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THURSDAY, JUNE 20, 1872

LIVINGSTONE

DR. LIVINGSTONE is one of those men, becoming scarcer now in these nervous days of hurry and excitement, who do what they put their hands to with all their might. He went to Africa to discover certain regions then unknown, and especially to determine the extent and character of the great catchment basins on the eastern side of the continent. His object was not solely, or even chiefly, the advancement of geographical knowledge. In his eyes geography is only a means to an end. He hopes, through an extension of the knowledge of the interior of Africa, to call forth a spirit which may be the means of securing the great objects of his life—the extinction of the slave trade, and a permanent improvement in the condition of the negro race.

Some six years ago Dr. Livingstone landed at the mouth of the Rovuma, and disappeared from the knowledge of European seekers for news. Then there came a wild report of his murder, and staunch old Sir Roderick sent out an expedition, under Mr. Young, to Lake Nyassa, which successfully performed its mission, and gave us the assurance that the report was false and that Livingstone was alive. All this while the great traveller was toiling steadily at his appointed task, and had completed the solution of one great geographical question, namely, that of the northern limits of the basin of the Zambesi river. Another long period elapsed, and once more a letter was received from Ujiji, on the banks of Lake Tanganyika, announcing the progress of the work. Having cleared up the problems relating to Lake Nyassa, Livingstone had ascended highlands which form the water-parting between the Zambesi and another great system of rivers and lakes to the north. He had been in a land where the vegetation was saturated with moisture—a land unlike all previously-conceived ideas of this part of Africa. The work was beginning to tell upon him. He described himself as a mere "bag of bones." But he gave no sign of faltering in his purpose. His great discovery was not half achieved, and the time for rest was still distant. His will was unsubdued; his life-work must be completed before he could turn aside to be refreshed; and thus he disappeared again.

Years passed away—first one, then another and another, and for a third time the anxiety of the country began to increase. For Britain still cares for and watches over her great sons. The indomitable yet unostentatious resolution of this grand old man has touched the heart of the nation to its very core. Sir Roderick Murchison died in the full hope and expectation of soon receiving tidings of his friend. No truer nor more steadfast friend ever lived; and the news of Sir Roderick's death will be the saddest words that Livingstone has heard since he lost his brave wife in the wilds of the Zambesi. Then it began to be felt that it would be wrong to wait longer. Our patience was exhausted; an appeal was made to the country which was warmly and munificently answered; Lieut. Dawson left this country in command of a search expedition, reached Zanzibar, and proceeded without delay to make preparations for his march into the interior.

The rest of the story must be gathered from the telegrams which have arrived from Bombay and Aden within the present week. News, it seems, came down to Zanzibar last May that Livingstone was alive, that he had reached Kazeh, on the road between Lake Tanganyika and the coast; but that he declined to return home until his work was completed. In those years of enforced silence, during which his letters had been intercepted by Arab slave traders, he had been working hard. He had completed one more great discovery; but still the work was not all done, and he would not come home. All honour to this man of iron will and unchanging purpose!

The second great discovery of Livingstone, since he landed at the mouth of the Rovuma, is more important, if possible, than the first. His first discovery was the north-eastern water-parting of the Zambesi. His second, the tidings of which arrived by telegram last week, is the limits of the great basin of Lake Tanganyika, and that a vast and separate system intervened between the basins of the Nile and the Zambesi. The discovery of the basin of Tanganyika, extending from about 3° to 10° S. latitude, and 27° to 39° E. longitude (or 700 miles long by about 450) is the last and not the least important of Livingstone's discoveries. It would appear, from the telegram, that the great explorer traced the chain of lakes and the streams which flow from them, until he discovered that all the waters found their outlet in the Tanganyika. He then, it would appear, visited the northern end of the lake, and found that the rivers still flowed into it. The waters of the lake are fresh; and it is, therefore, to be inferred that the lake has an outlet. Livingstone now knows the southern, western, northern, and north-eastern sides of the lake. The south-eastern side alone remains to be explored, and there, if anywhere, the great outlet for its waters must be. That outlet must be discovered and examined before Livingstone's great achievement is ended; and thither, therefore, he will now proceed.

We already have some knowledge of the river which, as it would now seem, flows from Lake Tanganyika to the sea. Mr. Desborough Cooley, in 1841, gave the information obtained from an intelligent Sawahili named Khamis bin Othman, who came to London in 1835. This man had travelled up the ravine of a river named Lufiji, from its mouth due west of the island of Monfia (south of Zanzibar) to its source in the lake. Nearly half a century ago, when Captain Owen was making a running survey of part of the East Coast of Africa, he was off the mouths of this river Lufiji, and they are shown on his chart, published in 1825, though Mr. Cooley and Captain Burton appear to have overlooked them. But Captain Burton, in his exhaustive paper on these lake regions, has shown that the Lufiji is the same river as the Rua or Ruaha, though he says that the source is unknown. It will be found on the maps to the east of the south end of Lake Tanganyika. It must not be confused with another Rua, mentioned by Livingstone to the west of Lake Tanganyika, and north of the Lake Moero. The sentence in Lieut. Dawson's telegram, "Underground village next attracts Livingstone's attention," has, perhaps, been satisfactorily explained by Colonel Grant. He gathered, from the intelligence he and Captain Speke obtained in the country, that the waters of the Tanganyika force their way through a rent in the mountains, at the south-eastern extremity of the lake, and that under the river there is a natural tunnel. This tunnel was described to Colonel Grant as being two months' march from Unyanyembeh, and as a tunnel made by God, which takes a caravan from sunrise to noon to march through it. An unfordable river, with rocky cliffed sides, flows over the tunnel at right angles with Lake Tanganyika. This river is now supposed, on apparently good grounds, to be the Ruaha of Burton, and the Lufiji whose mouths are shown on Captain Owen's chart.

We now learn that Livingstone has reached Unyanyembeh, that stores are being sent up to him as rapidly as possible in charge of his son, and that he will march southward to explore this Ruaha or Lufiji river, this mighty outlet of the great system of waters that he has discovered, with its lofty cliffs and alleged natural tunnel. Thus, for the third time, all fears have been dispelled, again we get a glimpse of this true knight-errant, and again we find him stedfastly working at the task he set himself to do six years ago, and which he will not abandon until it is finished. This last section of his labours will comprise the complete discovery of the great basin of the Tanganyika, including the collection of accurate information respecting its limits, its varied climates, its productions and capabilities and people, its rivers and lakes, and its outlet to the Indian Ocean. The addition to geographical knowledge will be enormous, and we may well hope that this knowledge will be the means by which a new country will hereafter be opened to European enterprise, and the object of Livingstone's life be attained. If he dies in the midst of his discoveries he may well be envied, for a nobler and more glorious end can hardly be imagined. If, as we all hope and as is more likely, he is spared to return home, and perhaps to watch in his old age the progress of the mighty work which he is now initiating, he will receive a welcome from his countrymen such as few have experienced and fewer still have so justly earned.

CONVERSATIONS ON NATURAL PHILOSOPHY

Conversations on Natural Philosophy. By Mrs. Marcet author of "Conversations on Chemistry," &c. Revised and Edited by Francis Marcet, F.R.S. 14th Edition. (Longmans, 1872.)

