details of the number and composition of the Statistical Society's members, and of the number of members of the allied societies. Into these details we shall not enter, but we shall restrict ourselves, in what we have yet to say, to the views expressed by Dr. Guy on the subject of the claims of science to public recognition and support. After pointing out that science has found favour, encouragement, and support under every form of Government, that kings have acknowledged that it adds lustre even to thrones, and republics have deemed it quite consistent with their sterner virtue to hold out to it the hand of fellowship—a recent notable example of which has been afforded in the pecuniary assistance and means of transport afforded by the United States to two parties of its citizens bent upon voyages to Spain and Sicily to view the total eclipse on the 22nd of December, an example which our Government has at last, however, willingly consented to follow,—the paper proceeded to do justice to our own Government. The refusal, followed by a slow repentance, was quite an exception to the rule in England. It could only have occurred during one of those cold fits of economy to which the nation is subject at the close of some feverish paroxysm of prodigal expenditure; or it may have been an outbreak of the hypochondriac fancy that they are on the brink of ruin, which is apt to seize the richest nations no less than the wealthiest individuals. It is not difficult to show that Science, in the sense of knowledge of the more precise, exact, and exquisite order, has claims to public recognition and support on the ground of benefits conferred on the nation in the shape both of honour and profit; that it shares with righteousness the prerogative of exalting a nation (for the love of truth, which causes men to seek after knowledge and the patient industry and self-denial which are the first conditions of the search, are among the manly virtues that give strength and solidity to a people); that it must be preferred before learning, as being more practical, and coming into more direct contact with the realities of life; before art, as less apt to be turned to unworthy uses, more sure not to become an agent of effeminacy and luxury.

Of the good gifts which Science showers upon mankind, we may find grand and convincing examples in the works of the hygienic heroes of the last century—Sir George Baker and his masterly demonstration of the cause of the Devonshire colic, Captain Cook and his successful prevention of scurvy, John Howard and his prison work, ending in the destruction of the Jail Fever, and Jenner, with his discovery of vaccination. We fully sympathise with the concluding words of this part of our author's paper:—"By what figures of arithmetic shall I attempt to measure the greatness of these four gifts of science, freely bestowed upon us, and upon all men everywhere, in the short space of a single generation? I believe it to be no exaggeration to affirm that the great war of the French Revolution was brought to a successful issue as much through the lives thus saved as by the valour of our soldiers and sailors. Such have been the triumphs, such the precious gifts, of this one science of hygiène." Other illustrations of the same class, that is to say, showing direct profit to the nation, may be drawn from the Science of Chemistry, of which the whole history, from first to last, is one unbroken series of purely scientific discoveries made for love of truth, without thought or hope of reward, but, sooner or later, turning to profit in the hands of our manufacturers.

We might cite examples from the discoveries of Davy and Daniell, and the arts of electrotyping and photography, discoveries appealing to universal experience of the manifold obligations under which science and scientific men have laid mankind for all the arts which make our civilised existence to differ from the rude life of the savage. The Penny Post, with its world-wide benefits, is the result of a scientific demonstration belonging to the methods and domain of Social Science.

We conclude with the following statement of the special claims of the Statistical Society and its associates in the culture of Social Science:—"The scientific labours of our members, inspired by a mere love of truth, looking to no pecuniary reward, and bearing directly on the very questions which come under discussion in the Legislature, are in many cases a direct saving of expense to the nation. An important (perhaps a very costly) return is made to Parliament. It abounds in tables and columns of figures. The work of analysis, which must be undertaken if the return is not to become so much waste paper, if Parliament and the public are to profit by the expense incurred—this work of analysis is done by some member of the society seized with a wholesome curiosity to know the truth. He bestows upon it time, and thought, and the skill acquired by practice; he submits his work to the criticism of the Society, his paper is published in its *Journal*, at its proper cost; and thus the public and the Government save money and become possessed of wholesome and fruitful truths." These are claims which, we think, the Government will feel bound to recognise, and we wish the cultivators of the Social Societies every success when they come to represent them in the proper quarter.

THE SOURCES OF PHOSPHATIC MANURES

"PRACTICE with Science" is the title of a volume of essays (the second of a series), issuing from the Royal Agricultural College, Cirencester, and containing contributions from the members of the staff of that institution. Amongst other papers is an interesting account by Prof. Thiselton Dyer of the geological distribution of Tricalcic Phosphate; that is to say, a sketch of the chief sources of mineral phosphate of lime, whether as apatite, osteolite, phosphatite, coprolite, or guano. Mr. Dyer points out the abundance of phosphate of lime in igneous rocks, but hesitates about tracing its origin in such beds either to direct chemical combination, or to the inclusion of organically-formed phosphate in the rocks in question. He does not, in short, discuss the possibility of the combination of phosphoric acid and lime in the primæval state of the globe without the intervention of life, which one distinguished geologist at least denies. Mr. Dyer traces the occurrence of tricalcic phosphate in the various sedimentary deposits with great care, having obviously taken much trouble to render his statement an exhaustive one. He considers the many structureless masses of phosphatic deposits which occur "as residuary evidence of formerly existing life, of which they are to some extent the measure," as graphite is in other cases. A greater influence in the production of these masses is attributed to animal than to vegetal life, though marine plants are stated to be especially rich in phosphate of lime, and have undoubtedly played their part in its introduction into sedimentary strata. Mr. Dyer mentions that the recent Brachiopod Lingula has 86 per cent. of phosphate of lime in the mineral ingredients of its shell; and the occurrence of large quantities of phosphate of lime in the great Laurentian and Silurian formations is noticed by him in detail, as well as its occurrence in Devonian and Carboniferous limestones. In emerging to the group of mesozoic strata, we leave behind almost entirely those veins and beds of "phosphate" which occur in the older and more changed rocks, where the segregation of the phosphate of lime has been more completely effected, owing to the greater age of the beds. In mesozoic and tertiary strata we find those nodules which have so erroneously been confused with "coprolites"—the droppings of fish, which are not unfrequently preserved in the fine sediment of the Liassic and the Rhætic beds of the chalk—though beds of flaggy phosphate also occur in some deposits of this age.

Mr. Dyer accepts the history of the origin of these nodules which I have advocated (Geol. Magazine, vol. v.), in describing those which occur below the Suffolk Crags. Clay has a remarkable power of detaching phosphate of lime from its solution in carbonated water; and the phosphatic nodules are bits of clay which have become imbedded with great quantities of bones, and in some cases, very probably—as suggested by Mr. Seeley, of Cambridge, with regard to the Cambridge nodules—with sea-weed too; whence, by the intervention of gas-charged water, they have extracted the phosphate: hence all beds of phosphatic nodules occur near to argillaceous strata of special character. Much of this process, no doubt, went on whilst the bones and clay-lumps lay on the ancient shores, and were daily washed and infiltrated by the sea-water, or lay entirely submerged in masses: but Mr. Dyer thinks that the process of transference would continue after the beds had been left high and dry, and may be now going on; though I think it is clear that the phosphate of lime in the nodules came from bones which have been destroyed and lost in the process, having been very different in mineral condition to the fragments which now remain amongst the nodules of these valuable "bone-beds." Mr. Dyer notices Rhætic, Jurassic, Cretaceous, and Tertiary accumulations of phosphatic nodules. There is one which has not been hitherto recorded, and which is not alluded to in this paper, but is interesting, and in a well-known locality; it occurs in the Wealden series, near Brook, in the Isle of Wight, and is in parts five or six feet thick. The nodules are light-coloured, and aggregated into masses so as to form a solid bed, and not a pebbly conglomerate, as is usual.

The distribution and origin of Guano is briefly given. True guano is simply the dung of sea-fowl, and can only accumulate in rainless districts. Guano rock is the result of the action of water on this matter and subjacent calcareous coral rocks; the celebrated Sombrerite is of this nature. It is very possible that much of the palæozoic phosphatic rock may have been produced in this way, in those beds, at any rate, which we may believe to have been formed subsequently to the evolution of terrestrial vertebrate forms of life.

The other essays in this volume treat of more strictly agricultural subjects, and are accordingly of more limited interest.

E. RAY LANKESTER

SCIENTIFIC YEAR BOOKS

The Year-Book of Facts in Science and Art. By John Timbs. Pp. 288. (London: Lockwood and Co., 1870.)

Annual of Scientific Discovery, or Year-Book of Facts in Science and Art for 1870. Edited by John Trowbridge, aided by Samuel Kneeland, M.D., and W. R. Nichols. Pp. xxii. and 354. (Boston: Gould and Lincoln; London: Trübner and Co., 1870.)

L'Année Scientifique et Industrielle. Par Louis Figuier. Quatorzième Année (1869), pp. 606. (Paris: Hachette; London: Williams and Norgate, 1870.)

Causeries Scientifiques. Neuvième Année (1869). Par Henri de Parville. Pp. 363. (Paris: Rothschild; London: Williams and Norgate, 1870.)

Annuaire Scientifique. Par P. P. Dehérain. Neuvième Année (1869), pp. 387. (Paris: Masson; London: Williams and Norgate, 1870.)

Jahrbuch der Erfindungen. Herausgegeben von H. Hirzel, und H. Greschel. Fünfter Jahrgang, pp. 416. Leipzig: Quant; London: Williams and Norgate, 1869.)

GROUPING these volumes according to the languages in which they are written, we may dismiss the first two with a very few remarks. Mr. Timbs literally gives his readers nothing whatever but a collection of cuttings from the most miscellaneous sources, including the *Pall Mall Gazette*, *Times*, *Spectator*, *Illustrated News*, *Liverpool Albion*, &c.; while Mr. Trowbridge and his coadjutors (who have a respectable scientific status) present us with a much more perfect, although still an incomplete, picture of the leading discoveries of the year. The introductory notes by the Editor constitute the most valuable portion of the American book, which treats of the progress of science, under the respective heads of (1) Mechanics and Useful Arts, occupying 135 pages; (2) Natural Philosophy, to which 64 pages are devoted; (3) Chemistry, (4) Geology, (5) Biology, (6) Astronomy and Meteorology, and (7) Geography and Antiquities. This volume, like that of Mr. Timbs, exhibits a too free use of the scissors, but the extracts are almost invariably taken from periodicals of good scientific repute.

If our readers require any specific evidence of the English editor's unfitness for his office, we would refer them to the article headed "Singular Plant," in p. 200 of the "Year-Book of Facts." It is obvious from the most cursory perusal of the history of this "singular plant," that it is merely a fine specimen of coral, and the absurdity of the story was exposed in a number of the *Gardener's Chronicle* subsequent to that in which it originally appeared. The correction was, however, overlooked by the learned editor.

The French Year-Books differ materially from one another in their modes of arrangement. In this respect we prefer that of M. Figuier to the others. It includes a large number of subjects arranged in the following order:—Astronomy, Mechanics, Physics, Meteorology, Chemistry, Civil Engineering, Voyages and Travels, Natural History, Public Health, Physiology and Medicine, Agriculture, and the Industrial Arts.

The science-gossips of M. de Parville are a collection of papers such as a physicist might contribute to a popular journal. The matter in this volume is more digested, and is in a far less crude and fragmentary state than in the other books we have noticed, and the individual facts are dovetailed together so as to make the style agreeable and the reading continuous. It includes in its range—Astronomy, Physics, Mechanics, Chemistry, Physiology and Medicine, Natural History, Engineering, and unplaceable topics.

In some respects M. Dehérain's volume is the best of the three. Although less comprehensive in its scope than that of M. Figuier, or even than that of De Parville, it is more perfect so far as it goes. It is divided into two parts, treating respectively of the pure and of the applied sciences. Under the pure sciences he places Astronomy, Physics, Chemistry, Meteorology, Botany, Physiology, and Anthropology; while the applied sciences include Civil Engineering, Applied Chemistry, Medicine, and *Exploitation des Animaux*, for which we have no exact English equivalent. It is, we think, doubtful whether this sub-division of the sciences will bear criticism, but it is needless at present to discuss that subject. Instead of flying from flower to flower like the busy bee of our early days, M. Dehérain confines himself to one or two of the most important subjects in each department, and these he treats with far more fulness than the preceding writers. For example, under Chemistry we have an article on Explosive Compounds, containing a review of the works of Nobel, Abel, Berthelot, and Saint-Claire Deville, by the editor; and another, by M. Laudrin, on the Influence of Pressure on Chemical Phenomena, being a review of the works of MM. Berthelot and Cailletet; and these, with a biographical sketch of Professor Graham, complete the section on this science. Similarly, the only information that he gives us on Botany is included in an article by M. Vignes on the Geographical Distribution of Vegetable Species, based on the works of Sir Dalton Hooker (*sic*), and an article by himself on the Maturation of the Cereals.

Amongst the subjects most fully considered by MM. Figuier and De Parville are the Suez Canal; the cause of the explosion in the Place de Sorbonne, and the recent history of picrates and other explosive compounds; the discussion regarding the modification of the metre; the Newton-Pascal forgeries; chloral and its action; and the deleterious effects of absinthe. The question whether Coralline (one of the coal-tar colours) is or is not deleterious as a dye is fully discussed in the volumes of MM. Figuier and Dehérain.

Although it is less extensive in its range of subjects, we are inclined to prefer Hirzel andGretschel's "Year-book of Inventions" to any of the preceding volumes. The departments of science which it includes are Astronomy, Physics and Meteorology, Mechanics and Mechanical Technology, and Chemistry and Chemical Technology. With the view of briefly explaining the arrangement adopted by the editors, we may state that under "Physics and Meteorology" are included molecular physics, acoustics, optics, the theory of heat, and electricity and magnetism. Under the heading "Mechanics and Mechanical Technology," only five subjects are considered, but they are all treated in considerable detail. They are—dynanometers, mechanism applied to locomotion (including the mountain railway system of Marsh and Fell, the road engines of Larmanjat and Thompson, velocipedes, and Kettendampffschifffahrt or chain-steam-navigation), sewing and knitting machines, to which more than thirty pages are devoted, and new pumps constructed on various principles. This work is executed in a higher scientific spirit than any of the preceding volumes, excepting, perhaps, that of M. Dehérain.

It would carry us far beyond the proposed bounds of the present article if we were to notice, however briefly, the various German Year-Books that are devoted to special subjects, and some of which—as for example the great Year-Book of Chemistry founded by Liebig and Kopp—are complete histories of the science of which they treat. There are, however, two French Year-Books of this class, of comparatively small size, that are deserving of notice, and which we can strongly recommend to the notice of our readers, namely, M. Micé's "Rapport sur le Progrès de la Chimie Organique," of which only one volume has yet appeared, and M. Vivien de St. Martin's "L'Année Geographique," of which the eighth volume, for 1869, is now lying before us. Both of these works are models of what such volumes ought to be, and show an immense amount of labour on the part of their respective editors. We should be very glad to see something like "L'Année Geographique" attempted in this country, where we have no summary of the annual progress of geographical progress, excepting the necessarily imperfect summary contained in the anniversary address delivered by the President of the Geographical Society. G. E. D.

OUR BOOK SHELF

An Elementary Course of Botany; Structural, Physiological, and Systematical. By Prof. Arthur Henfrey. Illustrated by upwards of 500 Woodcuts. Second Edition, revised, and in part re-written, by Maxwell T. Masters, M.D. (Van Voorst: 1870.)

We heartily welcome a new edition of this standard work, brought fairly down to the present state of knowledge by one of our most active and conscientious botanists. We have not yet had time to collate the present edition with the original. Cutting the pages (what an unnecessary nuisance this is in lesson-books!), no criticism worth noting occurs to us, unless it be by way of protest against the double index—one of plant-names, the other general and glossarial. This is certainly worse than letting the book into the market uncut! Had the work been new and original, other comments would not have been wanting; as it stands, we can only congratulate the editor on the very satisfactory way in which he has accomplished his work. By the way, with regard to starch, about which Prof. Henfrey was rather strong, we can imagine the sort of haze a student who had been grinding from this edition (pp. 495-496) would manifest in his paper, were he asked to state something of the origin of that substance. Not that there is any inaccuracy in the book, but rather because possession of a greater amount of preliminary knowledge than it is reasonable to look for seems to be taken for granted. Some hint might have been given as to the head-quarters of Aleurone. Lastly, we should have preferred seeing *Amphisarca*, *Tryma*, *Diplotegia*, and their kin quietly dropped out of the edition. We doubt if there be a professor of botany in the island worth his salt who could define them. D. O.