WE opened this new and revised edition of Mrs. Marcet's "Conversations on Natural Philosophy" with expectation and interest; we closed it with disappointment and regret. The influence Mrs. Marcet exerted upon the early career of Faraday, besides awakening the first love for science in hundreds of the last generation, will cause her name always to be remembered with gratitude and respect. Science, however, has made great strides since Mrs. Marcet wrote; and if her admirable works are touched at all, they should, where necessary, be carefully and accurately revised. That this has not been done in the book before us we will briefly point out. Opening at the Conversation on Heat, we read the following statement on p. 207:—"It is because heat, light, and electricity are not subject to the general properties of other bodies, and in particular to that of gravity, that they are commonly known by the name of imponderable fluids;" and on the next page we read "that modern chemists having adopted the new word *caloric*, to denote the principle that produces heat," we are told that "caloric is found to exist in a variety of forms or modifications; and we shall consider it under the two following heads, viz.: 1. Free or radiant caloric. 2. Combined caloric. The first free or radiant caloric is also called heat of temperature," &c. Again further on, p. 250, the same instructor says, "the two principal solvent fluids are *water* and *caloric*," leading thereupon to a lively conversation as to how caloric dissolves bodies. This, we are told, is the way:—"Caloric, we may conceive, dissolves water, and converts it into elastic vapour by a process similar to that by which water dissolves salt. . . . . It is now ascertained that the solvent power of the atmosphere depends solely upon the caloric contained in it"! Vivid pictures of caloric are given, as "a fluid so extremely subtle that it enters and pervades all bodies whatever, and forces itself between their particles;" in similar language specific heat is defined, on p. 275, as "that which is employed in fitting the capacity of a body for caloric, in the state in which that body actually exists." Thus the minds of young children for whom this book is intended are drilled into the needless and obsolete jargon of the material theory of heat.

Even the most elementary facts are often left wrongly explained. Thus, on p. 255, the formation of hoar frost is accounted for in this way:—"The freezing of the watery vapour, which the atmospheric heat could not dissolve, produces what is called a hoar frost; for the particles descend in freezing and attach themselves to whatever they meet with on the surface of the earth." We venture to think there are few intelligent unscientific people who could not correct this.

We have dwelt thus far on the subject of heat, for here it is that new editions of once famous books need most revision. But glaring errors are to be found in other parts. The diagram representing the shadow which a large luminous body casts behind a small opaque body (Plate 21, Fig. 3), is incorrectly shown, the converging umbra only being represented without the accompanying diverging penumbra. On p. 313 the luminiferous ether and water are spoken of as inelastic bodies. The absence of a sound shadow in air is affirmed on the same page, whereas among other instances every one must have noticed when watching the approach of a distant railway train, how, as it winds along and is occasionally hidden from view, corresponding sound shadows flit across the ear.

We have only space to indicate a few more blunders that catch our eye. In voltaic electricity the electric light does *not* "dart from one point of charcoal to another," as soon as the points are brought from "half an inch to an inch" asunder. The thickness of a silk handkerchief (as the writer has tried with a battery of nearly 200 cells) will prevent the discharge taking place before contact is made. The term "conjunctive wire," p. 428, was used by Oersted, but is not now used to express the wires which join the poles of a battery. In the electric telegraph the current does *not* return through the earth to the battery whence it came. This is a very popular error.

We regret to be obliged to call attention to these serious defects in what might have been made a useful book. We still more regret to think that this volume, owing to Mrs. Marcet's excellent name, will find its way into families and schools; many will thus gain their only knowledge of science from a volume which contains not only many obsolete phrases, but which also omits all reference to the conservation of energy or the correlation of the physical forces. W. F. B.

THE GEOLOGICAL SURVEY OF OHIO

Geological Survey of Ohio. Report of Progress in 1870, by J. S. Newberry, Chief Geologist, including Reports by the Assistant Geologists, Chemists, and Local Assistants. (Columbus: Nevin and Myers, State Printers, 1871, pp. 568.)

THE labours of Prof. Newberry and his colleagues during the year 1870 have resulted in the accumulation of a great many details relating chiefly to the structure of that portion of the great Appalachian Coal-field which extends over a considerable part of Ohio. Without the aid of a good map it is somewhat difficult to follow the descriptions given in this Report, the numerous local references and details having a tendency to bewilder the reader. This, however, is unavoidable under the circumstances; and those who desire to obtain a full and clear conception of the geological structure of Ohio will have to wait the completion of the map and final report promised by Dr. Newberry, the present volume not pretending to be more than its title implies. Nevertheless, it contains a very large and varied amount of information, which will, no doubt, be duly appreciated by those for whom it has been prepared. Especially noteworthy are the numerous illustrative sections of Carboniferous strata, and analyses of coals, ironstones, fireclays, and soils, as also two ably written sketches "On the Present State of the Manufacture of Iron in Great Britain," and "On the State of the Steel Industry," both of which will repay perusal by those of us here who are interested in these matters.

Scattered through the purely geological portion of the Report are many points of interest, which arrest attention as one glances over the pages. Thus we are told that "at Zaleski, in mining the Nelsonville coal, a fine boulder of grey quartzite was found half imbedded in the coal, and the other half in the overlying shale. The quartzite is very hard, and the boulder was rounded and worn by friction before it came to the coal." It measured 17 in. by 12 in., and had adhering to it in places bits of coal and black slate which showed a slickensided surface. The stone appeared to have settled into the coal when the latter was in a soft state. Prof. Newberry speculates with diffidence on the possibility of the boulder having been "brought down by river ice from some higher and colder part of the old continent, which was skirted by the coal-producing lowlands." In connection with this it is somewhat interesting to find that a local deposit of quartz conglomerate occurs here and there underneath and skirting the coal-strata, and is believed by Dr. Newberry to represent an old beach of the period. From some such gravel and shingle deposit the boulder may have been transported, but whether by means of ice, water-plant, or land-plant, who shall tell?

Another exceedingly interesting and readable portion of the Report is the "Agricultural Survey," by Mr. J. H. Klippart, in which the writer discusses, amongst other subjects (such as prairies, forests, &c.), the origin of the soils in certain districts of the State. Those geologists who believe in the former existence during the Glacial epoch of mild interglacial periods will find much here to support their opinion. We are told that the succession of the Drift materials, beginning with the oldest, is as follows:—

a Glacial drift.
b Erie clays.
c Forest bed.
d Iceberg drift.
e Alluvium.
f Peat, calcareous tufa, shell marl.

The oldest deposit is believed to be the product of land-ice, and the presence of the Erie clays betokens that after the disappearance of the great glaciers, wide sheets of fresh-water overspread some districts of the State. The forest bed (consisting of roots, trunks, branches, and leaves of such trees as sycamore, beech, hickory, and red cedar) shows that by-and-by the fresh-water basins were in some places filled up, and the new soil covered with an abundant forest-growth. After this came a period of depression, when great deposits of gravel and sand gathered over the surface of the drowned land, and large boulders and erratics were floated by ice from the north.

These and other matters of interest and importance will, no doubt, be fully treated of in the final report, which is to consist of four volumes, the first two being devoted to the geology and palæontology of the State, the third to its economic geology, and the fourth to its agriculture, botany, and zoology. A large collection of fossils has been made, many species being new to science. It is to be hoped that the good people of Ohio will not grudge the money that will be required for the adequate representation and description of these remains, but that when published the final report will be found in every way as complete as those admirable works which have been issued by other States of the Union. Professor Newberry seems to have little doubt that it will be so, for he thinks that the value and significance of fossils are coming to be generally appreciated. "There are, however," he says, "yet some intelligent men, even editors and members of legislature, who cherish the notion that there is nothing which has any value in this world but that thing which has a dollar in it, and that so plainly visible as to be seen by them. Such men, to quote the language of one of them, 'don't care a row of pins for your clams and salamanders, but want something practical.'" This "practical" man must surely have been related to that colonial official who is said to have objected strongly to the expense of "engraved portraits of extinct bugs and beetles," as he irreverently styled certain Silurian fossils. But the day of such wiseacres has gone past, and it may be confidently expected that Dr. Newberry and his colleagues will have no difficulty in getting the necessary funds voted for the completion of their important Survey. J. G.

OUR BOOK SHELF

Meibauer's Physische Beschaffenheit des Sonnensystems. (Berlin: Carl Habel.)

THIS is a second and freshly-arranged edition of a comprehensive little treatise on the nature of the solar system. It requires no great acquaintance with the present state of science to vindicate the accuracy of the author's preliminary remark, as to the difficulty that students experience from the wide dispersion of modern observations among heterogeneous memoirs and journals in various languages, and the necessity of a large library and abundance of leisure; and it is impossible not to appreciate his attempt to combine these scattered materials in a condensed and accessible form. Nor can it be doubted that a considerable amount of labour has been devoted to the work, which has been made attractive by perspicuity of treatment and facility of style, as well as by occasional ingenuity in hypothesis. Yet the execution cannot be said to correspond with the excellency of the design; and the deficiency, more apparent perhaps to our own minds than to those of Continental readers, is such as necessarily results from one-sided and imperfect views. The eternity of matter, an idea to many minds especially and utterly abhorrent, should not, to say the least of it, have been assumed; and other less objectionable hypotheses and statements are adopted, which may not be as incontrovertible as unwary readers will be led to suppose. No doubt the author, in employing as part of his motto the words of Darwin, "False facts are highly injurious to the cause of science," was quite unconscious that the result of an inquiry into some of his own facts (or rather assertions) would not be quite satisfactory. But we do not know what to make of such statements as these—that Priestley called his vital air (oxygen) by the name of Phlogiston—that Huggins found in the nuclei of comets the lines of nitrogen, hydrogen, and carbon similar to those given by the Geissler tubes—that there are two bright lines in the spectrum of Sirius, one of which is displaced by the star's movement—that the red, green, and yellow tints of the aurora never lose their relative positions; that the force of gravity at the upper limit of the atmosphere may be considered not materially different from that on the earth's surface, while the centrifugal (tangential) force perceptibly increases. Nothing but an unkind, or bitter, or self-ignorant spirit would refuse to leave a fairly broad margin for inevitable human imperfection; but it must be a very large paper copy indeed that would find room for statements such as these. Nor is it easy to understand why Lockyer's just claim should have been ignored to an equal share with Janssen in the grand discovery of prominences round the uneclipsed sun; or why discredit should have been thrown upon the connection of the solar-spot maximum with Sabine's magnetic period, or the planetary one established by the Kew observers. Other omissions might be pointed out, and the work would have been greatly improved by a discussion of the effects of temperature and pressure in modifying elementary spectra—a branch of inquiry to which allusion has barely been made, but which is of essential importance in spectrum analysis, and the fuller development of which alone, perhaps, promises a more satisfactory solution of many cosmical phenomena. But while it appeared a matter of duty to mention these deficiencies, we must add, in all fairness, and with greater pleasure, that some of his theories are very interesting and well handled; such as that in which he would account for the eruption of the protuberances by the unstable condition of gaseous matter on the confines of fluidity, discovered by Andrews and Thomson; or that of the unlimited extension through space of the planetary atmospheres in extreme tenuity; and there is much ingenuity, at any rate, in the idea of accounting for the variations of atmospheric pressure and electricity between the tropics by the resistance, however infinitesimal, which our globe sustains in its rapid passage through a space to which neither Newton nor Laplace ascribed absolute vacuity. The curious inconsistency with which, as a denier of equivocal generation, he calls in the germs of terrestrial vegetation from external space, where they have been educed under certain conditions of temperature, pressure, and time, is but a specimen of the difficulties to which every hypothesis is subject, that ignores the existence of an omnipotent will; but there are some who will look with amusement, and some few with a warmer feeling, at his vigorous onslaught on the idea of a luminiferous æther; concluding with the keen remark, that to prove the existence of such an æther, recourse is had in turn to the very phenomena which it was invented to explain.

T. W. W.

Knapsack Manual for Sportsmen on the Field. By Edwin Ward. (Bradbury and Evans, 1872.)

ONE who has come so much in contact with sportsmen as Mr. Ward must have done should surely know that men do not go out with knapsacks when intent on killing big game. The title "Knapsack Manual" is most unfortunate. Moreover, if the book is intended for sportsmen *on* the field, why should a considerable portion of it be given up to the mode of setting up a tiger, which a sportsman is very unlikely to do for himself, and certainly would not attempt in the field? Mr. Ward, though he seems to have considerable regard for artistic treatment and compatibility in the setting up of skins, would yet appear to put lichens with his stuffed birds in the conventional style. What a relief to the eye it would be to see a case of stuffed birds without a particle of dead wood or lichens in it! The directions given for skinning and preserving specimens are not full enough; there are better works on the subject in existence. The lists of game to be found in various parts of the world, at the commencement, form the most useful part of the book. The account of a Museum of Natural History of the Earth from man to a granite stone contained in a case 10 ft. long by 7 ft. high, displays a lamentable amount of ignorance. Some of the remarks about the various creatures are very amusing, as, "Gasteropoda proceed by the belly." "Armadillos are very remarkably swift in flight." Altogether this book appears to be of the nature of an advertisement, and we think a not very successful one.

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 Method of Least Squares

IN the number of NATURE for June 6, Prof. Asaph Hall, of Washington Observatory, called attention to what he regards as a singular oversight in the history of this subject, viz., that in 1770-1773 Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les résultats de plusieurs Observations, &c." Prof. Hall remarks that the only notice of this memoir he has seen is contained in the *Berliner Jahrbuch* for 1853, and that in the abstract of a memoir of mine on the subject in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not appear.

As regards myself, I need only state that Lagrange's memoir, as well as Simpson's, is referred to in my paper; although, as no examination is made of it there, the name is omitted in the Abstract, where reference is only made to the authors of investigations in which an attempt is made to prove either the law of facility or the method of least squares, and which were therefore referred to with more or less detail in the paper itself.

Further, I should not regard it as an omission if in the history of Least Squares no mention was made of Lagrange; in fact, when I was examining all the investigations I could find on the subject, after looking through Lagrange's memoir (and reading carefully Todhunter's *résumé* of it), I came to the conclusion that it contained nothing that conld, properly speaking, be regarded as an anticipation of the later investigations of Gauss, Laplace, &c., and I contented myself therefore with merely a passing reference.

Lagrange's paper, as its title implies, gives a mathematical justification of the choice of the mean of a series of discordant observations, and a determination of the chance that the resulting error lies between certain limits, with developments, &c.; but the method of Least Squares may be described as an extension of the principle of the arithmetic mean to the combination of linear equations, involving more than one unknown; the problem being to obtain the best values of the unknowns from a series of discordant *linear simultaneous equations*.