Sketches of Creation. A Popular View of some of the Grand Conclusions of the Sciences in reference to the History of Matter and of Life. By Alexander Winchell, LL.D. (London: S. Low, Son, and Marston, 1870.)

The main portion of this volume is occupied by a sketch of the geological history of the earth; and had Prof. Winchell confined himself within strictly scientific limits, the book would have been one in every respect commendable. The titles of some of the chapters are sensational and repellent, *e.g.* "The Ordeal by Fire," "The Solar System in a Blaze," "Onward through the Ages;" and we could have wished that the author had kept aloof from speculations which are, to say the least, not profitable to the class to whom the book is addressed—on the former gaseous condition of the world and the solar system; and on the possible evolution of an animal superior to man. These parts being eliminated, the book may be safely relied on as the work of a practical geologist, who has a thorough acquaintance with his subject; and being laudably free from the excessive use of technical terms, occupies a place not precisely filled by any English treatise. The illustrations are numerous, and very various in quality. The drawing of Fingal's Cave at Staffa is a grotesque caricature; with others we are familiar in well nigh every geological handbook; especially interesting to English readers are those illustrative of the gigantic scale of geological action in the United States, as the Pictured Rocks of Lake Superior, and the Mauvaises Terres of Dacotah. Two chapters "On the Vitality of buried Vegetable Germs," and "On Prairies and their treelessness," have special reference to Prof. Winchell's well-known theory that the present vegetation of the prairies of America is lineally descended from that of the pre-glacial epoch, the seeds having retained their vitality in the ground during the whole of the intermediate time. We cannot admit that the instances quoted by the author of vegetable tissue retaining its *structure* during an enormous lapse of time, when not exposed to the oxidising influence of the air, have any bearing on the question whether germs can retain their *vitality* for the same lengthened periods; as he himself says, the proof of the theory ought to rest on direct evidence: "It must be confessed that the crucial observation has yet to be made; if vegetable germs exist in the drift, they can be discovered beforehand. I am not aware that any thorough search has ever been made for them."

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

MR. Wallace's "Reply" has disappointed me. From his unrivalled knowledge of the forms of animal life in those countries where nature is the most luxuriant, and from the extraordinary interest with which he invests every subject that he handles, I had expected from him something more conclusive than that he should charge his opponent with errors which he has not committed, and should reply to his arguments by a simple begging of the question.

The first "important error" with which Mr. Wallace charges me is, that "I lead my readers to understand that there is only one completely mimicking species of *Leptalis*." Where I have done so, I am unable to discover. I have, it is true, adduced one particular and striking instance as a sample of the rest, but distinctly say that "in a comparatively small area, several distinct instances of such perfect mimicry occur;" and point out how strongly, in my view, this tells against the theory of Natural Selection. In the next paragraph, "three great oversights" are alleged. Firstly, "that each *Leptalis* produces not one only, but perhaps twenty or fifty offspring." Mr. Wallace can hardly have supposed that I imagined each butterfly laid only a single egg, like the rok. The argument, however, is unaffected. In a species the numbers of which do not materially vary from year to year, it is obvious that, whatever the number of eggs laid, only one offspring from each individual, or rather two from each pair, survive to the period at which they themselves produce offspring. The "second oversight" is "that the right variation has, *by the hypothesis*, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful." By what hypothesis? The hypothesis that these small variations in the right direction are useful to the individual—the very hypothesis against which I am contending as unproved; as neat a case of *petitio principii* as one often meets with. My "errors" in fact, amount to a non-admission of my opponent's premisses, who then naively adds, "with these three modifications the weight of the argument is entirely destroyed!" Of course it is. The "new factor of which I take no account" in the next paragraph, is again entirely dependent on the admission of the natural selectionist premisses.

With regard to the distinction between man and other animals, I much regret if I have unwittingly misrepresented Mr. Wallace's view; but if I have done so, I think it is owing to that view not having yet been clearly pronounced. Mr. Wallace distinctly states his opinion that "a superior intelligence has guided the development of man in a definite direction." ("Contributions," p. 359.) I have Mr. Wallace's own authority for saying that M. Claparède has misinterpreted him in referring this superior intelligence to a "Force supérieure," a direct action of the Creator; what alternative is there left but to suppose that it was man's own intelligence that he had in view? Whenever Mr. Wallace more clearly enunciates this portion of his theory, I think there will be no difficulty in showing that the same principle, whatever it may be, is operative in the lower creation as well as in man.

Having disposed, as I think, of Mr. Wallace's chief points of reply, I may be permitted to point out one or two errors into which he has himself, it seems to me, fallen. The changes of mimicry are, he says, "wholly superficial, and are almost entirely confined to colour." I was certainly surprised to read this, recollecting so many instances to the contrary, not only among tropical insects, but in the close approximation in form of some of our own Diptera to certain genera of Hymenoptera; and recollecting also the numerous illustrations of protective form and habit which Mr. Wallace himself gives, not only describing them but having also drawn them with such exquisite fidelity. (See "Malayan Archipelago.") In the *Kallima paralekta* of Sumatra, for instance, he says, "we thus have size, colour, form, markings, and habits, all *combining together* to produce a disguise which may be said to be absolutely perfect." ("Contributions," p. 61). Another sentence I had to read three or four times before I could believe that Mr. Wallace had penned it. In objecting to my parallelism between the development of protective resemblance and of instinct, he says, "in birds mimicry is very rare, only two or three cases being known." I do not know whether Mr. Wallace draws any subtle distinction between "mimicry" and "protective resemblance;" but if so, he should have noticed that it is the latter which I speak of as "being strongly developed in birds." I had, on reading the above sentence, to turn again to my "Contributions," to see whether I was correct in my impression that we find there the statement that "in the desert the upper plumage of *every bird* without exception is of one uniform isabelline or sand colour;" that "the ptarmigan is a fine example of protective colouring" ("Contributions," pp. 50, 51), and that two whole chapters are devoted to the wonderful protective instinct of birds in the matter of their nests.

On one point raised in my paper I am disposed somewhat to modify my views, and I do so with the greatest pleasure, in my objection, namely, to the title of Mr. Darwin's great work. Taking the origin of *species* as distinct from the origin of mere *varieties*, there is undoubtedly a sense, as Mr. Wallace points out, in which natural selection may be considered a prime factor. The law of variation is a centrifugal, the law of natural selection a centripetal force; the one acting by itself would produce a wild chaos, the other a barren uniformity: equilibrium can only be the result of their joint co-operation.

Whatever may be my "inability to grasp the theory," I hope I have shown that I have not fallen into the errors with which Mr. Wallace charges me. All the main points of the argument seem to me to be left untouched by him. He has brought forward no evidence that extremely small variations do afford any immunity from the attacks of enemies. He gives no explanation of the tendency of the *Leptalis* referred to by Mr. Bates "to produce *naturally* varieties of a nature to resemble *Ithomiæ*." He does not attempt to account for the parallelism of the development of protective resemblance and of instinct in the animal world. He fails to explain the nature of the intelligence which was operative in the creation of man, and which is a principle unknown in the rest of the organic world. Students of Nature who have spent their lives in their own country must always yield in point of experience to those who have had the advantage of comparing the faunæ and floræ of other climates, and can only arrive at their conclusions from the facts brought to their notice by travellers; these, I think, I have not misrepresented. Appeal to authority, as authority, is always to be deprecated in Science. I may, however, perhaps be permitted to strengthen my position by a quotation from a work, which I had not read at the time of writing my paper, by one who will be acknowledged to have some knowledge of the ways of Nature (Huxley's Lay Sermons, p. 323):—"After much consideration, and with assuredly no bias against Mr. Darwin's views, it is our clear conviction that, as the evidence stands, it is not absolutely proven that a group of animals, having all the characters exhibited by a species in Nature, has ever been originated by selection, whether artificial or natural."

ALFRED W. BENNETT

Westminster Hospital, Nov. 19

P.S.—Since writing the above, Mr. Jenner Weir has kindly called my attention to two papers read by him before the Entomological Society, "On the Relation between the Colour and the Edibility of Lepidoptera and their Larvæ." In one of these I find the following remarkable statement:—"Insectivorous birds, as a general rule, refuse to eat hairy larvæ, spinous larvæ, and all those whose colours are very gay, and which rarely, or only accidentally conceal themselves. On the other hand, they eat with great relish all smooth-skinned larvæ of a green or dull brown colour, which are nearly always nocturnal in their habits *or mimic the colour or appearance of the plant they frequent.*" 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?

THE soul of many an anti-Darwinian will have been cheered by Mr. A. W. Bennett's paper on "The Theory of Natural Selection from a Mathematical Point of View." It is, in fact, a very admirable piece of special pleading, based on a skilful assumption of premisses which, to a careless or biassed observer, might seem indisputable.

The tendency to variation is spoken of as something very mysterious, of which no adequate account has ever yet been given. Yet the very simple explanation is no bad one, that where two parents are concerned in the production of any offspring, the product in part resembling each of the producers must of necessity also in part differ from each of them. Between the parents themselves, Mr. Herbert Spencer has shown that differences of age and external circumstances would ensure the requisite want of resemblance in the absence of any other cause.

"The rigid test of mathematical calculation" is then applied to the case of mimetic butterflies, with the view of showing that they could not have been produced simply according to the laws of variation, inheritance, and natural selection. In the application of this rigid test the very first step is a perfectly gratuitous assumption, "that it would require, at the very lowest calculation, 1,000 steps to enable the normal *Leptalis* to pass on its protective form." Who is to prove that fifty differences would be insufficient? An interval of a thousand years might be granted for establishing each one of these variations. Suppose even 50,000, instead of only 50 steps to be necessary, it is another gratuitous assumption that "the smallest change in the direction of the *Ithomia*, which we can conceive in any hypothesis to be beneficial to the *Leptalis*, is at the very lowest one-fiftieth of the change required to produce perfect resemblance." How small a difference must decide the choice made by a donkey placed equidistant between two bundles of hay! Certainly, then, a bird on the wing, having to choose amidst myriads of butterflies, may be determined by an almost infinitesimal distinction. Further, though the whole change may be produced by an immense number of small changes, it is not necessary to suppose that all the changes will be equally small. It is merely begging the question to assume that the first change could not possibly be large enough to be of any use. And if it may be of use, the whole mathematical calculation, based on its being useless, breaks down from the beginning. Again, since the *Leptalis* may have spent 1,000,000 years in arriving at its present likeness to the present *Ithomia*, it is impossible to assert that the normal forms of the two butterflies were as wide apart at the beginning of that period as they are at present. The mimicry having once set in, might be retained by parallel variations. This, indeed, cannot fail to be the case, if the protection is to be a lasting one; for when the *Ithomia* varies in outward appearance, unless the *Leptalis* varies in the same direction, the resemblance will be lost. This progressive mimicry would be more valuable than an imitation in which no changes occurred, since the enemies of a mimetic species would in time become aware of a fraud which had no variations at its command, as birds are said now-a-days to pounce without hesitation upon caterpillars which very much resemble twigs. Even "a rough imitation" may be useful in the first instance, and yet when hostile eyes have long been exercised, and have acquired greater and greater sharpness, finally nothing less than *absolute identity* of appearance may be thoroughly effective. Thus the perfecting of the resemblance will be no "mere freak of Nature," nor shall we be "landed in the dilemma that the *last* stages are comparatively useless" in this procedure.

The array of figures brought forward to prove that the *Leptalis* could not have made twenty steps of variation in the direction of the *Ithomia by chance*, would be much to the purpose if any exponent of the theory of Natural Selection had ever argued or supposed that it could. The calculation takes it for granted that the theory is erroneous, instead of proving it to be in error. Upon this assumption, it might have been put far more strongly, only that a stronger way of putting it would have borne on the face of it the suspicion of some inherent fallacy. It begins by supposing that there are "twenty different ways in which a *Leptalis* may vary, only one of these being in the direction ultimately required;" it might quite as truthfully, or even more so, have said a thousand instead of twenty, and then the second step would have given the chance as only one in a million, instead of one in four hundred. But while the theory of Natural Selection speaks of numerous minute useful variations, Mr. Bennett will not allow that combination of terms. Let them be numerous and minute, if you will, he says, but if small they cannot be useful, if useful they cannot be small. He claims to have Mr. Darwin's own word for it, that a large variation would not be permanent, as though Mr. Darwin had said, "living creatures have come to be what they are by successive useful deviations of structure permanently propagated, but no large deviations are permanent, and no small ones are useful." It is quite obvious that in the use of relative terms, such as great and small, Mr. Darwin neither intended to stultify himself nor has done so. A thing may be large enough to be useful without being large as compared with something twenty times its own size; and a man may be said to have a huge brain in a very small body, although the body in solid content far exceeds the brain. When Mr. Darwin says that "Natural Selection always acts with extreme slowness," he does not imply that its steps must therefore be so numerous as to be too small to confer any advantage. This would be a contradiction in terms. But the steps may be exceedingly small notwithstanding, and also sometimes separated by enormous intervals of time from one another.

In introducing his own explanation of things, Mr. Bennett affirms that "resemblances, and resemblances of the most wonderful and perfect kind" in the vegetable kingdom, "are in no sense mimetic or protective." This may be so, but it can hardly be said to be proved. When he speaks of "man's reason" having "assisted him so to modify his body as to adapt himself to the circumstances with which he is surrounded," and suggests that the instinct of animals may have assisted them also to modify their bodies by slow and gradual degrees to the same purpose, it is difficult to imagine the process intended, and still more difficult to see how "the slow and gradual degrees" will escape the rigid test of mathematical calculation which Mr. Bennett has elsewhere applied; for if the steps are great they ought not to be permanent, and if small they ought not to be useful. A theory which makes it possible for a bee to "modify its proboscis" by instinct, or for a man to treat his nose in the same manner by reason, seems harder of digestion than the Darwinian.

THOMAS R. R. STEBBING

Torquay, Nov. 12

MR. BENNETT, in his very able paper read before the British Association at Liverpool, and published in NATURE of the 10th November, calls in question the explanation given by the theory of Natural Selection of the various instances of mimicry found in the animal kingdom.

He bases his argument principally on the fact that the alterations in the early stages being useless to the animal would not be preserved, and that these changes must be very slow.

He assumes that to enable the normal *Leptalis* to imitate a species of *Ithomia*, it may be considered to have gone through at least 1,000 stages, and that no change less than one-fiftieth of the whole alteration effected would be of any use to the insect. He gives us no information as to how he arrives at these figures, and we are left with the idea that they are selected principally because they are what are called "round numbers," and are more easily dealt with in the calculation which he gives us.

Now I think that the number of stages which Mr. Bennett considers it necessary for a *Leptalis* to pass through so as to mimic an *Ithomia* is vastly too great: 1,000 stages means at least 1,000 years.

Let us look at the alteration which frequently takes place in the colouring of a butterfly, possibly in one generation, as shown by varieties of which sometimes only solitary specimens are known, figured in Newman's work on English Butterflies. I need only refer your readers to the figures of varieties of *Apatura iris*, *Epinephele janira*, *Limenitis sibylla*, *Melitæa athalia*. Now can it be contended that it required 1,000 of such stages to effect the alteration?

If any of these variations happened to be useful, there seems no reason for supposing that one stage might not make much more than $\frac{1}{50}$ of the alteration, which Mr. Bennett lays down as being the least which would be useful, and which I agree with him in considering much too small. Why might not one stage make one-fourth or one-sixth of the alteration required?

Mr. Darwin quotes a passage in his work on Natural Selection (page 32) from Sir John Sebright with regard to pigeons, in which he says that it takes three years to produce a given feather, but six years to make a head and beak. If the bony structure of an animal so far above a butterfly can be altered in six years, we surely do not require more than that time to effect an alteration in the colour of a butterfly's wing.

Mr. Bennett states that the early stages of the alteration would be useless to the insect; every one, I think, will grant this, when each stage is only one-thousandth of the whole, but not if it be a much larger quantity. Here again we may observe the instance Mr. Bennett quotes, the mimicry of *Leptalis* to *Ithomia*. *Leptalis* is normally a white insect, and as such, would be more liable to attacks from its persecutors, as shown by Mr. Wallace, while any variation which gave colour to the wing would make the insect less conspicuous, and being useful to it, would be preserved.

That we are quite ignorant of the laws regulating variation is quite true, and that when we do understand them it will throw much light on these questions is undoubted, and that we may probably find in them some additional explanation for many of the facts now accounted for by Natural Selection; and Mr. Bennett does good service in the cause of truth in reminding us of what still has to be done. S. N. CARVALHO, JR.

London, Nov. 17

FOUR years ago I advanced the opinion that Natural Selection is insufficient to explain the "Origin of Species," and that, rather, the origin of the variations of which Natural Selection is said to avail itself must be looked to for this purpose. I may perhaps, therefore, be allowed to say a few words in examination of Mr. Wallace's explanation of this point in last week's NATURE.

One of the objects of Mr. Darwin has been to show that the existence of species as an absolute entity is a mere idea of our minds; that if we could at the same moment look around us in space, and also backwards in time, we should find the organic world connected together as one whole, one great mass of beings extremely closely allied to each other, and distinguishable only by an accumulation of small and perhaps scarcely appreciable differences. A second and closely-connected object has been to show that this great mass of beings has had a common origin from one primeval ancestor (or at most a few ancestors). These two points are the chief ones involved in the "Origin of Species" question, *as it is ordinarily understood;* and if they be borne in mind, it will be seen that the doctrine of "Natural Selection, or the Survival of the Fittest," deals with only a small portion of the numerous problems involved in this great question. I am sure that Mr. Wallace, after having written as he has done about man, that in his case other influences than this survival of the fittest have been at work, may reasonably allow importance to other powers than Natural Selection in the case of other organic beings.

If Mr. Darwin's book had been entitled "The Influence of Natural Selection on the Formation of Species," some misconceptions might, perhaps, have been avoided. Its present title undoubtedly tends to convey the idea that Natural Selection is *per se* the Origin of Species. I believe Mr. Darwin, however, holds no such idea.

The picture above alluded to, of a complicated mass of beings connected together by innumerable gradations, is so different from what we find existing around us, that one of the first questions suggested by it is, where are the connecting links? This first question has never yet been answered to any extent, or with anything like adequacy. The links produced are but few, and not sufficient to bear the great weight attached to them. For at no period of the geological record do we find any traces of the general and intimate connection of beings with one another that Mr. Darwin's views would lead us to look for. The creatures composing the organic world at any one given moment were, so far as the evidence of geology goes, separated from one another by lines of demarcation of similar value to those existing among animals now.

What is wanted to explain the phenomena of various limited and defined species arising from one common ancestor is, then, first, a law, or group of laws, to throw light on the origin of variation and dispersion; and, second, another law or laws to explain the limitation and separation of the varieties so produced. It is quite out of the question to suppose that the theory of Natural Selection does all this. Those, however, who have studied Mr. Spencer's work will be well aware that his theory of evolution may be applied to deal with the question in this its more extended light. And I believe that those who wish well for the survival of Natural Selection will do well to insist on its only being considered in connection with a more extensive doctrine of evolution. This is where I think Mr. Wallace errs in his advocacy.

I will not here allude to the question of mimicry more than to say, that Mr. Wallace has never answered, but rather avoided, the chief difficulties I have advanced against it; and that his theories on the subject are undoubtedly open to the objection that he insists on seeing all the phenomena from the point of view of a natural selectionist, and nothing more. As Mr. Wallace has, however, already discovered that Natural Selection, though applicable to man, is not sufficient, unsupplemented, to account for him, we may hope that he will yet see this with regard to the rest of the organic world. D. SHARP

Thornhill, Dumfriesshire

The Chromosphere

WHILST mapping down, in preparation for the coming eclipse, all the bright lines that have so far been observed and accurately measured in the chromosphere or solar prominences, I was struck with the absence of a faint yellow line, which I have myself several times observed whilst examining the contour of the sun's disc. This line is probably identical with Angström's absorption line 5883·0 (spectre normal du soleil), D″ lying almost midway between D′ and the line in question. There is no danger of mistaking it for the bright yellow line seen in every solar prominence, and lying near Angström 5865·1, since the two yellow lines were seen on each occasion at the same time on the more refrangible side of D″.

I suppose the D‴, mentioned in a late communication from Dr. Young, to be identical with the bright yellow line, for it is most improbable that he could have failed either to see or to record the bright line whilst mentioning the faint one, since the latter, as far at least as I have observed, is never visible unless in company with the former.

The only observation that I can at all identify with my own is that mentioned in NATURE, December 16, 1869, where Mr. Lockyer, speaking of the absorption line, which corresponds to the orange line of the chromosphere, says that Padre Secchi's bright line is less refrangible.

Stonyhurst Observatory S. J. PERRY

From London to Catania

A FEW practical details as to the best way of getting to Sicily, the accommodation to be found there, &c., may be of use to many readers of NATURE who are thinking of going there next month.

We have first the sea passages from London, Southampton, or Liverpool, to Messina or Malta, of which if any be chosen it will probably be that from Southampton to Malta by the P. and O. steamers, which start every Saturday at 2 P.M., and are nine days on the voyage. (Fares 20*l.* and 10*l.*) From Malta there are steamers twice a week to Messina; they touch at Catania when the weather permits them to enter the small harbour, otherwise they go on to Messina, so that passengers for Catania must in that case avail themselves of the railway.

Few probably will wish to go the whole way by sea, the land route therefore by which the Indian mails are now sent will be taken; viz., over the Brenner Pass. The night mails leave Charing Cross at 8.45 P.M., Cannon Street at 8.50 P.M., Victoria and Ludgate Hill at 8.30 P.M., and arrive in Dover in time for the Calais and Ostend boats; the line from Calais to Brussels *may* not be practicable, and so the longer passage to Ostend may be preferred; by going straight on one ought to arrive at Cologne at 4 P.M. the next day (if one goes by Calais one has three hours' rest at Brussels). The day service train, first and second-class, leaves all the stations at 7.40 A.M., and one should arrive at Cologne *via* Ostend at 10.55 P.M. (*via* Calais at 4.50 A.M. next day.) From Verviers to Cologne there are only first-class carriages in this train. The fares to Cologne by Ostend are 3*l.* 8*s.* 10*d.* first, and 2*l.* 9*s.* 5*d.* second-class, by Calais they are 3*s.* or 4*s.* more.

Those who like to go from London to Ostend or to Antwerp *direct* can leave St. Katherine's Wharf by steamer on Sunday, Tuesday, or Thursday mornings for Antwerp, or on Wednesday or Saturday mornings for Ostend, and proceed by rail to Brussels, the fares from London to Brussels being 30*s.* first and 22*s.* 3*d.* second class, *via* Antwerp; 26*s.* 8*d.* first and 20*s.* 10*d.* second class, *via* Ostend. The fare from Brussels to Cologne is about 25 francs first and 18 francs second class by the ordinary trains; express about three francs more.

The way then is by Coblentz, Mayence, Darmstadt, and Aschaffenburg to Munich. By leaving Cologne by the 6 A.M. express, one ought to get to Munich at 9.10 P.M. In times when through-tickets are granted the fare by Ostend and Cologne to Munich is 6*l.* 7*s.* 3*d.* from London (first class), and 5*l.* 10*s.*

mixed first and second; this will give some idea of what the cost will be.

After Munich the regular trains may be relied on; one can leave Munich at 9.50 P.M. (first and second class), pass Innsbruck, and crossing the Brenner in the early morning descend the Italian side in the forenoon, getting to Verona at 1.20 P.M.; or by leaving Munich at 10.15 A.M. (first, second, and third class) one may get to Verona at 5.50 A.M. next day; but in this way one misses the best of the scenery. The fares from Munich to Verona are about 37*s.*, 25*s.*, and 18*s.*

The way, then, is to Padua, Bologna, Pistoia, and so either to Florence or to Leghorn (by Pisa); from Florence to Rome, passing by Lake Trasimene; from Leghorn to Rome by the coast line, or else straight to Messina or to Naples, and then to Messina, by one of Rubattino's boats, which leave almost daily. From Rome one goes to Naples by train, and thence by boat to Messina, passing close to Stromboli. Some of the boats go on to Catania, but it is advisable to land at Messina and take the train, as often the steamer cannot get into the harbour at Catania, in which case it goes on to Malta. The railway fares from Verona to Naples come to about £4 10*s.* first; £3 5*s.* second; and £2 5*s.* third class.

By travelling *almost incessantly* one should (supposing the trains regular) get to Naples from London in five days and six nights by this route: no time is lost by spending a night at Cologne.

At Messina, the Custom House authorities are usually rather troublesome, but one does get off at last, and, passing along a most exquisite coast-line, arrives at Catania (Κατ' Αἴτνη).

The hotel at which to stay, if possible, is the Grande Albergo, kept by Herr Werdenberg, where there is every comfort, the very small salon being redeemed by the fine billiard-room. The front rooms face towards the north, and are cold and unsuited for invalids, but they (especially those of the third story) afford a most splendid view of Etna, the sunrises and sunsets seen from them being superb. The rooms at the back are much warmer, but of course give no sight of Mongibello. The Grand Hôtel Central is an Italian establishment in the Piazza dell' Università, and may be considered to be the second, though much inferior to the first-mentioned house.

In Catania itself a good post of observation would probably be the Giardino Bellini on the Corso; it is high and sufficiently large.

To go to Nicolosi a two or three-horse carriage is necessary. There are at this village two inns, one at the entrance to the village (not to be recommended), the other one, which is preferable, farther on in the village. The accommodation is of a very primitive description. Everything should be taken from Catania, as little can be got at Nicolosi.

From Nicolosi one can visit the Monti Rossi (in half-an-hour or three-quarters), from which one has a magnificent view, and one can, if one is curious enough, go down into a hole, known as the Fosse dei Palumi, a volcanic vent.

It is from Nicolosi that the ascent of Etna is made, and a description of an ascent, under especially favourable circumstances, will be found in the number of NATURE for June 23 last.

The best guide is Pietro Cravagna, who knows the mountain thoroughly, and who also speaks tolerable Italian; he is in every way to be trusted, and if another guide be necessary, it will be well to let Pietro find him. The writer was on one occasion subjected to great annoyance from the incompetence of one of the so-called guides.

The Casa del Bosco, about two-and-a-half-hours' ride from Nicolosi, is uninhabited during the winter. A fire of sticks may be made there, and a few plates, &c., will be found; the key must be got at Nicolosi; there is plenty of good water close at hand. This house might be used for purposes of observation; it has two rooms, and an outhouse for the mules.

The Casa degli Inglesi, near the top of Etna, is almost sure to be buried in the snow.

In descending from the summit it may perhaps be possible to go down into the Valle del Bove, and return to Catania by Zaffarana. Those who visit Sicily should not return home without stopping a day or so at Taormena (between Catania and Messina), and seeing a sunrise from the ruins of the theatre.

W. H. C.

The Spectrum of the Aurora

As some of your correspondents seem scarcely aware of what has already been accomplished in observation of the auroral spectrum, perhaps I may be pardoned a few remarks on the subject.

The line usually most prominent in the auroral spectrum is a yellowish green one, the wave-length of which was measured by Angström as 556·7, and its position by Professor Winlock as 1280 on Huggins' scale, which, reduced to wave-length, closely agrees with the determination of Angström.

Angström also observed the same line in the spectrum of the zodiacal light, in March 1867, but it seems possible it might be due to faint aurora concealed by the light. He says that "it is a remarkable fact that this bright band does not coincide with any of the known rays of simple or compound gases which I have as yet examined." The wave-length of H β is about 486·2; much less than that of the auroral line. Angström also saw three very feeble bands near H β (F).

Professor Winlock (*American Journal of Science*, Nov. 1869,) states that in addition to the line at 1280 Huggins' scale, he saw six faint bands, viz., at 1400, 1550, 1680, near F, 2640, and near G.

In the *American Journal of Science*, Sept. 1869, it is stated that during the solar eclipse a bright line was seen in the spectrum of the corona at 1474 of Kirchoff's scale, and that it coincided with an auroral line. 1474 Kirchoff corresponds to about 1550 Huggins' scale.

I have also somewhere seen it stated that the auroral line at 1280 coincided with a telluric line in the sun's spectrum, which might be possibly due to oxygen.

I have myself seen several feeble bands between the green line and F, but owing to their faintness have not yet been able to determine their position with much accuracy.

The red line which was so bright in the aurora of the 24th and 25th ult. is only occasionally visible. Mr. T. W. Backhouse has observed it repeatedly, and informs me that it is sometimes visible when the aurora does not appear red to the eye, but that he never recollects seeing it when some part of the sky was not red. This quite agrees with my own experience. As your correspondent, "T. F." observes, the red line probably belongs to a spectrum distinct from that of the green line, and may be due to some other gas. It may, however, be only a fresh line of the same gas due to different temperature. Its position from repeated direct comparison is about ·4 of the distance from Hα, to Na. as I stated a week or two since. It is, therefore, not identical with Hα, to which the ordinary red light of ignited hydrogen is due.

Changes of pressure and temperature do not affect the *position* of lines, but merely influence their breadth and intensity, making new lines visible and expanding old ones. Sometimes, as in the well-known case of nitrogen, an entirely fresh spectrum is produced; but while any line remains visible its position is unchanged. Hydrogen gives several such spectra, but I believe none of them have a line in the position of the auroral one.

I am at present engaged in a little research on the spectra of certain gases in relation to that of the aurora; but it is not yet sufficiently advanced for publication.

It is particularly desirable that the positions of lines should be accurately determined. In the case of the aurora I am acquainted with no better method for doing this than by comparison with such a spectrum as the band spectrum of N. This is a most convenient natural scale, with thirty or forty brilliant bands; and may readily be obtained from a small tube containing rarefied air or nitrogen, by the aid even of a Ruhmkorff's smallest coil.

With regard to the spectroscope, a simple flint glass prism fitted to a tube carrying an adjustable slit, and without any lenses, gives a brighter spectrum than any other form of instrument that I am acquainted with.

HENRY R. PROCTER

Clementhorpe, North Shields, November 12

The November Meteors

On the nights of the 12th, 13th, and 14th of November the sky was constantly watched from 5 P.M. to 7.30 A.M. The weather throughout was most unfavourable.

On Nov. 12th it was completely overcast from 7 P.M. to 7.30 A.M.

On the 13th from 5 P.M. to 7 P.M. the amount of cloud was $\frac{7}{10}$, and only one meteor was seen. The sky was then obscure until near 1 A.M. of the morning of the 14th.

Nov. 14th, from 1 A.M. to 3.50 A.M., the amount of cloud was $\frac{8}{10}$, and four meteors were seen, two starting from near γ Leonis

This was preceded by a hailstorm and rain, with occasional breaks in the clouds, through which we observed four meteors between 5 and 6.30 A.M.

On the evening of the 14th the sky was only half covered with clouds from 5 to 8 P.M., and eight meteors were observed between 5.48 and 6 40; one at 6.2 was of a brilliant red colour, with a pale greenish white train.

From 7.40 to 8.35 five other meteors were seen. The sky cleared for a short time towards 8 P.M., but at 9 a mist came on which obscured the heavens during the remainder of the night, clearing off, however, occasionally for a short time. I will not trouble you with the path of each separate meteor, though each was carefully noted. From the above observations I should be inclined to think that we had passed through the maximum during the afternoon of the 14th. Had there been any brilliant display during the night of the 14th, I think it would hardly have escaped me in spite of the mist.

Stonyhurst Observatory S. J. PERRY

SPAIN AND THE ECLIPSE EXPEDITION

THE following is a translation of a letter which appears in the *Astronomische Nachrichten* for Nov. 15, on the facilities offered by the Spanish Government to such foreign astronomers as purpose visiting Spain on the occasion of the approaching eclipse:—

"MADRID, *Nov.* 5.

"I have the honour to inform you that the Spanish Government, at the request of the Observatory at Madrid, and in accordance with the resolution taken at the time of the eclipse of the sun in 1860, has just agreed that similar measures shall be adopted for facilitating to foreign astronomers the observation of the approaching solar eclipse on the 22nd of December of the present year. The Government has in consequence resolved,—

"'That at the Spanish Custom Houses no duty or deposit shall be demanded on the astronomical or physical instruments that astronomers bring into Spain for the observation and study of the eclipse.'