The method of Least Squares was first proposed in print by Legendre in his "Orbites des Comètes" (Paris, 1805), as a convenient way of treating observations without reference to the Theory of Chance. Legendre's words are "la méthode qui me paroît la plus simple et la plus générale, consiste à rendre minimum la somme des quarrés des erreurs . . . et que j'appelle Méthode des moindres quarrés. The method, regarded from a practical point of view, is a very natural one; we shall clearly get a good result by determining the quantities to be found so as to make the sum of the $2n$th powers of the errors a minimum, and in order that the resulting equations may be linear (and therefore manageable), we must take n equal to unity.

Though first published by Legendre, the rule was applied by Gauss, as he himself states, as early as 1795, and the method is explained and the usual law of facility for the first time found in the "Theoria Motus Corporum Cœlestium, Hamburgh, 1809 (not 1808, as in Prof. Hall's letter). The principle on which Gauss proceeds may fairly, I think, be stated as follows:—If there are given a number of discordant observations V_1, V_2, &c., of a quantity x, so that we have the equations $x - V_1 = 0, x - V_2, = 0$, &c., then it is known that a very good result is obtained by giving to x the arithmetic mean of its observed values, and writing $x = \frac{1}{n}(V_1 + . . . + V_n)$; and it is required to find an equally good rule for determining x, y, z, &c., from a number of discordant equations of the form $a_1x + b_1y + c_1z + . . . = V_1, a_2x + b_2y + c_2z + . . . = V_2$, &c. Assume therefore that $x = \frac{1}{n}(V_1 + . . . V_n)$ is the most probable value of x derived from the first system of equations, and find the law of facility of error that this may be the case; then, having this law, the most probable values of x, y, z, &c., can be found for the second system.

The law of facility Gauss finds to be represented by $\frac{h}{\sqrt{\pi}} e^{-h^2x^2}dx$, viz., this is the chance of an error of magnitude intermediate to x and $x + dx$; and thence it follows that the most probable values of x, y, z, &c., are found by making $(a_1x + b_1y + c_1z + . . . - V_1)^2 + (a_2x + b_2y + c_2z + . . . - V_2)^2 +$, &c., a minimum. Gauss then proceeds to determine h in the manner still generally adopted.

Subsequent writers, Laplace, Poisson, &c., have in consequence investigated how far the arithmetic mean is the most probable result, &c., and in one sense Lagrange (and *a fortiori* Simpson) may be said to have very slightly anticipated a portion of the analysis required in these researches, although, as far as the method of Least Squares is concerned, there is no anticipation. A slight examination will show how greatly superior Laplace's analysis is to Lagrange's on the same subject.

With reference to the independent discovery of the method of Least Squares by Dr. Adrain of New Brunswick, U.S. (see Prof. Abbe's note in the *American Journal of Science*, June 1871), I may remark that if for distinction we call the introduction of the merely practical use of the rule its "invention," and its philosophical deduction by the Theory of Probabilities its "discovery" (so that Legendre invented the method and Gauss discovered it), then Dr. Adrain can only be credited with the independent invention of the rule, viz., he only did what Legendre had done two years previously. This is worth noticing, as from the occurence of the function e^{-x^2} in Dr. Adrain's paper, it might be supposed that it contained some anticipation of Gauss' investigation; but such is not the case, and Dr. Adrain's reasons for the adoption of the law are of so trivial a nature that it is incredible that any mathematician should have been led to the discovery of the method by means of them. I imagine that he had noticed the practical convenience of the rule, and subsequently endeavoured to justify it analytically; it may be noted that it is possible that Dr. Adrain may have seen or heard of Legendre's memoir published two years before; his silence on the matter, however, renders it unlikely that this was so. On the whole, by far the greater part of the merit of the introduction of the method is due to Gauss; while the credit of the first suggestion of the practical rule must be assigned to Legendre, Dr. Adrain having, in all probability independently, also suggested the same rule subsequently. It is necessary to be thus particular, as Gauss' publication having taken place in 1809 and Adrain's in 1808, it might be thought that the latter had anticipated the former to some extent, which is in no wise the case.

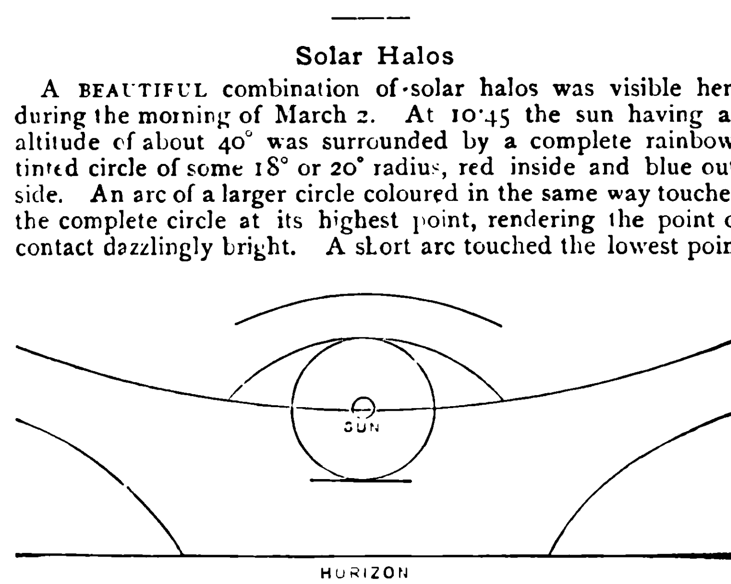
In writing the history of the Theory of Errors or the Theory of the Treatment of Observations, there are several memoirs anterior to Legendre's that would have to be included, and notably Thomas Simpson's "Miscellaneous Tracts," 1757 (which is the work Prof. Hall doubtless refers to), Daniel Bernoulli's "Dijudicatio maxime probabilis plurium observationum discrepantium," &c. Acta. Petrop. 1777, Trembley's paper in the "Berlin Memoirs," 1801, "Observations sur la méthode de prendre le milieu entre les observations," &c. For the above references I was indebted to Todhunter's "History of the Mathematical Theory of Probability from the time of Pascal to that of Laplace" (London, 1865), which contains a notice of every work or memoir on the subject to the commencement of the present century (there is a *résumé* of Lagrange's memoir occupying 13 pages), so that no one need have any fear of passing over any writings published previously to 1800. Having had occasion to make much use of the work, I may be permitted to say that its value, both as regards accuracy and completeness, cannot be over-estimated.

J. W. L. GLAISHER

Trinity College, Cambridge, June 8

Solar Halos

A BEAUTIFUL combination of solar halos was visible here during the morning of March 2. At 10·45 the sun having an altitude of about 40° was surrounded by a complete rainbow-tinted circle of some 18° or 20° radius, red inside and blue outside. An arc of a larger circle coloured in the same way touched the complete circle at its highest point, rendering the point of contact dazzlingly bright. A short arc touched the lowest point



of the circle in the same manner. A white halo passed through the sun's position parallel to the horizon, and two fainter white arcs intersected it obliquely in the point opposite to the sun, forming a conspicuous sun-dog. There were also two rainbow-arcs having their convexities toward the sun. These were blue inside and red outside, and their centres appeared to be about 90° from the sun, and some 15° below the horizon. Later an arc concentric with that touching the complete circle appeared above it, having the colours reversed, namely, blue inside and red outside. These appearances lasted about an hour and a half before beginning to fade away.