"But as this privilege, which has been granted with readiness to astronomers, might be taken advantage of by persons noways connected with Science, the Government has deemed it necessary to adopt certain measures of precaution, the principal one of which is, to be made cognisant of the names of the persons who are making preparations to come to Spain to observe the eclipse. In consequence thereof, the Minister of Finance has directed 'that such astronomers as purpose availing themselves of the resolution above spoken of should have the goodness to make known in writing to the Observatory at Madrid their names, the number and the class of instruments which they bring, and the point of the coast or frontier where they purpose entering Spain.' These particulars will be communicated by the Observatory to the Government, which will send orders to the Custom-houses to pass without difficulty all the instruments entered on the lists the astronomers furnish. Foreign astronomers may, moreover, reckon on the sedulous protection of the provincial governors and of the local authorities, from whom they will receive all the co-operation necessary to enable them to devote themselves with entire liberty to their scientific labours.

"In the Almanac of the Observatory of Madrid for 1870 (which you have not received owing to the want of communication with Germany for several months) there is contained a somewhat detailed account of the approaching eclipse, accompanied by two maps. As you will observe, in the zone of the total eclipse there have been inserted all the principal towns, in order to assist astronomers in the selection of their stations for observing. This central line is not of great dimensions in Spain (about sixty nautical miles) yet, nevertheless, there are numerous important towns in proximity to the central line, as, for instance, San Lucar, Jerez, Puerto de Santa Maria, Puerto Real, San Fernando, Cadiz, Medina Sidonia, Estepona, and at those places observers will meet with all the resources requisite for carrying out their labours with facility. The sole disadvantage of so short a line is, that if the weather should prove unpropitious at one station, it will probably be so at the others as well.

"If you think any further details necessary, or in the case of any astronomer wishing to consult the map of the eclipse, nothing more will be necessary than to apply to the Director of the Observatory at Madrid, who tenders his services to such foreign astronomers as require them, and to whom it will afford great pleasure to aid his colleagues in bringing their scientific mission to Spain to a successful result. ANTONIO AGUILAR"

THE CONSTRUCTION OF HEAVY ARTILLERY

IN few other manufactures has it been found necessary to search so deeply into the materials nature provides in order to find out the best and strongest, and then to apply it skilfully, so as fully to develop its strength, as in the manufacture of guns. The construction of the amazingly-powerful ordnance which modern naval warfare employs is pre-eminently a question of strength of material; indeed, it may be termed *the* question of strength of material. In nothing else does man employ forces even nearly so powerful and violent. The force of steam, even when doing its mightiest work, is but faint and small compared with that of the exploding charge of gunpowder that sends from the gun a 300lb. or 600lb. shot with a velocity which carries it through thick armour plates of wrought iron. A 600lb. shot will pierce twelve inches of iron at 200 yards distance. This gigantic force is imparted to the shot in the brief fraction of a second that it is moving down the barrel of the gun. Remembering that "the gain in power is loss in time," and consequently that when the time is diminished the power is proportionately increased, we may form some conception how enormously great is that force which is exerted within the breech of a heavy gun, and which is resisted by it every time it is fired It is a force which, if turned into foot pounds, would represent the steam power not of a ship but of a navy. Yet all its work is to be done in the space of a few inches, and it must be surrounded with iron strong enough to resist it. Here we have the skill of man grappling with enormous difficulties, searching out the strongest and most suitable material that nature supplies, and exerting all his art to apply it to the utmost advantage. The construction of these exceedingly powerful guns has been entirely developed within the last few years. The gun now manufactured in Woolwich Arsenal is more unlike the gun of 1850 than the gun of 1850 is unlike that of Queen Elizabeth's reign. The progress of twenty years surpasses that of three centuries. And the change has not been so much in enlargement of size as in difference of construction. Queen Elizabeth's pocket-pistol is not more unlike a 600-pounder in external appearance than in internal structure. The gun which is carried in the turret of one of our ironclads, and which, at a single discharge, expends as great a weight of powder and shot as the whole broadside of a good-sized frigate of our own early days, does not surpass the gun which peeped from that frigate's ports so much in size and power as in the superior scientific principles of its manufacture. We propose in the present article to give a general view of these principles. The method of manufacture will be first explained, and afterwards the principles which guide the selection of the best material. Although the material must be selected before it is manufactured, yet a knowledge of the construction of a heavy gun, and of the qualities sought by construction to be developed, will very greatly facilitate our comprehension of the reasons of choice and preference among the many kinds of iron that might be and that are used.

In explaining the construction of modern ordnance as made for the British Government, it will be best to notice the gradual progress in the manufacture since wrought-iron began to be used instead of cast-iron. This was the first great change, and from it dates a new era in this branch of industry. And it was not only a great change, but a great advance. Wrought iron is a very much superior material to cast-iron, and one which demands very much more skill in its manufacture. Cast-iron is of a granular or crystalline nature; wrought-iron is fibrous: cast-iron is hard; wrought-iron tough. The difference between them may be illustrated by the difference between glass and wood. One is strong to resist a statical strain or pressure, the other to resist a dynamical strain or blow. There is a vast difference between the two kinds of strength. A brick which is at the foundation of a lofty factory chimney supports an enormous weight, but it would be broken by a blow that would not injure a stout walking-stick. Wrought-iron having that kind of strength which resists dynamic force

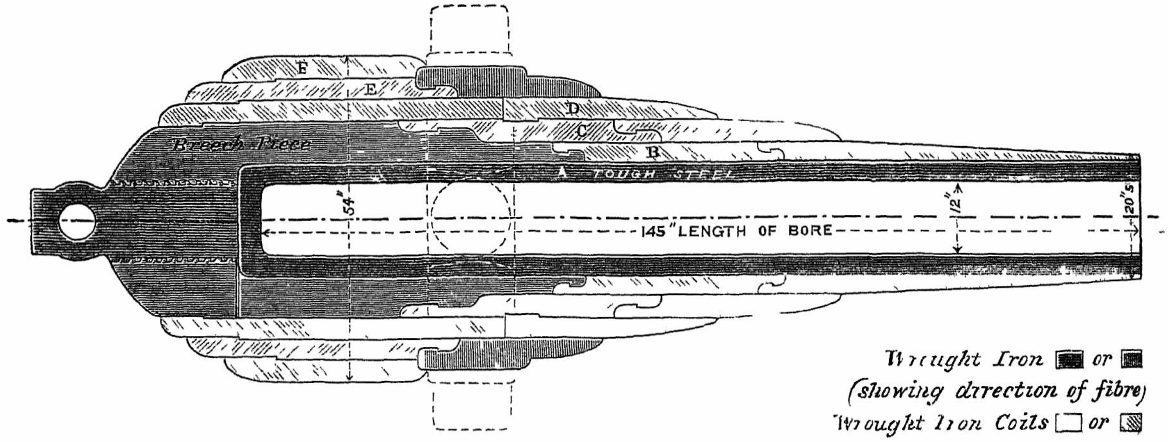

FIG. 1

is therefore far preferable to cast-iron for resisting the violent and sudden shock of explosives, the most powerful dynamic strain with which man's art has to grapple. It averages three times the dynamic strength of cast-iron, that is, it will bear three times as great weight without breaking. It will yield sooner; but when cast-iron yields it breaks. In this another great advantage is gained. When a cast-iron gun breaks it does so explosively, it breaks up into fragments, and gives no warning, no indications of yielding beforehand. But a wrought-iron gun shows when its use is becoming dangerous.

Though this discussion seems rather at variance with the plan laid down, yet it is necessary to have a general knowledge of the material used in order to understand the method of manufacture. Wrought-iron, while it is so much better a material for the construction of heavy

Scale 3/8" = 1 Foot

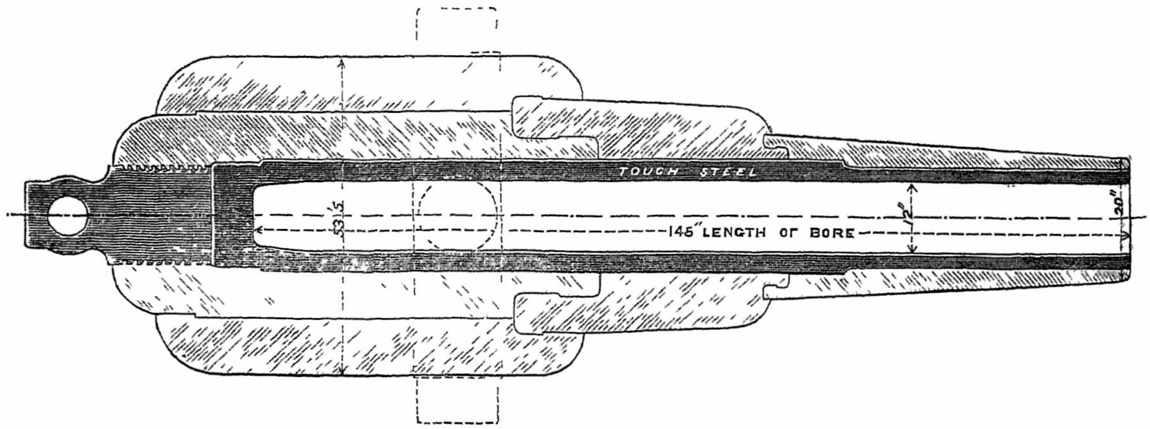

FIG. 2

guns, is yet very difficult and expensive to work. The wrought-iron gun cannot be made as easily as the gun for which the molten metal was run into a mould and then bored out and finished exteriorly. It requires large furnaces, huge steam-hammers, and skilled workmen to give it shape. Before the wonderful appliances of modern science and machinery were invented, wrought-iron could only be made and worked in comparatively small quantities. And even now to forge the mass necessary for a gun 7, 12, or 25 tons weight, would be a most difficult and costly, perhaps in the last case an impossible undertaking. No doubt there are larger forgings used in large steam ships for cranks and shafts, and in other machinery; but these masses of wrought-iron are not heated and hammered the whole at once. Separate parts are welded together, or successive portions are heated and hammered. It is needless to say that these methods would not do for the construction of a gun. The fiercely expanding gas of the exploding powder would speedily and fatally detect any plane or point of weakness. Moreover, wrought-iron is not equally strong in all directions; being fibrous in its texture, it is twice as strong with the grain as across the grain. As in the case of wood, which it is much more easy to split than to break, so it is much easier to tear the fibres of wrought iron from each other than to break them across. It therefore follows that a gun forged in a solid mass, and bored out, would not put the strength of wrought-iron at its greatest advantage. Such a gun would be very strong along its longitudinal section, very strong to resist the strain of the gunpowder to tear out the breach; but it would be only half as strong in its transverse or cross section to resist what may be specially termed the burst-

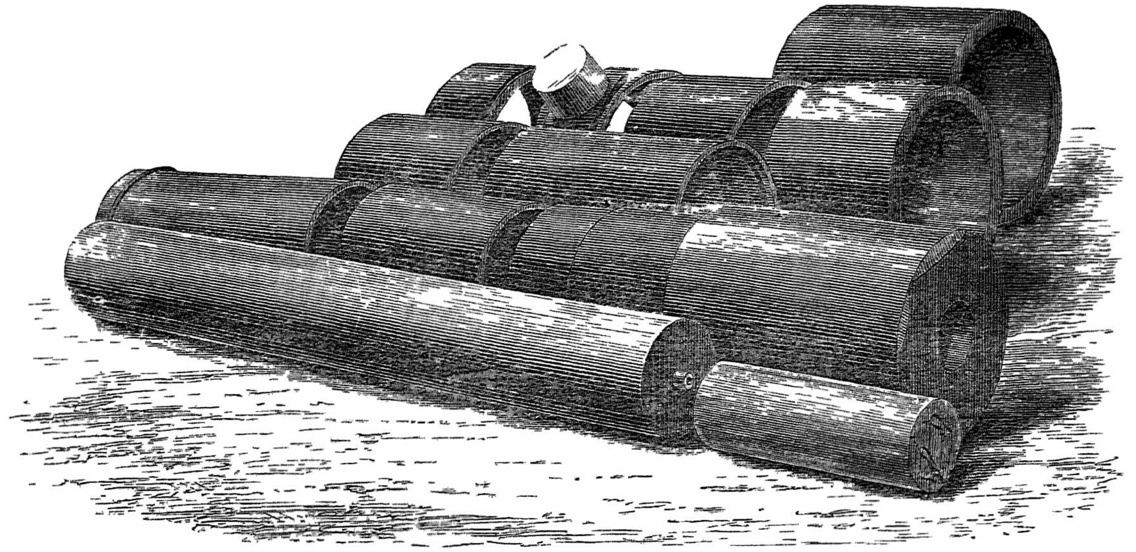

FIG. 3

ing strain. These difficulties were entirely overcome by Sir William Armstrong's system of making a gun of coils of wrought iron bars. By that the difficulty of forging a large mass is altogether put away, and the fibre of the iron passing round the bore of the gun instead of along it, gives the greatest possible resistance to the bursting strain of the powder's explosion. This was a very great advance, a most valuable improvement in the manufacture. It took away the difficulty and expense which were the great obstacles to constructing ordnance of wrought-iron, and at the same time applied it in such a manner as to increase, or rather put at the utmost advantage, its strength in resisting the transverse or bursting strain of the powder's explosion, which is the most difficult and important strain to overcome. It is for this last that this invention deserves its highest praise; for gun-making is

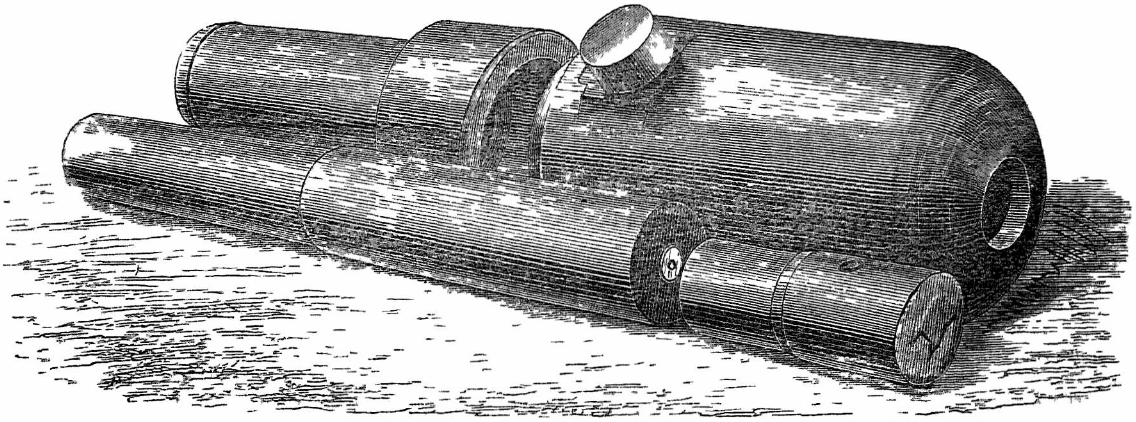

FIG.

above all a question of strength of material. The best material applied in the best way is hardly strong enough to resist the enormous charges behind the enormous shot which are required to pierce the armour-plated vessels of modern warfare.

The method of making the coils is as follows. A long bar of iron is heated to nearly a white heat in a long low furnace, and when thus rendered soft, it is hooked on to the side of what resembles a gigantic reel of iron. This reel or core is then turned by machinery, and the glowing bar is wound upon it; being drawn from the furnace upon a travelling groove, which, aided by blows on the bar from a heavy hammer suspended above and guided sometimes by two men, keeps the bar in its proper position as it is coiled. When that is completed, it is removed from before the furnace, the reel or core taken out, and the coil allowed to cool. Afterwards it is heated in a reverberatory furnace, and welded together by blows on the ends from a steam-hammer; the edges of the coiled bar are melting from heat, and therefore unite when thus forcibly pressed on each other, so that it forms a complete hollow cylinder or tube of wrought-iron, the fibre going round the circumference. The rough surfaces are afterwards turned off in powerful lathes. These coils are made of various sizes, and several of them are required for each gun. They are *shrunk on*, that is, the outer ones are not quite large enough to go over the inner ones, but are heated, and when thus expanded are placed over their smaller brethren, whom, as they cool, they clasp in a tight embrace. Thus all the coils are in a state of tension inwards, and this was supposed to increase their power of resisting the shock of the discharge which came from within outwards. However, this theory very decidedly admits of question. Even a little observation seem sufficient to show that anything in a state of tension is thereby weakened to resist a shock in any direction. A shock produces a kind of undulation or vibratory action, so that its effect returns back in the direction in which it was imparted. In Sir Joseph Whitworth's guns the hoops were made accurately to fit each other, so that no shrinking was required; but a little shrinkage, we believe, was used to ensure close fitting. It will be seen further on that this question of the advisability of shrinkage does not apply to the guns now made for the British service. Hitherto we have only spoken of coils, which, though a main part and distinctive feature of the Armstrong gun are not the whole of it. A gun made altogether of coils would lack strength in resisting the longitudinal strain, or tendency of the discharge to tear out the breech endways, and this would be an awkward event for the gunners, if the gun fired, so to speak, at both ends. To prevent this, Sir William Armstrong had a large forged piece of iron, like a great cap, placed on the breech end of the inner tube and under the coils. The fibres of this, running longitudinally, made it strong in that direction, and guarded against a catastrophe so much to be dreaded. This, however, was a large forging, and therefore very expensive; and, also, while it strengthened the gun longitudinally, it weakened it transversely, by taking up the space nearest the bore where the greatest part of the strain was sustained, and filling it with iron whose fibres were in the direction of the bore instead of around it, as those of the coils. It is easy to see how much this took from the power of the coils to resist the lateral, transverse, or bursting strain of the discharge. The force which the expanding gas exerts on the material of the gun must necessarily be inversely as the square of the distance from the centre of the bore. A coil removed from the tube by the thickness of the forged breech-piece cannot resist the full strain of the explosion nearly so effectively as if it came at once round the tube. Its strength is applied at a disadvantage represented by the ratio of the square of the radius of the coil round the forged breech-piece to the square of the radius of a coil round the inner tube.

Besides, there is another large forging in the Armstrong gun, the trunnion-piece, which is placed round the middle, and carries the trunnions or short arms by which the gun rests on its carriage.

An Armstrong gun may be thus summed up. (The section of a 600-pounder is shown in Fig. 1 as an example.) First there is a tube of steel (A); this metal is always used for the inner part, as its hardness and closeness of grain make it better adapted for the rifling—the grooves would be quickly worn by the friction of the studs of the shot in softer metal—and also better to resist the action of the violently expanding gas of the exploding charge of powder. All attempts to make the inner tube of coils were unsuccessful; the gas at its enormous pressure searched out and took advantage of the most microscopic flaws. Next comes the large forged breech-piece behind the steel tube, and extending some distance along it. Then come the coils (in 5 sets, B, C, D, E, F), shrunk on one over another. And lastly, the trunnion piece round the middle.

The Armstrong gun, as described in our former paper, was the pattern on which all our guns were made for the British service till 1866, when very important changes, which had been proposed by R. S. Fraser, member of the Institute for Civil Engineers, and Deputy-Assistant-Superintendent of the Royal Gun Factories, after a prolonged series of trials, were approved and adopted by Government. This gentleman, not long before, introduced into the manufacture of ordnance a cheaper, and, at the same time, a better kind of wrought-iron than that before used, and he has imported into the construction of the Armstrong gun very considerable modifications, by which the country is provided with a stronger gun, one-third cheaper, and more quickly made.

These are three very important items of improvement; viz. strength, cheapness, and rapidity, because simplicity, of manufacture. The saving effected is from 35 to 40 per cent. on the vast sums expended on heavy ordnance. Most of our readers have heard of the Fraser gun, but few, perhaps, know where or how it differs from the original Armstrong gun, although all our heavy ordnance is now made on this pattern. The information, therefore, may be not uninteresting, and a comparison of Fig. 2 with Fig. 1 will help to make the difference clearer. Instead of the forged breech-piece, the many small coils, and the forged trunnion-piece that form the Armstrong gun over the inner steel tube, Mr. Fraser uses one immense coil, of which the trunnions are part, and which is closed behind the tube by a large screw forming the cascable, and which is the only forging used in his gun. This will show at once how the economy is effected. Both the large forgings of the Armstrong gun, the breech-piece, and the trunnion-piece are got rid of; and instead of having many coils to be turned, and have their inner and outer surfaces reduced, upon which labour and time were expended, in addition to the waste of metal, there are only the two surfaces of the one great coil to be turned. In the 600-pounder, on the old principle, there were sixteen coils, and twice that number of surfaces, each representing labour and loss. For these reasons also the guns may be made much more quickly. This is a very important advantage, as in an emergency the country could be more quickly armed. Strength is also gained to resist the transverse strain in two ways, because the coiled iron comes next the steel tube, where the forged breech-piece used to come formerly, and so the coils are applied at greater advantage, and secondly, because the one thick coil is stronger than several thin coils, just as a triple deal is stronger than three inch deals. And further, the gun is stronger to resist the longitudinal strains, because the breech and trunnions are all of one piece, and so the force of the discharge upon the gun acts through the trunnions on the carriage, and has not, as in the old pattern, a tendency to destroy it by tearing one piece or part of the gun from another. It is converted from a longitudinal bursting strain into recoil.

It only remains to describe how this immense coil, which is the marked feature of the Fraser gun, is made. A long and thick bar, much thicker than the one used in the Armstrong pattern, is heated and coiled in the manner before described. When this has cooled, another bar, somewhat longer, is coiled upon it in an opposite direction, that is, if the first coils go from right to left, the second go over them from left to right, just as the boa constrictor overlaps his coils on the prey which he is crushing. And then a third bar is coiled in the same direction as the first. The whole is then heated in a large reverberatory furnace, and a few blows from a powerful hammer weld them into a thoroughly combined mass.

This principle of construction seems to apply the iron to the utmost possible advantage in resisting the force of the exploding charge. There is an eloquent testimony to the excellence of the system in one of the first guns made on the Fraser principle, which was tested to destruction in the preliminary trials that took place before the system was adopted, and is now to be seen in the *cemetery*, or place where such guns are preserved for inspection in the Royal Arsenal. This gun, a 64-pounder, having fired a greater weight of powder and shot than any other of its own size, and latterly with charges increased till it was destroyed, burst in this way: part of the tube, which was worn through, and the coil round the front of the tube came out and left the entire mass of the trunnion and breech-piece uninjured, so that not only would this bursting have done no injury to those who served the gun, but if a new tube and fore-part were put in, the trial might have commenced again.

Welding a coil, however large, is a much easier and less expensive process than forging and hammering into shape a mass of iron of much smaller size. However, the great size of the coils of Fraser guns of large calibre necessitated the employment of correspondingly large furnaces and machinery. These difficulties have been very successfully overcome in the Royal Gun Factories. The furnaces have been enlarged from a cubical content of 60 feet to 600 feet. At present a gun is being made of 35 tons weight, which will hurl a shot of 700lb. weight with a charge of 120lb. of powder (the battering charge for the ordinary 25-ton 600-pounder being 70lb.) All the coils for this enormous weapon have been welded without accident or hindrance. In one case as much as 28 tons of iron have been heated in one piece in the furnace, seized by the tongs, and placed in a glowing mass beneath the hammer. This is an achievement unprecedented in iron manufacture, and which reflects the highest credit on this most important Government department. Nowhere else, and for no other purpose, have such gigantic masses of metal to be heated and manipulated.

Figs. 3 and 4 show the parts of an Armstrong gun, and of a Fraser gun, before they are put together. Both are 300-pounders, and the engravings have been made from photographs of the actual guns.

NOTES

WE are in a position to state that the arrangements of the Eclipse Expedition are rapidly progressing,—thanks to the untiring labours of the strong Organising Committee, which meets almost daily. As we stated before, the Government are bringing all their power to bear in favour of the work, and, should the weather be favourable, we may expect such a series of observations as has never been made of an eclipsed sun. As at present arranged, there will be four parties. Beginning with Spain, we have one to Cadiz, in charge of the Rev. S. J. Perry, and one to Gibraltar, under Captain Noble. The English branch of the Anglo-American Expedition will be under the charge of Mr. Lockyer; while there will be a fourth small expedition, under the charge of Mr. Huggins, to Oran; the Cadiz, Gibraltar, and Oran parties will leave Portsmouth on the 5th of December in the *Urgent*. The Sicilian party will leave London on the night of the 7th by the Brenner pass, a ship of war meeting them at Naples. Although not a single official astronomer has olunteered to go, there will be lack of neither skill, discipline, nor organisation; and arrangements are already being made which will ensure a full and early publication by the Organising Committee of the scientific results obtained. Printed instructions are being prepared by the Committee for each class of observations. So much for the English Government Expedition. With regard to the American one, we may add that it has been no less strongly and carefully organised, with the distinct advantages that astronomy is more cultivated in America than it is here, that the official observatories are fully represented, and that as all the observers were present at the Eclipse in 1869, they therefore may be regarded as veterans. Professors Young, Pickering, Newcomb, Peters, Watson, Harkness, and others are at present in London, and are daily affording most valuable information to the Organising Committee and the various observers.

THE following memorial to Her Majesty's Government on the danger to which the scientific, literary, and art collections of Paris are now exposed, has been forwarded to the Earl of Granville from the University of Dublin:—"We, the undersigned, Provost, Fellows, and Scholars of Trinity College, and Professors of the University of Dublin, desire to express our satisfaction with the efforts made by Her Majesty's Government to restore peace in Europe, and our earnest hope—shared, we believe, by the nation at large—that these efforts may be eventually successful. But if, unhappily, our desire should not be realised, your memorialists venture to urge that the interposition of Her Majesty's Government may be directed to preserve, if possible, the great scientific, literary, and art collections of Paris, which are, in truth, the property of the whole civilised world. It is impossible to contemplate calmly the irreparable loss which the destruction of these collections, or even any serious injury to them, would inflict upon students of every nation. To avert, if possible, such a calamity, is now the duty of all; it is more especially the duty of every scientific and literary institution. Your memorialists would, therefore, in the name of our ancient University, earnestly entreat Her Majesty's Government to interpose their good offices with the belligerents, for the purpose of saving these matchless treasures from a danger which the fate of the Library of Strasburg proves to be only too real."

WE understand that Dr. Neil Arnott, in addition to his recent munificent donations to the Universities, has just presented 500*l.* to the Aberdeen Mechanics' Institution, to aid in maintaining lectures in Physical Science.

AT the examination for Foundation Scholarships, held in the week after Easter, 1871, one or more scholarships will be obtainable by proficiency in the Natural Sciences, at Trinity College, Cambridge. Should one scholarship only be so assigned, preference will be given to the candidate who shows the greatest proficiency in physiology and the allied subjects. The Examination in the Natural Sciences is open to all undergraduate members o the Universities of Oxford and Cambridge. The value of the scholarship is about 80*l.* per annum for five or six years.

DR. MICHAEL FOSTER (the newly-appointed Prælector of Physiology at Trinity College, Cambridge) commenced on the 14th inst. his course of lectures in a part of the new museums, which has been temporarily fitted up as a Physiological Laboratory. He gave a lucid and able exposition of the three great factors of life—contractility, as evinced chiefly in muscles; irritability, as evinced chiefly in the nervous system; and secretion. Dilating upon the much-vexed question, how far these are attributable to physical agencies, or are to be referred to another agency called "Life," he compared the latter view to a fortress closely besieged by an able band of investigators who are ever narrowing its area, and pressing the physical forces closer and closer upon it. But it has not yet capitulated. No one has a right to say that it will or will not capitulate; and till it has done so we are perfectly justified in regarding it as an entity, as a something to be taken into account in the investigation and the attempts at the explanation of living processes. He should still, therefore, use the term without committing himself to either view. He gave definitions of Physiology and Morphology. He spoke of the enormous importance of vivisection to the advance of Physiology. By it Vesalius might be said to have laid the foundations of Physiology; by it Harvey had been enabled to obtain the proofs of his great discovery. Without it all that had been written on Physiology would have gone for very little, and we should still have been in the Aristotelean mists darkened by the theories of the Schoolmen. He wished, however, to state that in the teaching of Physiology it would be necessary for him to resort to it much. He stated the plan he intended to pursue in carrying out the intentions of those who had placed him in that honourable position. Lectures he did not regard as a very fructifying mode of sowing seed. He thought it far better that men should work and see for themselves. With the munificent aid of Trinity College, he hoped, ere long, to make the physiological laboratory in Cambridge one of the best working laboratories in the country. He intended to have practical classes in addition to the lectures; and students who were competent would have opportunities for private work. It would be a labour of love to him to render practical aid to those who needed it, and to promote the study of physiology by every means in his power.—A considerable number of the senior members of the University were present, as well as undergraduates, and warmly applauded at the close of the lecture. The lectures are for the present open to all members of the University without fee.

AT the recent examination for the Natural and Experimental Science Moderatorship at Trinity College, Dublin, Gold Medals were awarded to R. Apjohn, W. F. Burton, and T. F. Fleetwood (*Sch.*), and a Silver Medal to R. Barrington. The subjects examined in were—1. Physics; 2. Chemistry; 3. Mineralogy and Geology; 4. Palæontology, Zoology, and Botany. No candidate was allowed to present himself for examination in more than two of the four branches.

REV. PROF. HAUGHTON, M.D., F.R.S., has commenced a course of Lectures in Trinity College, Dublin, on Physical Geology, and Prof. Macalister, M.D., a course on the Anatomy, Physiology, and Classification of the Mollusca.

BY the resignation of Mr. J. J. Bennett, the office of Keeper of the Botanical Collections at the British Museum is now vacant. The appointment rests actually with three only of the trustees, the Archbishop of Canterbury, the Lord Chancellor, and the Speaker of the House of Commons, by virtue of their offices. Among the whole body of the trustees, who would naturally be consulted, there are only four scientific men, the President of the Royal Society, Sir R. J. Murchison, Lord Enniskillen, and Sir Philip Grey Egerton. Now that we are about to remove our national collections to a new building erected for the purpose, the suggestion naturally arises whether this is a condition of things which is desirable to perpetuate. The whole subject of the mode of appointments to these Government offices is one well worthy of the consideration of the Royal Science Commission. Mr. Bennett entered the Museum in 1829 as assistant, and succeeded the late Robert Brown as Keeper at his death in 1858. Mr. W. Carruthers, the present senior assistant, on whom the appointment would naturally fall, and who is so well known for his researches in vegetable palæontology, entered on that office in 1859.

THE first course of Cantor Lectures of the Society of Arts for the ensuing Session will be "On Artists' Colours and Pigments," by Frederick S. Barff, M.A., F.C.S., and Fellow of the Cambridge Philosophical Society. It will consist of five lectures, to be delivered on Monday evenings, the 21st and 28th November, and the 5th, 12th, and 19th December, at eight o'clock. These lectures will treat of—the Nature of Colour; Chemistry and Manufacture of Colours and Pigments; Vehicles and Media used in Painting; Fresco and Silicious Painting; Destructive Influences on Colours, &c. Other courses of lectures are under arrangement for delivery during the Session. These lectures are open to members, each of whom has the privilege of introducing two friends to each lecture.

THE *Gardener's Chronicle* states that an interesting exhibition of fruit has been recently opened at Appenzell, Switzerland. Eight communes have furnished 689 exhibits, comprising 80 sorts of apples and 120 of pears. The fruits are arranged according to the height above the sea of the localities where they are grown. Thus, in the lowest zone are shown fruits which have been produced from 1,300 to 2,000 feet above the sea; in the next, those grown at an elevation of 2,000 to 2,600 feet; in the third group, those gathered at a height of 2,600 to 3,000 feet; and lastly, are exhibited fruits produced above the last-mentioned elevation. Great care has been taken to ensure the accurate nomenclature of the fruits exhibited.