W. W. J.

Gambier, Ohio, March 5

The Volcanoes of Central France

THE Auvergne volcanoes threaten to be as periodic a subject of controversy as the authorship of the letters of Junius. It is only seven years since the last eruption of letters. At that time I contributed a paper to the *Geological Magazine* (vol. ii. p. 241), in which I collected, printed, and translated all that I could find on the subject, and came to the conclusion that it was very probable there had been some local outbreak of volcanic action. Thus I agree with Mr. Garbett, but it appears to me that he has not made his case in one respect so strong as it might be. In the passage "nunc ignes sæpe flammati *caducas culminum cristas* superjecto favillarum monte tumulabant" (as the edition which I follow has it) he translates *culmina* "roofs," and again in the parallel passage of Avitus. I think it more likely to mean summits (of mountains), and to refer to the formation of one or more new cones in the hill country.

My reason for this may be given in the words which I used in the paper above named—"though Sidonius is inclined to bombast, he scarcely seems equal to a flight like this. . . . In the parallel passage in Avitus, the reference to Isaiah ii. 10, 19, 21, and Luke xxiii. 30 appears too clear to allow any other meaning than mountain-top to be assigned to *culmen*." To this I may add that the ridge-roofs, *cristæ culminum*, would be those least likely to be broken by a shower of ashes, and the ridges would be the part where the smallest quantity of ashes would rest. T. G. BONNEY

St. John's College, Cambridge

Force and Energy

REFERRING to Mr. Brooke's article in NATURE of the 13th on Force and Energy, I would suggest that though it is quite true that heat is a "mode of motion," this is probably not true of magnetism and static electricity. Heat is molecular *motion*, magnetism and static electricity are molecular *tensions*.

I would also remark that the term "radiant heat" ought to be discarded as misleading. Radiant heat is not a kind of heat; it is quite distinct from heat, but it is nearly identical with light. We ought to introduce the word *radiance*, and then we get to this statement:—All rays of radiance have more or less heating power, and some of them have also the power of producing the sensation of light. But the fact that only some rays, and not those which have the most heating power, produce the sensation of light, belongs rather to the retina than to the rays.

Mr. Brooke thinks the proposition that the sum total of energy in the universe is unchangeable is incapable of proof. I do not speak as having any authority, but it seems to me that if this is not true the conservation of energy cannot be universally true. JOSEPH JOHN MURPHY

Pelagic Fish-Nest

SEEING an extract from NATURE with reference to the nest of the pelagic fish, allow me to inform you of the discovery of what I presume to be a similar nest in lat. 25° N., long. 65° W., whilst on a voyage between Buenos Ayres and New York last January. I had improvised a drag-net out of a barrel hoop and a biscuit bag, to fish up for examination the straw-coloured floating gulf-weed, which covered the sea in long lines and patches between 20° and 32° N. lat.; and one day there came up in the net a mass of weed compactly woven by strong, white, silky fibres into a round ball of about ten inches in circumference. The surface of this ball was covered with a network of these fibres, to which large numbers of glassy eggs, about the size of partridge shot, were attached. The eggs were transparent, and their cases very tough. The only living inhabitants of the ball were one or two small shrimps and a small crab, who was carrying his own particular egg-sac.

Another curious fact I am tempted to mention. About 200 miles from Cape Frio, the sailors caught a dolphin, which had in its stomach twenty pieces of coal, varying from a large walnut to a marble in size, together with the heads of four iron nails about an inch in length each. I am tolerably certain that these articles had not been thrown from our vessel, but they did not appear affected by the internal wear and tear, however long they might have been digesting. GEORGE J. HINDE

Toronto, Canada West, May 18

Why are Red Sandstones Red?

I HAVE lately been interested in the reply to this question given by Prof. Ramsay, and stated by Prof. Geikie in his recent edition of "Jukes's Manual of Geology" (pp. 567, 568). But the explanation, viz., that the red colour is derived from the precipitation of red (consequently anhydrous) peroxide of iron in inland seas, appears to me to give rise to this other question—Why should the precipitated peroxide be anhydrous, and not hydrous and brown, as is the case with limonite, which is found deposited in marshes, ponds, and lakes?

I have tried some experiments in precipitating the peroxide of iron from a solution made as saturated as possible by long boiling of water or oxide of iron (obtained from a natural spring), common salt, and finely divided sulphate of lime (these last two minerals being found to accompany the red rocks), filtering hot, and allowing to stand till cold. For want of experience in these matters, probably, I have not yet succeeded in obtaining any red colour.

I have, however, to-day fallen on a paper describing a similar experiment to account for the presence of anhydrite in the Stassfurt mines. In this case it is stated that the *anhydrous* sulphate of lime was obtained on evaporating a concentrated solution of gypsum and rock salt.

I should be glad to learn whether the attention of any of your readers has been drawn to this question, and whether they have succeeded in obtaining (under conditions analogous to those of an evaporating inland sea) a precipitation of the red colouring matter. A YOUNG GEOLOGIST

Mounting of Thermometers

I HAVE experienced precisely the same inconvenience as that mentioned by Mr. Whipple in NATURE last week.

I several times removed the outside case of a thermometer such as he describes, and took every precaution to dry the air before replacing the packing, but the moisture in the tube persistently reappeared. It then occurred to me that the amount of moisture was out of all proportion to the quantity of air confined, and that the mischief arose from the packing not being air-tight; and fresh damp air was thus continually finding its way into the tube, and depositing moisture. Accordingly the tube was again removed, and after drying carefully, I replaced it, and pushed in the india-rubber packing about an eighth of an inch. The intervening space was filled up with common putty, which was made to assume a conical form round the thermometer stem. After being left for a day or two to harden, the putty was painted over with two or three coats of sealing-wax dissolved in alcohol. This thermometer has been constantly exposed on the grass for about four months, and though I purposely took no means to dry the air in the case, not the slightest inconvenience from a deposition of moisture has since been experienced.

REGINALD BUSHELL

Hinderton, Neston, Cheshire, June 17

A Few Millions

IN your reprint of Prof. Mayer's paper, entitled "Acoustical Experiments" in NATURE for May 9, 1872, there occur some strange numerical errors, which perhaps it will be well to point out, lest some of your readers should make use of the numbers given at the end of the paper without previously testing them. After describing his experiments, he proceeds:—"We will now examine the analogical phenomena in the case of light:—Let fork No. 1, giving 256 vibrations a second, stand for 595 millions of millions vibrations a second, which we will take as the number of vibrations made by the ray D_1 of the spectrum." Taking the velocity of light as 185,300 miles per second, and the wave-length of D_1, as given by Angström, at 0·00058950 millimetres, gives 5,058,700,000,000,000 vibrations per second, or a little more than five thousand millions of millions, instead of a little less than six hundred millions of millions vibrations per second, as given by Dr. Mayer. But to proceed—"Then fork No. 3 will represent 590 millions of millions vibration per second," this should be 594 millions of millions vibrations, "which give a wave-length ·0000042 millimetres longer than D_1." This again is not quite right, even according to Dr. Mayer's own showing; it should be ·00000495 of a millimetre longer than D_1. Dr. Mayer then goes on to say that such a wave-length nearly corresponds with an iron line situate ·42 div. below D_1 on Angström's chart; and "we saw that fork No. 3, giving 254 vibrations a second, had to move toward the ear with a velocity of 8·734ft., to give the note produced by 256 vibrations per second emanating from a fixed point; so a star sending forth the ray which vibrates 590 millions of millions a second will have to move toward the eye with a velocity of 28,470 miles per second to give the colour produced when ray D_1 emanates from a stationary flame." This again, according to Dr. Mayer's own method, should be 1,557 miles, or less than a nineteenth of the velocity given by him.