A REMARKABLY low wave of temperature passed over these islands in the middle of this month. At Blackheath the mean temperature for the week ending Nov. 16 was nearly 7° below the average. It is remarkable that the wind was in the W.S.W. during nearly the whole of the week, the air being almost saturated with moisture, and yet the rainfall scarcely appreciable, 0·04 in. For the fourteen stations in England, eight in Scotland, and one in Ireland, recorded by Mr. Glaisher in the *Gardener's Chronicle*, the lowest minimum was 19·0° at Paisley, the highest, 31·5°, at Norwich. The mean temperature was nearly the same in Scotland as in England, about 37·5°. Another singular meteorological phenomenon occurred this week in the successive thunderstorms which burst over London from 3 A.M. on Tuesday morning the 22nd to 6 A.M. on Wednesday morning the 23rd. The wind was blowing strongly from the S.W. during the whole time, with occasional violent rain, and the average temperature was about 40° F.

THE Address on Medicine at the annual meeting of the British Medical Association will be delivered by Dr. George Johnson, and that in Surgery by Prof. Lister.

THE current number of *Fraser's Magazine* contains the first portion of a paper on "Mystic Trees and Flowers," which will interest those who have paid any attention to the subject of Tree-worship, with regard to the origin of which no clear theory has yet been proposed. The writer considers that the religious homage paid to trees "must be referred to a distinct religious phase in the development of races, and to a period later than the ideals and myths with which poets invested them." The legends and superstitions of all countries are brought together, showing the points of convergence of the great religions of ancient races; and the connection of the folk-lore of the present day with its prototypes in all ages and in all nations. Reference is made to the recent researches into the history of the popular tales of different countries, and the whole paper teems with suggestive facts. The principal trees dwelt upon in this instalment are the apple, oak, ash, lime, willow, palm, elder, and juniper.

THE *Engineer* states that when the Russian American telegraph is completed the following feat will be possible. A telegram from Alaska for New York, leaving Sitka, say at 6.40 on Monday morning, would be received at Nicoleaf, Siberia, at six minutes past one on Tuesday morning; at St. Petersburgh, Russia, at three minutes past six on Monday evening; at London twenty-two minutes past four on Monday afternoon; and at New York at forty-six minutes past eleven on Monday forenoon. Thus, allowing twenty minutes for each re-transmission, a message may start on the morning of one day, to be received and transmitted the next day, again received and sent on the afternoon of the day it starts, and finally reaches its destination on the forenoon of the first day, the whole taking place in one hour's time.

FOR the purpose of connecting the Madras Observatory with the midnight and noon guns, the Indian Government has voted 200*l*.

MUSICAL INTERVALS

A BRIEF summary of the remarkable papers on musical intervals, by MM. Cornu and Mercadier, published in the *Comptes Rendus* of February in last year, may perhaps interest those of the readers of NATURE who have not met with the original.

The authors remark, in the first place, that two schemes of musical intervals have been proposed, in which the ratios of the number of vibrations in a given time are as follows:—

	Octave.	Fifth.	Fourth.	Major Third.	Minor Third.	Sixth.	Seventh.
(1)	2	$\frac{3}{2}$	$\frac{4}{3}$	$\frac{3^4}{2^6}$	$\frac{2^5}{3^3}$	$\frac{3^3}{2^4}$	$\frac{3^5}{2^7}$
(2)	2	$\frac{3}{2}$	$\frac{4}{3}$	$\frac{5}{4}$	$\frac{6}{5}$	$\frac{5}{3}$	$\frac{15}{8}$

and the object of the paper is to examine the claims of each for adoption.

On comparing these two systems, it may be observed that the Octave, Fifth, and Fourth are the same in both, and that the other intervals are connected by the following relations:—

$$\frac{3^4}{2^6}=\frac{5}{4}\times\frac{81}{80}\quad \frac{2^5}{3^3}=\frac{6}{5}:\frac{81}{80}\quad \frac{3^3}{2^4}=\frac{5}{3}\times\frac{81}{80}\quad \frac{3^5}{2^7}=\frac{15}{8}\times\frac{81}{80}$$

A third view of the subject, according to which either of the two systems may be adopted indifferently, because they differ only by a "comma" (an interval represented by 81 : 80), may be at once dismissed, since, as a matter of fact, the ear is capable of appreciating intervals much smaller than the comma.

As regards the scheme (2), it seems impossible not to admit that a major third is most harmonious when the resultant tone is exactly the double octave below the fundamental note, *i.e.* when the interval is represented by the ration 5 : 4. Any deviation from this proportion produces unpleasant beats. This argument seems decisive in favour of (2).

On the other hand, an ear which hears successively the notes emitted by an entire string, and by $\frac{4}{5}$ths of its length, will pronounce the third so formed to be too low. And, in fact, stringed instruments for concerted music are tuned by perfect fifths. Thus the intervals given by the violoncello, alto, and violins, &c., in a concerto would be c, g, d', a', e''.

But this involves a third (c, e) defined by the ratio $\frac{3^4}{2^6}$; for e must be the double octave below e'', and the interval (c, e'') is by hypothesis $\left(\frac{3}{2}\right)^4$; hence

$$(c, e)=\frac{1}{2^2}\left(\frac{3}{4}\right)^4=\frac{3^4}{2^6}$$

which is the ratio for a major third according to scheme (1).

Thus, experiment apparently gives contradictory results.

The authors of the paper then proceed to describe a series of experiments made with the voice, violoncello, violin, organ-pipes, and monochord, all of which lead to conclusions reconciling this apparent contradiction, viz.:—

That musical intervals do not belong to any single scheme, but that the ear is capable of distinguishing between the intervals of the two schemes in question, and requires,

(*a*) When notes are heard in succession, forming what is called melody, that the intervals should belong to a series of fifths, in accordance with scheme (1);

(*b*) When notes are heard simultaneously, forming chords, or harmony, that the intervals should be adjusted according to scheme (1).

The details of the experiments, which are well worth study, would be too long to give in *extenso;* but the subjoined table will enable the reader to form a judgment of the results.

	Major Third.		Fifth.	
Notes produced by	Harmony.	Melody.	Harmony.	Melody.
Voice	—	1.260	—	1.497
Violoncello	1.251	1.266	1.499	1.508
Violin	1.249	1.264	1.504	1.504
Organ pipes	1.252	1.267	1.493	1.497
Monochord	—	1.271	—	1.500
Mean of observation	1.251	1.266	1.499	1.501
Calculation	$\frac{5}{4}$=1.250	$\frac{81}{64}$=1.2656	$\frac{3}{2}$=1.500	1.500

The direct experiments were made with these two intervals only; but the same conclusions are shown to apply to the other intervals.

The authors then proceed to inquire whether there is any reason for limiting the prime numbers which enter into the ratios of the harmonic intervals to those (1, 2, 3, 5) actually occurring in scheme (1). An answer to this question is found in the chord of the dominant seventh, usually defined as the common chord with the addition of a minor third (*e. g.* Do, Mi, Sol, Si♭). The ratios of these intervals, according to scheme (2) are

$$1:\frac{5}{4}:\frac{3}{2}:\frac{3}{2}\times\frac{6}{5}=20:25:30:36$$

and the simplest whole numbers near to these are 4 : 5 : 6 : 7; and these, it is argued, are in fact an improvement on the former. For the ear, which alone can decide the question, will choose those notes which will form a chord devoid of beats, and whose difference tones do not introduce any notes foreign to the chord itself. Now

$$7-6=1,\ 6-5=1,\ 5-4=1$$
$$7-5=2\ \ 6-4=2$$
$$7-4=3$$

So that from the chord 4 : 5 : 6 : 7, we obtain the group of difference tones 1, 2, 3, all of which belong to the natural series terminating with the chord itself. While from the chord 20 : 25 : 30 : 36 = 4 : 5 : 6 : 7·142. we at once derive an inharmonious difference tone. This *a priori* conclusion may be verified on the violin, by first tuning the two upper strings in unison; then by shortening one of them so as to form a minor third (6 : 5) with a difference tone 1; and finally, shortening the other until the difference tone 1 is again heard. This will, of course, give an interval (7 : 6) perfectly agreeable when sounded simultaneously, but not so when sounded in succession. In the same way the ear might be called upon to decide whether the numbers 11, 13, &c., are or are not admissible in harmony.

I trust that this very brief abstract may induce some of your readers to examine the paper itself.

W. SPOTTISWOODE

ON THE GREAT MOVEMENTS OF THE ATMOSPHERE*

MY original paper was based on the mean monthly pressures calculated for 516 places, and on the mean monthly direction of the wind calculated for 203 places over the whole surface of the earth. From these mean pressures the monthly isobars were drawn for every tenth of an inch, a pressure of thirty inches and upwards being represented on the charts by red-coloured isobars, and pressures of 29·9 inches and less by isobars coloured blue. Thus the distribution of the mass of the earth's atmosphere from month to month was graphically represented, the red lines showing

* This paper, presented to the recent meeting of the "British Association" at Liverpool, is a brief *résumé* of a paper, and the discussion which followed, "On the Mean Pressure of the Atmosphere, and the Prevailing Winds over the Globe for the Months and for the Year," originally read before the Royal Society of Edinburgh, and published in the Transactions of that Society in the beginning of December last.

where there was an excess, and the blue lines where there was a deficiency. The general results are that, in each hemisphere, pressures are highest in winter, and lowest in summer; that in winter the highest pressures are grouped over the continents, and in summer the lowest, and that in winter very low pressures prevail in the northern parts of the Atlantic and Pacific oceans respectively. The position of the isobars is wholly determined by the relative distribution of land and water.

As regards wind, those places were selected which are favourably situated for observing its direction. In calculating the average direction, the element of force was wholly excluded. The years were, so far as possible, the same as those for which the mean pressure had been calculated; but where this was not possible, care was taken to see that a good average was in every case obtained. The mean direction of the wind for each month is shown in the charts by arrows.

Thus two quite distinct facts were exhibited on the charts, viz. lines showing the mean pressure of the atmosphere, and arrows showing the prevailing winds at the earth's surface, each being independently arrived at by the summing and averaging of observed facts. What relation is there between these two classes of facts?

1. *As regards regions of Low Pressures:*

In every case where such occur at any season, it is universally seen that the relations of the winds to the isobaric lines is exactly the same that is illustrated in every storm of wind when the winds and pressures are set down in synophic charts.—The relation is this; the wind neither blows round the centre of least pressure in circles, nor as tangents to the concentric isobaric curves, nor does it blow directly towards that centre, but it takes an intermediate direction, approaching however more nearly to the direction and course of the circular curves than of the radii to the centre. The angle is not a right angle but from about 60° to 80°. Thus the whole system of winds seem to blow vorticosely in upon the spaces of low pressure. This is the relation known as *Buy's Ballot Law of the Winds.*

2. *As regards regions of High Pressures:*

In all cases where such occur, the winds are seen to flow out in every direction. In these cases also, the behaviour of the winds differs in no respect from what occurs on particular days on which the isobaric lines present the same conditions of pressure. The winds flow out of these spaces of high pressure in courses exactly the opposite to what takes place when they flow in upon spaces of low pressure, and hence such meteorological phenomena have been called by Mr. Francis Galton, "Anticyclones." This is also, it will be seen, in strict accordance with Buy's Ballot Law. Hence then this broad result is arrived at, viz., that the prevailing winds over the globe at all seasons obey Buy's Ballot Law, with reference to the distribution of atmospheric pressure.

The *outflow* of the air from a region of high pressure, and the *inflow* upon a region of high pressure is reducible to the single principle of gravitation, and so marked is this, that if there be any other force or forces which put the winds in motion, they must be altogether insignificant as compared with gravitation.

The annual averages of the 115 places distributed over the north temperate zone were minutely examined with the view of ascertaining how far the commonly-alleged prevalence of equatorial and polar currents is borne out by observation. The result of the analysis is this:—There are two maximum directions of prevailing winds at the stations of which the S.W. and N.E. at Greenwich may be taken as an example. The chief prevailing winds of the north temperate zone blow from some point from S.S.W. to W. (the true equatorial direction) at 41 per cent. of the stations, leaving 59 per cent. at which they are from other points of the compass. And the secondary prevailing winds come from some point from N.N.E. to E. (the true polar direction) at 34 per cent. of the stations or only a third of the whole. Hence whilst the largest percentage of prevailing winds are in the direction in which truly equatorial and polar winds should blow, the percentages from the direction are so large as to preclude the supposition of a general flow of the surface winds of the temperate zone towards and from the polar regions. The truth is it is not the poles of the earth, but the regions of high and low pressures which must be regarded as the true poles of the winds towards which and from which the great movements of the atmosphere proceed; and owing to the unequal distribution of land and water, the poles of pressure and movements of the atmosphere are, as in the case of the poles of temperature, very far from being coincident with the North Pole.

The causes which bring about an unequal distribution of the mass of the earth's atmosphere are chiefly two—viz., the temperature principally, and secondarily the moisture of the atmosphere, their relations to the geographical distribution of land and water. The charts show that where there is an excess of temperature, as in the interior of Asia in summer, and where there is a relative excess of moisture, as in the belt of calms and in the north of the Atlantic in winter, and where there is an excess of cold, as in the interior of Asia in winter, and where there is a deficiency of moisture—there atmospheric pressure is high. Hence where pressures are low there the air is specifically light, and where pressures are high there the air is heavy relatively to that of surrounding regions.

Further, since vast columns of air are poured by the prevailing winds into those regions where pressures are low without increasing that pressure, we must suppose that there is an outflow from these regions through upper currents, and this inference is all the more inevitable, seeing that the specifically light air resting over these regions supplies the conditions of an ascending current. Again, since vast volumes of air are driven off from the regions of high pressure without diminishing the pressure, it must be inferred that the high pressure is maintained by accessions poured in upon it by the upper currents, and this inference is the more certain because in such regions the air is relatively heavier than in surrounding regions, thus supplying the conditions of a descending current.

It is evident that the currents from the regions of low pressure will continue to ascend as long as their pressure is less than that in regions surrounding them *at the same height*, but that as soon as they reach a height where the pressure is less, towards and over that region will they flow as upper currents of the atmosphere The courses of these upper currents will be directed towards those regions where the air is coldest and driest near the surface of the earth, because being thereby densest, the great mass of the air will condense in the lower beds, thus leaving less air or a diminished pressure in the upper regions. Thus the high winter pressure of Asia will be maintained by air being poured upon it by upper currents from the regions of low pressure in the north of the Atlantic, the north of the Pacific, and the Equatorial regions of the south.

From these considerations the following *Theory of the Movements of the Earth's Atmosphere* necessarily follows. The winds on the surface of the earth are known from the isobaric lines, the direction being from regions of high towards regions of low pressures, subject to the changes produced in the direction of the currents by the earth's rotation; and the upper currents of the atmosphere may be inferred from the isobaric lines taken reversely together with the isothermal lines taken directly. In other words, the regions of lowest pressure, by giving the ascending currents, point out the sources or fountains whence the upper currents flow; and the isothermal lines, by showing where, on account of the low temperature, the greatest portion of the air is condensed in the lower beds and so diminishing the pressure in the upper beds, point out the regions towards and over which the upper currents diffuse themselves.