Instead of involving ourselves in millions of millions, and the translation of millimetres into English miles, it seems simpler to avoid the calculation of the number of vibrations per second, and to get at the required velocity by a simple rule-of-three sum, thus: As the emitted wave length is to the difference between the observed wave-length and the emitted wave-length, so is the velocity of light to the required velocity, to or from the observer.

A. COWPER RANYARD

PROF. CANNIZZARO'S FARADAY LECTURE

THIS lecture was delivered on May 30, by Prof. Cannizzaro. The lectureship was founded by the Chemical Society in honour of the illustrious Faraday, to be held by some eminent foreign *savant*, who, during the term of his tenure is to deliver a discourse before the Society. Dr. Frankland, in introducing the lecturer, said that in 1869, M. Dumas had honoured them with his presence there, and on that night they were to listen to Prof. Cannizzaro, of Palermo. After alluding to the numerous investigations which the Professor had made in organic chemistry, and amongst others the discovery of benzylic alcohol, the first normal aromatic alcohol that had ever been prepared, and to the important theoretical views which he had originated, the President, in the name of the Society, presented to him the Faraday Medal, struck in honour of his visit.

Prof. Cannizzaro said that when he received the flattering invitation to deliver the Faraday Lecture, he was placed in very unfavourable circumstances to respond to it, as he had no definite results to lay before the Society, and was, moreover, on the point of suspending his labours and abandoning his old laboratory in order to remove to Rome, and establish a new one there. In this difficulty a subject for a discourse fortunately presented itself, one which the celebrated French chemist, Dumas, had promised to treat of in 1847, namely, the form which the theory of chemistry should take at the present time. Although this could not be fully discussed in so short a space of time, it would at least have the advantage of directing the attention of chemists to a question of great importance in the transition stage which our science is at present going through.

In recalling the promise which M. Dumas had made to the Academy of Sciences of Paris in 1847, to examine the form which theoretical instruction in chemistry should take in the present state of the science, the lecturer proposed to consider in his discourse the limits within which the exposition of general theories should be included in teaching chemistry, and the form that it was desirable that they should assume. Whilst giving a broad sketch of the progress of modern chemistry, he showed that the atomic theory had become more and more intimately interlaced with the fabric of chemistry, so that it is no longer possible to separate them without rending the tissue, as it were, of the science ; and that up to the present time we have been unable to enunciate even the empirical laws of chemical proportion, independently of that theory ; for those who employ the term equivalent in the sense that Wollaston did, commit an anachronism. Consequently, in the exposition of the value and use of symbols, formulæ, and chemical equations, not only are we unable to do without the atomic and molecular theory, but it is inconvenient to follow the long and fatiguing path of induction which leads up to it. By one of those bold flights of the human mind we can at once reach the height whence we discern at a glance the relations between facts.

He then went on to show that the solid basis, the corner-stone of the modern molecular and atomic theory, the crown of the edifice of which Dalton laid the foundation—is the theory of Avogadro and Ampére, Koenig and Clausius, on the constitution of perfect gases, to which chemists, unknown to themselves, have been led in the progress of their science. He thought the time had arrived for reversing the order which had hitherto been followed in teaching chemistry, that instead of setting out from the criteria for determining the weight of molecules, and then showing their ratio to the vapour densities, they ought, on the contrary, to commence with the latter, with the theory of Avogadro and Clausius, demonstrating it from physical considerations ; to found upon that the proof of the divisibility of simple bodies, that is to say, the existence of atoms ; and to show, as occasion presented itself, that the weights of the molecules and the number of the atoms deduced by the application of this theory, are in accordance with those which are deduced from chemical criteria. By this means we can measure the degree of confidence to be placed in the latter criteria ; since so-called compound equivalents do not suffice to determine the weight of molecules, or even to prove their existence, although they may be deduced from a single principle, the theory of the constitution of gases. This is the natural transition from physics to chemistry.

The Professor then stated in detail how he applied the principles he had laid before them. He introduced his pupils to the study of chemistry, by endeavouring to place them on the same level as the contemporaries of Lavoisier, and to teach them to appreciate the importance of the principle of the conservation of the weight of matter, showing them that this is quite independent of any idea of its nature or constitution. They are thus led to examine the ponderable composition of substances, so that the student passes rapidly from the epoch of Lavoisier to that of Proust, and then to that of Berzelius at the time when he commenced his researches on proportions. At this stage the same impulse is given to the pupil as Berzelius received on becoming acquainted with the hypothesis of Dalton. The latter is laid before him without any accessory, the use of symbols and formulæ being introduced dogmatically. There will now arise in his mind the same doubts and difficulties that assailed Berthollet, Sir Humphrey Davy, and Wollaston in the application of Dalton's theory, and at the same time a desire for an explanation of the simple relation which exists between the vapour volumes of bodies which react on one another and of the products which are obtained. Now is the moment to state or recall to mind the physical theory of the constitution of the perfect gases. Commencing with a rapid glance at their general and special characters, he insisted, that in this part of the instruction the mind of the student should not be diverted from the numbers expressing their relations, by considerations of the variations caused by changes of temperature and pressure. In applying the theory of the constitution of gases, it will be perceived that the molecules of simple bodies are not always the atoms of Dalton, and a certain confusion will thus be produced in the mind of the beginner in the conception of the ideas of atoms and molecules. The hypothesis of Dalton can now be laid aside, substituting as a starting-point the theory of the relation of molecular weights to the vapour densities. A table must be prepared of the vapour density compared with that of hydrogen as 2, that is to say, the weights of their molecules compared with the weight of the semi-molecule of hydrogen taken as unity. We must then compare the composition of the molecules containing the same element—including, or not, the molecule of the element itself—and thence deduce the law of the existence of atoms, that is to say, the amount of each element which always enters, by whole multiples, into the molecules which contain them. We here have the atoms of Dalton which, in the present state of the science, express not only all that Dalton discovered, but also the composition of equal volumes of their vapours, and in the choice of which those doubts can no longer arise which embarrassed Davy and Wollaston. The ideas of molecules and atoms suggested to the student by this law are devoid of all considerations of form, size, continuity, or discontinuity ; the only property indissolubly connected with them is that of ponderability ; the very definition of matter.

Recollecting that no physical theory of the constitution of matter had yet been advanced which thoroughly conformed to chemical ideas, he insisted upon the advisability in teaching the molecular and atomic theory, to keep it free from all that is not absolutely essential, so that it may preserve sufficient plasticity to adapt itself to the progress of our physical and mathematical knowledge. For this purpose he thought it useful to allow the student in the first place to glance at the changes in the hypothesis of the constitution of matter, and then to cause him to estimate the degree of confidence they merit in the actual state of our knowledge. Having thus placed upon a solid basis the fundamental notions of atoms and molecules by the comparison of the composition of equal volumes of the bodies in the gaseous state, it becomes necessary to consider the difficulties which arise in the application of these notions when the vapour densities are wanting; he explained and justified the use of the various auxiliary criteria to which we have resource in these cases, proving them in the first instance by the touchstone of the theory of Avogadro and Clausius, by showing that they gave results in accordance with that theory whenever the two methods can be employed simultaneously.

He believed that we should never lose sight of the starting point, nor give the formulæ of all compounds as of equal probability. "It is not by concealing the obscurity of these questions that we shall enlighten the student; on the contrary, we should estimate each fact at its true value by showing him that our science does not merit an equal degree of confidence on all points." This forms the introduction, the preparation for the study of the transformations which matter undergoes; the real object and aim of our science.

The comparison of the atomic composition of molecules has led chemists to the law of substitution, to the theory of types of Dumas, then to that of Williamson and Gerhardt, and lastly to the theory of the different valency of atoms and their modes of union, or the so-called theory of atomicity which includes the former. Although at present it is impossible, in teaching chemistry, entirely to eliminate this latter theory, which gives a summary of several laws, and guides us ordinarily in the co-ordination and even prevision of a large number of facts, yet it is difficult to keep it within just bounds so as to avoid infusing into the mind of the beginner illusions which are dangerous for his intellectual education. In order to avoid this, it is advisable to bear in mind the progress of this doctrine and the actual phase of development which it has at present reached. It is still far from being a complete and well-established theory, but is in a state of transition; for although doubtless it embraces a large number of facts, as yet it does not embrace them all. It is only a partial representation of the reality, and that from a restricted point of view, showing but little relation to our views of the constitution of matter, for it is the result of a comparison of diverse facts expressed by means of the atomic and molecular theory. It is convenient, therefore, to consider each part of this doctrine exclusively in relation to the group of facts which has suggested it.

It is unadvisable to define the valency of atoms as a property inherent in them, and then to deduce as a corollary their different modes of union; on the contrary, it is preferable to regard each portion of this doctrine as a deduction from the observation and comparison of a determinate group of facts, until an opportunity offers to unite these fragments into one whole, not forgetting, however, to notice the gaps which exist, never going beyond what the facts themselves suggest, and never applying to all bodies indiscriminately, the laws which suit only a single group. For instance, we must not pass over in silence the facts that whilst certain elements are bi tetra- or even hexa-valent, others are tri- and penta-valent; but the pupil should be prevented from acquiring mechanical and geometrical ideas of the cause and effects of the valency of atoms, by frequently reminding him that chemical facts show nothing about the size, form, continuity, or relative position of atoms. If we are sometimes obliged to employ the expression, "relative position of atoms in the molecules," and even to represent them graphically, we must warn the student that these are only artifices to express certain transformations, and that we are really ignorant of the relative position of the atoms either in space or in the mutual action of different portions of matter. With these reservations, it is possible, in teaching to derive considerable advantage from the theory of atomicity and at the same time to avoid its inconveniences.

In the study of the transformations which matter undergoes, we should direct the pupil's attention, not only to the ponderable changes in the composition of molecules, but also to the electrical and calorific phenomena which accompany these transformations. Even from Lavoisier's time it has been recognised that we cannot separate the study of matter from thermic considerations; and every day the connection which exists between chemical and thermic phenomena becomes more apparent.

As in the study of ponderable changes we were guided by the law of the conservation of weight, so in the connection between chemical and dynamical phenomena we are guided by the law of the conservation of force; the two studies mutually supplementing and illustrating one another. Not only will the atomic and molecular theory and that of atomicity help us to compare dynamical phenomena, but the study of dynamical phenomena will show us analogies and differences between chemical actions which would not be observed in the ponderable equations. We should therefore instruct the student in the little definite knowledge which we at present possess concerning thermic and electric phenomena, and especially fix in his mind the fundamental notion of a mechanical equivalent, and the manner of comparing it with chemical action as expressed by the atomic theory. In this we should be aided by the previous or simultaneous instruction of the student in physics under the form and language of the thermodynamic theory.

The lecturer concluded by observing that in the choice of methods and of matter for a course of chemistry, it should always be borne in mind that it was eminently a progressive science, and that even at the time of its most rapid development. The student should start not only with a knowledge of certain definite and fixed principles, but with an aptitude and sufficient preparation to enable him to follow the science in its unceasing transformation and progress, whether he intends expressly to cultivate chemistry, or has only learnt the elements of the science as an auxiliary to other studies or professions. Moreover, the end of chemical instruction for both these classes of students is not only to fix in their memory a certain amount of knowledge, but to assist in their intellectual education. For this, chemistry of all sciences is one of the best, offering both in verbal and practical instruction—excellent occasions for the exercise and harmonious development of all the faculties of the human mind.

He had desired to call attention to what he considered to be the most efficient means of imparting a knowledge of chemistry so that it might serve as an instrument of intellectual education, and that the student, by following it in its ulterior developments, might judiciously apply it to the study of the other branches of natural science. If the attention of the eminent chemists and professors there present were once attracted to this subject, he felt certain that a bright light would be thrown on it, and that our young professors would find numerous suggestions to direct them in teaching chemistry, and that at the very moment when instruction in our science had become so difficult, on account of the rapid transformation which it was undergoing.

Dr. Williamson said, that those who were there present ought not to separate without some expression of the pleasure that they had felt on listening to so learned, vast, and eloquent a discourse, treating as it did of a most difficult and important problem. There was scarcely anything of greater moment in the scientific education of youth than the rightly setting before them those wonderful transformations of matter which it is the province of chemistry to explain. These great and growing truths, for, as the lecturer had said, they were growing truths, should be set before youth in such a manner as to form a coherent whole. He hoped to study this masterly discourse with profit and delight, and would now propose a vote of thanks to his illustrious colleague for the honour which he had done them in delivering to them the Faraday lecture.

Prof. Tyndall said he had heard the discourse with deep interest, for it showed that the lecturer knew the importance of a teacher's vocation, and that his province was not merely to communicate knowledge, but to do it in such a manner as to arouse an interest in and love of the subject in the pupil by presenting it in its proper relations. He would have welcomed the lecturer to that Institution, even had he come to tear in pieces the notions which he cherished regarding atoms and molecules; how pleasant it was then to find such a broad agreement between their views. The chemist cannot halt at equivalent proportions—he must ask himself whence they arise, and the inevitable answer is some form of the atomic theory. This theory, however, cannot be confined to chemical phenomena. The motions of those atoms and molecules underlie all our explanations of the physical cause of light and heat, and it is already taking up the field of magnetism and electricity. Consider, for example, the heat of gases, both as regards the motion of translation of the molecules which produce temperature, and the motions of rotation and vibration of their constituent atoms, which, though they do not express themselves as temperature, constitute a portion of the heat. Clausius has shown that even in the simplest gases nearly two-fifths of the whole heat is due to these internal motions; while in gases of complex molecular constitution which condense on combining, the ratio of the total heat to the heat of temperature is still greater. The experiments of Regnault, which show that the specific heat of a perfect gas at a constant volume is constant, proves, as Clausius has shown, that the one kind of motion is proportional to the others.

The lecturer had also referred to atoms of the same kind combining together, so that, free oxygen and free hydrogen being considered as composed of molecules each containing a pair of atoms, has certainly simplified the results. But it must not be forgotten that this combination of like atoms is widely different from that of unlike atoms. The union of oxygen with oxygen or nitrogen with nitrogen produces no such effects upon the luminiferous ether as the union of oxygen with nitrogen. With the same quantity of matter the amount of *vis viva* sent forth as radiant heat may be augmented a thousandfold, perhaps a millionfold, by the act of diverse combination. This act seems to carry with it a condensation of the ether to a dense atmosphere around the atoms. Had a cannon the power of gathering round itself a dense atomsphere, it would send forth a greater amount of *vis viva* as sound. A gun fired at Chamouni may be heard upon Mont Blanc, while the same gun fired on Mont Blanc may not be heard at Chamouni, because the air on which the concussion takes place is denser in the one case than in the other. In the same way the diverse atoms vibrating in the denser atmosphere formed on combination show their vast superiority as radiators over like atoms which, except in such special cases as ozone, &c., are incompetent to produce a similar condensation. The speaker then asked them to echo the resolution so well put to the meeting by Prof. Williamson.