In this discussion of the prevailing winds and mean atmospheric pressure over the globe, there is contained the first approximate answer to the two questions:—

1. What are the motions of the earth's atmosphere?
2. What are the causes of these motions?

It has been shown by Dr. Balfour Stewart* that these questions form the first two stages in the development of meteorology. Considering the importance of the subjects of this discussion which enter deeply into Physical Geography, Geology, the Science of Navigation, and the General Physics of the globe, it is to be hoped that observation will be made and turned to account in rectifying the isobaric lines and filling up the blanks of the winds over the ocean, and portions of Africa and South America, data from these regions being in this as well as every similar discussion to a large extent wanting. ALEX. BUCHAN

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, November 15.—Prof. W. H. Flower F.R.S., V.P., in the chair. An eighth letter was read from Mr. W. H. Hudson, containing further observations on the Ornithology of Buenos Ayres.—Mr. Sclater exhibited a specimen

* See NATURE of December 2, 1869, p. 129.

of the new Australian Mudfish, recently described by Mr. Krefft in the Society's "Proceedings" as *Ceratodus forsteri*. This specimen had been obtained from the Mary River, Queensland, and forwarded to Mr. Sclater by Mr. E. P. Ramsay.—Dr. J. Murie read a memoir on the form and structure of the Manatee (*Manatus americanus*), as deduced from a fresh specimen of this animal forwarded to this Society in a living state by Mr. G. W. Latimer, of Porto Rico, in April 1866, but which had unfortunately died just before reaching Southampton.—A communication was read from Mr. Morton Allport, relating to the progress of the experiments for introducing Salmon and Trout into Tasmania.—Professor Flower read a memoir on the anatomy of the Panda (*Ailurus fulgens*), as deduced from a specimen of this animal which had been presented to the Society by Dr. Simpson, in May 1869, and had lived for some time in the Society's Gardens. After an elaborate examination of every part of this animal, Professor Flower came to the conclusion that it belonged to the Arctoidean group of the Carnivores, and was most nearly allied to the Raccoons and other members of the family *Procyonidæ*. Mr. Bartlett read some notes chiefly on the habits of the same animal, as observed when living in the Society's Gardens.—A communication was read from Dr. J. E. Gray, containing the outlines of a new arrangement of the Delphinoid Whales.—A communication was read from Mr. W. Harper Pease, containing remarks on the Mollusks of the genus *Triphoris* and descriptions of some new species.—Mr. A. G. Butler communicated a notice of some abnormalities observed in the neuration of the hind wings in *Acræa andromacha*.—A paper was read by Dr. A. Günther containing descriptions of two new species of Lizards of the genera *Eumeces* and *Calotes*, proposed to be called *E. brachydactylus* (from Pegu) and *C. jerdoni* (from the Khasya hills).—A paper was read by Messrs. Sclater and Salvin on the recent collections of Venezuelan birds made by Mr. A. Goering in the vicinity of Merida. The present collection was stated to embrace examples of 105 species, nine of which were considered to be new to Science. Amongst the latter were two new Parrots, proposed to be called *Urochroma dilectissma* and *Conurus rhodocephalus*.—A communication was read from Mr. H. Adams, containing descriptions of 27 new species of Shells collected by Mr. R. McAndrew in the Red Sea. A second communication from Mr. Adams contained descriptions of a new genus and four new species of Shells from Borneo and other localities.

Anthropological Society of London, November 15.—Dr. Charnock, V.P., in the chair; H. R. Adam, and David [illegible] were elected fellows. Dr. R. H. Bakewell read a paper on "The Condition of the Blood-corpuscles in certain Races." The author's researches had been undertaken at the request of Dr. Barnard Davis, F.R.S. While investigating the pathology of malarious fevers, the writer made numerous microscopical examinations of the blood, both of the sick and the healthy, in Trinidad. On comparison of the various races of English, French, Portuguese, Italians, Germans, Indians, Chinese, Africans (both indigenous and of West Indian birth), and Creoles of various breeds, he found that, besides the differences produced by disease, there were well-marked differences between the various races. The corpuscles of the flesh-eating Mussulman and the vegetable-feeding Hindoo were contrasted, and it was found that the Hindoo's blood contained a much larger number of white corpuscles, the red corpuscles also differing in form from those of the Mussulman. The author coined the word "nummulation" for the phenomenon observed in the aggregation of the corpuscles like *rouleaux* of coin. He gave the results of the examination of the blood of about a hundred different persons.—Mr. C. Staniland Wake, Director A.S.L., then read a paper on "Tribal Affinities among the Aborigines of Australia." After tracing the distribution of the several forms of native habitations, canoes, and weapons, and also of certain burial customs and initiatory rites, and referring to the apparently universal practice of cannibalism and blood-revenge, the writer stated, firstly, that the phenomena presented by the generality of the western natives are, on the whole, of a more simple character than those exhibited by the other aborigines agreeing with the milder disposition they apparently possess; secondly, that the southern and eastern natives agree generally in their customs with the aborigines of the western part of the continent, but that they present certain peculiarities which seem to suggest an internal influence. This influence must have proceeded from the north, and there the very customs of phenomena which constitute the differentia between the natives of the west and those of the east are met with. Comparing the distribution of initiatory rites with that of other customs, it would seem that scarifying the flesh and nose-perforation are, like the use of the semi-circular hut, primitive customs at one time common to all the aborigines; that the custom of removing a front tooth is almost limited to the canoe-making peoples of the north, south, or east, who also possess in common certain funeral rites unknown to the western natives; whilst circumcision is limited to the northern and southern tribes and to some intermediate ones. The final conclusion of the paper was, that the western natives represent the most primitive and simple form of the Australian stock, the natives of the south and east having been intermixed with fresh comers from the north, who introduced new customs. This southward movement was in two directions—one across the continent to the head of the Great Bight, thence spreading east and west along the coast; the other along the north and eastern shores, and gradually spreading over the eastern portion of the continent.—Dr. Robert Peel contributed a communication on Australian Aborigines and Half-Castes, and exhibited skulls (which he had presented to the Society's Museum) in illustration.

Chemical Society, November 17.—Prof. Williamson, F.R.S., President, in the chair. The following papers were read:—"Mineralogical Notices," by Prof. Maskelyne and Dr. Flight. The contents of this communication were—1. On the formation of basic cupric sulphate. In 1867 M. Pisani described a mineral which he supposed to be the Woodwardite of Mr. Church. The substance, however, is not the latter mineral. It had previously been examined in the Laboratory of the British Museum, and the results sufficiently tallied with those of M. Pisani to identify the mineral. The only interest this mineral offers is in the light it seems to throw on the possible modes of the formation of native basic cupric sulphates. The actions of solutions of magnesium or calcium sulphate on malachite may terminate in the production of langite. An experiment in the laboratory showed that an insoluble cupric sulphate and acid magnesium carbonate were actually formed. 2. Opal from Waddela Plain, Abyssinia. Mr. Markham presented to the British Museum some remarkable specimens of green opal from the above locality, Its analysis showed it to consist of about 92 per cent. of silica, 6 per cent. of water, and the remainder was iron, manganese, calcium, and magnesium. 3. Francolite, Cornwall. Its analytical numbers point to the formula

$$5\left.\begin{matrix}[3\,Ca,\ 2\,(PO_4)]\\ Ca\,CO_3\end{matrix}\right\} + 2\,Ca\,F_2$$

It is, in fact, a fluor-apatite, in which one equivalent in every six of the calcium phosphate is replaced by carbonate. The crystallography of this mineral seems also to point to its not being ordinary apatite, and in fact to its not being the same mineral as the original francolite from Wheal Franco. 4. Epidote and serpentine, Iona. A pebble in which a green mineral traverses bright red felspar and quartz in veins was sent by the Duke of Argyll to the British Museum. Its analysis leads to the view that it consists of a lime epidote with some 23 per cent. of quartz, the former mineral having the following constitution:

$$3\,[2\,Ca\,O,\ Si\,O_2] + 2\,[2\,(\tfrac{3}{5}Al\ \ \tfrac{2}{5}Fe)_2\,O_3,\ 3\,Si\,O_2]$$

Two specimens of serpentine from the same locality gave the general formula:

$$3\,R''\,O,\ 2\,Si\,O_2,\ 2\,H_2\,O,$$

where R represents in one case Mg, with a little Fe and Ca; in the other Mg, with nearly one-fifth of its equivalent of an equal mixture of Fe and Mg. 5. Vivianite. Two kinds of this mineral were found in an unknown Cornish locality. The one is of a pale, bluish tint, the other of a brownish colour. Both proved to be octahydrated ferrous orthophosphate, $3\,Fe\,O,\ P_2\,O_5,\ 8\,H_2\,O$, and the difference in the colours can only be ascribed to some minute difference in the degree of oxidation of the iron. 6. Cronstedtite. The analysis of this mineral presented considerable difficulties, inasmuch as it was extremely difficult to free it from the substances with which it is found associated. The cronstedtite in question possesses an unusual interest from a crystallographical point of view, being one of the best defined types of hemimorphism. 7. Pholerite. This mineral, derived from India, is of a pale flesh white, penetrated in several places by patches and veins of a black mineral. A new name was proposed for this mineral, but the analysis shows it to be nothing but pholerite. Mr. Church observed that it was a matter of congratulation to have those beautiful specimens which are stored in our magnificent national collections investigated in so excellent a manner as the contents of the paper just read have shown.—The next communication was a note by Mr. Chapman "On the Oxides of Nitrogen." In a paper read at the last meeting of the Chemical Society, Mr. Chapman mentioned that he had quantitatively estimated nitric oxide by converting it into nitric acid, and determining the latter by the production and weighing of the baryta salt. Objections were then raised as to the possibility of the completeness of such a conversion. Mr. Chapman now endeavoured to show by referring to well-known chemical facts that whether $N_2 O_5$, $N_2 O_4$, or $N_2 O_3$ be formed when $N O$ is left with excess of oxygen over water, the final result must be the transformation into nitric acid. Mr. Harcourt reasserted that on his passing nitric oxide into oxygen he obtained as result nitric peroxide; when reversing this order and passing oxygen into nitric oxide, a mixture of $N_2 O_4$ and $N_2 O_3$ seems to be formed. Mr. Chapman replied that the different results obtained by Mr. Harcourt and by himself were in all probability due to differences of the temperatures at which the respective experiments had been executed. Prof. Williamson took occasion of this repeated mentioning of nitric peroxide to remark that this compound may be viewed as $\left.\begin{matrix}N O\\ N O_2\end{matrix}\right\} O$, *i.e.*, as water in which the one hydrogen was replaced by $N O$, the other by $N O_2$.

Linnean Society, November 17.—Dr. J. D. Hooker, V.P., in the chair. Notes on the *Passifloreæ*, by Dr. M. T. Masters. The paper treated of the morphology of the whole order, including the organography of all the genera; the minute anatomy, development, mode of fertilisation, and the movements which take place in the stamens and pistils of *Passiflora*; the affinities of the order, together with an inquiry into the value of characters, and the mode of estimating them; a complete list of the genera and species, with detailed descriptions of all the species which are not either American or African, and which will be published elsewhere; and, lastly, an account of the geographical distribution. (1.) Organography.—The tendrils, as shown by their development, minute anatomy and position, and the fact that they occasionally bear flowers, are merely modified flower-stalks. The leaf, whether single or divided, always commences in development at the apex, and proceeds from above downwards. There are only three bracts instead of five, as there would be in a perfectly symmetrical arrangement; their position, as well as that of the other parts of the flower, relative to the axis has been somewhat incorrectly described by Payer, Griffith, and Schleiden. Jussieu and St. Hilaire held that there is no true corolla in the passion-flowers, but two calycine whorls, because both organs fall at the same time. But the mode of development and the internal structure clearly demonstrate that the inner whorl is a true corolla; the calyx is quincuncial, while the petals appear simultaneously. The flower-tube is, according to Bentham, composed of a union of the calyx and corolla; Dr. Masters, on the other hand, believes it to be an expansion of the axis. Its development is comparatively late. The form of the corona was traced from its simplest form in *Turnera* to its most complicated arrangement in some Modeccas and Passifloras, in all cases it is a mere projection from the flower-tube, and is of late development, and morphologically of little importance, though essential to the individual life of the plant. The inner portion of the tube is a glandular secreting surface. Each separate thread of the corona has its own distinct vascular bundle. The stamens, unlike those of *Cucurbitaceæ*, are not perigynous, but truly hypogynous. The gynophore becomes gradually developed, raising the stamens with it. The anthers are invariably two-celled. The pistil is singularly uniform, one-celled, made up of three united carpels, with three parietal placentæ and three stigmas. (2.) Mode of Fertilisation.—The arrangement of the sexual organs favours cross-fertilisation. The anthers, originally introrse, become, when fully developed, distinctly extrorse, and it is thus rendered difficult for the pollen to fall on the stigmas of the same flower; it falls on to the rays of the corona, on which insects alight in search of the honey concealed at the base of the tube, and carry the pollen away to other flowers. Some species are more easily fertilised by pollen belonging to a different species than by their own; hence hybridisation abounds. (3.) *Affinities*.—Dr. Masters connects the *Passifloreæ* rather with the *Turneraceæ*, *Samydaceæ*, *Violaceæ*, and *Sauvagesieæ* than with the *Cucurbitaceæ*, chiefly on account of their truly hypogynous stamens. (4.) Geographical Distribution.—The order is essentially tropical, occurring between 30° N. and 30° S. latitude. Taking the genus *Passiflora* as the type, it is almost exclusively American, and chiefly Brazilian; a few outliers of the true passion-flowers occur in Madagascar and Mauritius, the latter probably introduced, and in North America there is a very remarkable form, the *P. incarnata*, or original passion-flower of the Jesuits; it is an annual, and apparently an alien, but remarkably uniform and invariable; its representative in Brazil is *P. edulis*, a shrub, and an extremely variable plant. On the western side of the Andes you get entirely different forms, especially the *Tacsonias*, which have generally a much longer flower-tube. In India there are a few species, not belonging to the American section of the order, but to the *Polyanthea*, which are found also in Ceylon and the Indian Archipelago, and one outlying species in an island aear Hong Kong. In Australia and the Pacific Islands occurs another perfectly distinct group; in New Zealand a perfectly distinct form is met with; and, again, another in Africa, where no true passion-flower is indigenous, except the one in Madagascar; the species here, few in number, belonging to six or eight distinct genera.—"On the White-beaked Bottle-nosed Whale," by Dr. Jas. Murie. The paper gave an account of several anatomical points in the structure of this whale, which had not been clearly described before. The little bag or sac connected with the double articulation between the lower jaw and the rest of the skull, which is found in all Mammals except the Cetacea, is however, present in the fœtus of the latter.

Mathematical Society, Nov. 10.—Prof. Cayley, President, in the chair. The following gentlemen were elected to form the council for the Session 1870–1:—President, W. Spottiswoode, F.R.S.; Vice-Presidents, Professors Cayley, Henrici, and H. J. S. Smith; Treasurer, Dr. Hirst; Hon. Secs., M. Jenkins and R. Tucker; other Members, W. K. Clifford, T. Cotterill, M. W. Crofton, *C. W. Merrifield, *J. F. Moulton, J. Stirling, Archibald Smith, Prof. Sylvester, J. J. Walker. Mr. J. J. Hamblin Smith, M.A., Caius College, Cambridge, was proposed for election. The new president having taken the chair, called on Prof. Cayley to read his paper, of which an abstract is given below—"Sketch of Recent Researches upon Quartic and Quintic Surfaces." The classification of quartic surfaces is, even as to its highest divisions, incomplete; and it is by no means easy to make it at once exhaustive and precise; an examination of all the *prima facie* possible cases would include forms which do not really exist. Premising that the expression "singular" means double or cuspidal, or refers to a higher singularity; "nodal" in general double (including the signification cuspidal), we may provisionally arrange the non-scrolar quartic surfaces as follows—1, without a nodal curve; 2, with a nodal line; 3, with a nodal conic, or line-pair; 4, with three nodal lines (not in the same plane) meeting in a point); 5, the quartic scrolls, omitting altogether the torse and cones. For the scrolls the division into twelve species appears to be complete (Memoirs by Cayley (3) (4), Cremona, and Schwarz). As regards non-scrolar surfaces, we have—1. Without a singular curve. The surface may be without a cnicnode (conical point), or it may have any number of cnicnodes up to 16 (Cayley, (5); Kummer (1) (2),). It may be remarked that the wave-surface, or generally the surface obtained by the homographic deformation of the wave-surface, called by Cayley (1) (2) the "tetrahedroid," is a special form of surface with 16 nodes. 2. Quartic surface with nodal line. Considered incidentally in *Clebsch* (2) (3). 3. Quartic surfaces with nodal conic. Such a surface may be without cnicnodes, or it may have one, two, three, or four cnicnodes. The cases other than that of three cnicnodes are mentioned, *Kummer* (1); but the question is examined and the remaining case of three cnicnodes established, *Cayley* (6). The general case of the nodal conic without cnicnodes is elaborately considered, *Clebsch* (1). See also *Geiser*; the several cases of one, two, three, and four cnicnodes are considered, *Korndorfer*. In the case where the nodal conic is the circle at infinity the surfaces have been termed "anallagmatic" (perhaps "bicircular" would be a more convenient name), and a great deal has been written on these surfaces by Moutard, Clifford, and others. The theory of the quartic surfaces with a cuspidal conic has been hardly at all considered, briefly referred to in *Cayley* (6), also in *Cayley* (8). "I do not know that anything has been done in regard to the quartic surfaces where the nodal conic becomes a line-pair, that is, where we have two intersecting nodal lines." 4. Quartic surface with three nodal lines (not in the same plane) meeting in a point. This is in fact *Steiner's* quartic surface, and it has been the subject of numerous investigations. The author then

* These gentlemen were not on the Council for the Session 1869–70.

proceeded to speak of the paper, *Kummer* (1), which discusses the cases in which a quartic surface has upon it a system of conics; or what is the same thing, in what cases there is a system of planes each intersecting the surface in two conics. It is, in the first place, remarked that there is no proper quartic surface cut by every plane in a pair of conics or even a proper quartic surface cut in a pair of conics by every plane through a fixed point. The cases considered are:—(I.) where the planes are non-tangent planes: (II.) where they are single tangent planes; and (III.) where they are double tangent planes (*Steiner's* surface, where every tangent plane meets the surface in a pair of conics, comes under II.). It was then pointed out that several of the papers by *Clebsch* and others, refer in their titles to the "Abbildung" of a surface, viz. they show that a (1, 1) correspondence exists between the points of a surface and the points of a plane. For surfaces of the higher orders it is only certain surfaces which admit of an "Abbildung," or (1, 1) correspondence of the points thereof with the points of a plane; viz. a surface, in order that it may thus correspond with the plane, (or say in order that it may be unicursal), must have a sufficient singularity in the way of a nodal or cuspidal curve. In the memoir *Clebsch* (3), after explaining the above method of the transformation of a cubic surface by means of two of the lines thereof, the author goes on to notice that the like method is applicable to certain quartic and quintic surfaces, viz. (1) quartic surfaces with a nodal conic; (2) quartic surfaces with a nodal line; (3) quintic surfaces having a nodal skew cubic (the nodal skew cubic may break up into a conic and line which meets it, or into three lines, two of them not meeting each other, but each met by the third line; and the like theory applies to these quintic surfaces). The memoir by *Korndorfer* relates to the "abbildung" of a quartic surface having a nodal conic, and one, two, three, or four cnicnodes. *Clebsch* (4) relates to the "abbildung" of a quartic scroll. "As regards quintic surfaces (not being scrolls), we have, so far as I am aware, only the paper *Clebsch* (3) relating to quintic surfaces with a nodal Skew Cubic; the paper *Clebsch* (5), which relates to the 'abbildung' of a quintic surface having a nodal quadriquadric. It only remains to speak of *Schwarz's* memoirs on Quintic Scrolls: it is to be remarked that the theory of scrolls is allied more closely with that of plane curves than with that of surfaces, viz. considering any plane section of the scroll the lines of the scroll have in general a (1, 1) correspondence with the points of the plane section, and the scrolls of any given order are properly arranged according to the deficiency of the plane section. This is what is done in the memoirs by *Cremona*, and this is the principle of classification in *Schwarz's* memoir. A model of Steimer's surface was exhibited, and many of its properties pointed out. The following list of memoirs will indicate the sources whence the sketch was principally drawn. Memoirs by the author: (1) Sur la Surface des Ondes. Liouville, tom. xi. 1846. (2) Sur un cas particulier de la Surface du quatrième ordre avec seize points singuliers. Crelle, tom. lxv. pp. 284—290. (3.) Second memoir on Skew Surfaces otherwise Scrolls. Phil. Trans. vol. cliv. (1864), pp. 559—577. (4.) Third memoir. Phil. Trans. vol. clix. (1869), pp. 111—126. (5.) A memoir on Quartic Surfaces. Proc. of London Math. Society (1870), vol. iii. pp. 19—69. (6.) On the Quartic Surfaces $(*\;)(U, V, W)^2=0$, Quarterly Journal of Math. tom. x. (1868), pp. 24—34. (7) Do. tom. xi. pp. 15—25 (1870). (8.) Memoir on Cubic Surfaces. Phil. Trans. vol. clix. (1869), pp. 231—326. *Cremona*: Sulle superficie gobbe di quarto grado. Mem di Bologna, tom. viii. (1868). *Schwarz*: Ueber die geradlinigen Flächen fünften Grades. Crelle, tom. lxvii (1867), pp. 23—57. *Clebsch* (1) Ueber die Flächen vierter Ordnung welche eine Doppelcurve zweiten Grades besitzen. Crelle, tom. lxix. (2.) Intorno alla rappresentazione di superficie algebriche sopra un piano. Atti di R. Ist. Lomb. (1868), 13 pages. (3.) Ueber die Abbildung algebräischer Flächen insbesondere der vierten und funften Ordnung. Ann. Clebsch und Neumann, vol. i. (1868), pp. 253—316. (4.) Ueber die ebene Abbildung der geradlinigen Flächen vierter Ordnung welche eine Doppelcurve dritten Grades besitzen. Ann. Clebsch und Neumann (1870), pp. 445—466. (5.) Ueber die Abbildung einer Classe von Flächen, 5 Ordnung. Gött. abh. tom. xv. 64 pages. *Gesser*: Ueber die Flachen vierten Grades welche eine Doppelcurve weiten Grades haben. Crelle, tom. lxx. (1868), pp. 249—257. *Korndorfer*: Ueber die ebene Abbildung Clebsch und Neumann, tom. ii. *Kummer*: (1) Ueber die Flächen vierten Grades auf welchen Schaaren von Kegelschnitten liegen. Berl. Monatsber. Jul. 1863. Crelle, tom. lxiv. (1864), pp. 66—76. (2) Ueber die algebräischen Strahlensysteme ins besondere über die der ersten und zweiten Ordnung. Berl. abh. (1866), pp. 1—120. (3.) (Surfaces of the 4th order with sixteen conical points). Berl. Monatsber. (1864), pp. 246—260, and 495—499.

Meteorological Society, November 16.—Mr. C. V. Walker, President, in the chair. Mr. J. H. Gilbert, Mr. C. R. Marten, Mr. F. E Sawyer, and Mr. T. H. Wilson were elected Fellows. A paper was read by Mr. G. Dines, "On Evaporation and Evaporation Gauges, with some remarks upon the Formation of Dew," in which, after referring to Dr. Dalton's investigations, he explained the experiments in which he has been engaged during the past eighteen months with gauges of different sizes, the experiments being made sometimes with water of ordinary temperature, and at other times with heated water and also with water artificially cooled, in the open air and in a closed room. Mr. Dines then gave the results of these experiments, one of which confirmed the statement of Mr. Glaisher in 1847, viz. that "the formation of dew was found to depend solely on the temperature of the bodies upon which it was deposited, and that it never appeared upon them till their temperature had descended below that of the dew-point in their locality." Finally, he stated that the conclusions which he had arrived at were, 1st. The greatest cause of evaporation is the movement of the air. 2nd. Whatever tends to increase the temperature of the air increases evaporation, and *vice versâ*; and 3rd, That which tends to lessen the temperature of the dew-point, increases evaporation, and *vice versâ*. Mr. Glaisher then made a communication respecting the November meteors, giving the results of the watches which were kept during the nights of the 12th-13th, 13th-14th, and 14th-15th. On the latter evening fifty-three meteors were observed. He also made some remarks on the great magnetic disturbances which occurred during the brilliant display of Aurora Borealis on the nights of October 24th and 25th. The President then adjourned the meeting till January 18, 1871.

MANCHESTER

Literary and Philosophical Society, October 4.—Rev. Wm. Gaskell, Vice-president, in the chair. Mr. Boyd Dawkins gave a short account of the work done in the Victoria Cave, near Settle, since the last notice brought before the Society. The two layers containing traces of man were separated at the entrance by a talus of fallen stones, seven feet thick, that gradually coalesced as the excavation passed into the cave, and at last became so confused together as not to be easily distinguished at a few feet from the entrance. The remains of a gigantic bear which had been eaten may probably be assigned to the lower horizon, which furnished flint-flakes, and a bone harpoon in form resembling that used by the natives of Nootka Sound; the upper or Romano-Celtic stratum continued to supply evidence of the comparatively late date of its accumulation in barbarous imitations of coins of Tetricus (A.D. 267-273.) A portion of the ivory handle of a Roman sword and a coin of Trajan have also been found, along with large quantities of the bones of animals that had been used as food. Several spurs of cocks proved that the inhabitants ate the domestic fowl, which was probably imported into this country either directly or indirectly by the Romans. The most striking object, however, is a beautiful sigmoid fibula made of bronze, and ornamented with a beautiful pattern in red, yellow, green, and blue enamel. It is an admirable example of the art of enamelling ("Britannicum opus"?) which the Celtic inhabitants of Britain probably taught their Roman conquerors.

October 18.—Mr. E. W. Binney, President, in the chair. Prof Balfour Stewart, F.R.S., exhibited a series of sun-spot curves projected from results obtained by himself and Mr. De La Rue, from observations of Schwabe, Carrington, and the Kew series of photographs of the sun. These extend over a term of about forty years, and exhibit a principal and secondary maximum and minimum in each solar spot period of eleven years, thus corresponding with the light curves of R Sagittæ observed by Mr. Baxendell, and β Lyræ by Prof. Argelander. Hence it may possibly be that notwithstanding the darkening of the sun's surface during the maximum spot period, the total light and heat emitted by the sun at this period is really greater than at the times of minimum spot frequency.—Mr. Lockyer, F.R.S., gave an account of his recent spectroscopic investigations of the solar atmosphere, and pointed out that the conclusions arrived at by De La Rue, Stewart, and Loewy, confirmed the views to which he himself had been led by spectroscopic observations of the sun during the last two or three years. These tended to show that the absorbing atmosphere, termed the chromosphere, which he had proved to exist round the sun's body, had gradually diminished in thickness since the last solar spot minimum in 1867.—Mr. Boyd Dawkins, F.R.S., gave a short account of the examination of Offa's Dyke made in the autumn by Col. Lane Fox and himself. The portion examined extended from Cherbury in the south to the abrupt range of limestone hills to the north of Llanamynech. At Nantcribba Hall, near Forden, the dyke passes nearly due north between the road to Montgomery and the abrupt boss of volcanic trap which looks at a short distance like a ruined castle, and which has been encircled by a very broad and deep moat. There can be no doubt but that this was a point of observation, and as it is but some twenty yards on the English side of the dyke, it was most probably one of the permanent positions occupied by the English followers of the Mercian King. From this point the dyke gradually swerves to the east from the road between Montgomery and Buttington, and makes directly over the low slopes of the hills, in some places being nearly ploughed down, and in others, and especially in the small valleys, being of considerable height and resembling a railway embankment, until it reaches the higher ground of Fron. Thence it runs through Pentre and gradually approaches the road, and finally dies away in the alluvium of the Severn, nearly a quarter of a mile to the south of Buttington Church. The commanding camp to the south of this portion of the line is Caer Digol, or the Beacon Ring, on the top of the Long Mountain. The morass, which in Offa's time must have extended between the main ditch and the Severn, prevented the necessity of any bank being made between Buttington and the Cefn. Where, however, the open country demands a defence to the north of Cefn, an embankment makes straight for the Greenstone ridge of the Garreg, and is very plainly seen close to the farm-house of that name, near the Trewern Gate. Here we lost our clue, and it is very likely that the steep ridges of Moel y Golfa, and the marvellously strong camps of the Breiddan and Middleton Hills, formed a sufficiently strong barrier without any dyke being raised. We picked it up again, however, on the western or Welsh side of the Severn, from which it runs, as shown in the Ordnance map, due north to the four crosses, where it joins the Oswestry road, and where it is cut across by the new railway. There it makes straight for the fortified hill of Llanamynech, its line coinciding with the high road. On reaching the summit of Llanamynech it takes the western or Welsh side of the two large camps, and passes down into the valley to the south of Whitehaven, which was the limit of our expedition. The results of our examination are the direct proof that the dyke was made for military purposes, and that it took the line which was best adapted for repelling the incursions of the Welsh. Throughout the district which was examined the embankment faces Wales, and was therefore made to defend the country within it from the Welsh. Dr. Wright's view, therefore, that it was a mere geographical boundary to prevent the Welsh from stealing the cattle of the Mercians cannot be maintained, although it may perhaps receive some confirmation from the nursery legend of Taffy. The camps in the neighbourhood of the dyke are probably older than Offa's time. The bronze spears found in Llanamynech imply that the camp is not later than the bronze age, while the Roman coins in that of the Breiddan point to its occupation by the Romans.

November 1.—Rev. William Gaskell, Vice-president, in the chair. Mr. William H. Johnson, Mr. Walter Morris, and Professor Balfour Stewart were elected ordinary members of the Society. Dr. Joule exhibited a series of curves obtained by Dr. Stewart from the self-recording instruments at the Kew Observatory, showing a large amount of disturbance of the magnetic declination and horizontal force during the progress of the aurora of the 25th October. He also showed a curve of the changes which took place in the magnetic dip as observed by himself at Broughton. The most remarkable variation occurred during the interval from $6^h\ 15^m$ to $6^h\ 23^m$ G.M.T., when the dip increased from $69°\ 8'$ to $70°\ 30'$.—"Notes on Glacier Moraines in Cumberland and Westmoreland," by Mr. Brockbank, F.G.S. The author referred to the proceedings of the Geological Society of London for 1840—1, which contain notices of the evidences of glaciers having existed in Great Britain, by Professor Agassiz, Dr. Buckland, and others, and which point out (1) "Moraine-like Masses of Drift," which occur near the junction of the Eamont and Lowther with the Eden, near Penrith; (2) The "large and lofty insulated piles of gravel in the valley of the Kent near Kendal, and the smaller moraines and their detritus, which nearly fill the valley from thence to Morecambe Bay;" (3) "Similar mounds near Shap," and (4) the "Gravel mounds near Milnthorpe and thence to Lancaster." Of these the author considered the Kentmere Group, near Kendal, as most nearly fulfilling the conditions required in true glacier moraines, and that in the other cases it admitted of doubt whether they were really due entirely to glacial action. The districts more particularly the subjects of the author's notes are (1) the valleys of Eskdale and the Duddon (which were not visited by Dr. Buckland, but in which he supposed moraines to exist, from the appearances of the valleys as delineated in Fryer's map of Cumberland); (2) the valleys eastwards from Bowfell; and (3) the district of Shap Fells.

DIARY

THURSDAY, NOVEMBER 24.

LONDON INSTITUTION, at 7.30.—On the Precious Metals and their Distribution: Prof. Morris

ROYAL SOCIETY, at 8.30.—Note on the Pendulum Observations in India: Col. J. T. Walker, F.R.S.—The Theory of Resonance: Hon. J. W. Strutt.

SOCIETY OF ANTIQUARIES, at 8.30.—Romano-Celtic Sword exhibited by Lord Wharncliffe: Mr. A. W. Franks.—Wall Decorations of the Roman Period in Sectile Work, especially in Glass: Mr. A Nesbitt.

SUNDAY, NOVEMBER 27

SUNDAY LECTURE SOCIETY, at 3.30.—On the Antiquity of Man: Dr. Cobbold

MONDAY, NOVEMBER 28

LONDON INSTITUTION, at 4.—On Chemical Action: Prof. Odling

INSTITUTE OF ACTUARIES, at 7.—On Legislation in reference to Life Insurance and Life Insurance Companies: Mr. T. B. Sprague, M.A.

TUESDAY, NOVEMBER 29

GEOGRAPHICAL SOCIETY, at 8.30

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY, at 7.

WEDNESDAY, NOVEMBER 30.

SOCIETY OF ARTS, at 8.—On Peat and its Profitable Utilisation: Robert M. Alloway

THURSDAY, DECEMBER 1.

CHEMICAL SOCIETY, at 8.—On some Derivatives of Anthracene: Mr. W. H. Perkin

LINNEAN SOCIETY, at 8.

LONDON INSTITUTION, at 7.30. On Gems and Precious Stones: Prof. Morris

SOCIETY OF ANTIQUARIES, at 8.30

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.

CONTENTS

PAGE

ERRATA.—Page 28, second column, line 10, for "cumulus" read "nimbus." Page 54, first column, line 25 from bottom for "Mr. Care" read "Mr. Earl."