THE OBSERVATORY ON MOUNT VESUVIUS

WHILE the scientific world and his own countrymen are rivals in doing honour to Prof. Palmieri for his zeal in remaining at his post in spite of all danger, it may be interesting to examine in some detail the work done at the Observatory of Mount Vesuvius. We know wonderfully little about the origin and mutual dependence of volcanic phenomena. This is due to a want of accurate observations. For the complete investigation we require first to know at what dates earthquakes and eruptions occur at different parts of the earth. Next we must have observations of the direction and exact hour at which a wave of disturbance passes different places whose positions are known. This gives us the velocity of the wave, and helps to determine the position, under the earth's surface, of the centre of disturbance; or, if a wave be propagated over the sea, we obtain a means of estimating the average depth of the intervening ocean; for the velocity of a wave increases with the depth of the sea. This method gives one of the best determinations we possess of the depth of the Pacific Ocean. But beyond this we must have observations made systematically at some place subject to earthquakes and volcanic eruptions. No place in Europe is more suitable for this than the neighbourhood of Mount Vesuvius; and it was for such observations that an Observatory was established there.

Everyone knows that Mount Vesuvius consists of a great cone of lava and ashes, at the top of which is the great crater. On the northern side, separated from it by the deep valley called the Atrio del Cavallo, rises the precipitous and semicircular Monte Somma. This once formed the crater of the volcano, and the present cone seems to have been formed inside that great crater at the time when Pompeii was overwhelmed. On a spur of rock, a mile or two in length, running down from the Atrio del Cavallo, the Observatory is placed. It is close to the well-known "Hermitage," or half-way house, in the ascent of the mountain. Being raised on this ridge above the surrounding country, it is comparatively safe from the molten lava that flows at times on either side of it.

The building itself is handsome; in fact it is to be regretted that so much money should have been devoted to the masonry instead of to additional instruments. On the ground floor are the inhabited rooms, all scantily furnished; but the pursuers of science cannot always expect bodily comfort. On the first floor we find the Museum, with a fine collection of minerals found on the mountain. Perhaps it may be as well here to correct a common mistake as to the nature of the yellow substance found about the craters, whose brilliant colours remind one so much of the Solfatara. This substance is not sulphur, but copper. The most interesting objects in the Museum are the "fumerolles," or smoke-holes. Occasionally at the end of an eruption you may see at the bottom of the crater a small cone of lava, with a hole in its top, through which the steam pours with a hissing noise like a wave breaking on a pebbly beach, or like a blast furnace, or as Pliny has it, like the grinding of a saw; the intensity of the sound varying with your position. These small cones are the fumerolles; they are a foot or two high; and Palmieri has actually had several of these natural chimneys cut off and transported to the Museum.

We now pass on to the Observing Room. There are solid piers carried up from the ground to support the instruments. First comes the elegant seismograph for the automatic registration of earthquake shocks. The object of the instrument is twofold: first to measure the direction and intensity of a shock; and, second, to write down a history of the earthquake. The shock may be either vertical or horizontal, or partly vertical and partly horizontal. For the vertical shocks a fine metallic point is suspended by a coil of wire over a cup of mercury. The coil of wire acts as a spring, and the slightest upward motion of the earth is sufficient to cause the point to dip into the cup of mercury. This completes a galvanic circuit, which stops a clock at the exact half second at which the shock occurred, and rings a bell to call the observer, and also does other work which we shall speak of again. There are three or four helices of wire of different strengths, which support small magnets above a cup of iron filings. When a vertical shock occurs some of these magnets dip into the iron filings. To one of these a light index is attached for measuring the intensity of the shock.

For horizontal shocks there are four glass tubes. Each of them is bent twice at right angles, so as to form a U tube. One arm of this tube has more than double the diameter of the other, and is shorter. The four tubes point inthe directions of the four cardinal points. Each tube has a certain quantity of mercury poured into it, and on the surface of the mercury, within the narrow arm of the tube, there rests a small weight attached to a silk fibre, which passes over a delicate ivory pulley, and has a counterpoise attached at the other end. Each pulley has an index and circular scale to mark the angle turned through. The extremity of a wire is fixed at a small distance above the surface of the mercury in each tube. If then a horizontal shock occur, the mercury rises in the corresponding tube; but it rises higher in that one which has its long arm to the north. The pulley is turned through a certain angle, which is measured by the index, and at the same time the mercury in rising comes in contact with the fixed wire, and so completes a galvanic circuit which rings a bell, and stops the clock at the exact half second when the shock occurred. If the shock comes from some intermediate point two of the indices will be moved, and the direction and intensity can be measured by observing both of them. We have seen up to this point that the instrument will measure the direction and intensity of a shock, will mark the time at which the shock occurred, and will ring a bell to attract the attention of the observer on duty, who may register succeeding shocks, or, if the earthquake has ceased, may reset the apparatus. But this is not all. The galvanic circuit, which is completed at the moment a shock occurs, releases at the same instant the pendulum of a second clock, which has been held out of the vertical by means of a detent. This clock allows a roll of paper to be unwound off a drum, as in any registering telegraph, at the rate of three metres an hour. A pencil rests nearly in contact with the strip of paper. It is connected with one arm of a lever, the other arm of which is slightly distant from an electro-magnet. As often as the current passes this end of the lever is attracted to the magnet, and the pencil in consequence is made to press on the paper, to be released only when the current ceases. By this means then a continuous history of the earth's trembling is registered, a pencil mark corresponding to a time of trembling, and a blank space to a period of cessation.

This instrument is extremely delicate, and registers motions of the earth which are too slight to be perceptible to the human frame. When we examined it some one happened accidentally to touch the casing of the instrument. The alarm was immediately given by the bell, and the two clocks were respectively checked and put in motion by the galvanic current.

The accompanying figure (borrowed from the *Engineer*, for the use of which we are indebted to the courtesy of the editor of that journal) may help to make the above description more intelligible. In Fig. 1, the clock A is shown with the pendulum arrested, as after a shock has occurred. The pendulum of the clock B is in a position ready to be set free when a shock occurs. At the same time the strip of paper *k k k* will be rolled on to the cylinder *i*, and at each trembling of the earth the electro-magnet D will cause a pencil to make a mark on the paper at the point *m*. P and R are two pillars, between which are shown the U tubes containing the mercury, the pulleys and indices are shown above. These pillars and tubes are also shown in plan. Metallic bars are seen connected with R, and passing over the short arms of the tubes. From these hang the wires that dip into the mercury. From the pillar P, metallic bars are also shown passing over the long arms of the tubes; to these are attached the wires that are almost in contact with the mercury, and which complete the circuit when a shock occurs. The metallic spring E, supported by the pillar T, above the cup of mercury *f*, is the apparatus for making a current during a vertical shock; *h h h* are the springs with magnets attached, which dip into iron filings. The index for vertical shocks is shown more clearly in Fig. 2.

For more violent shocks the heavy bob of a freely suspended pendulum is placed in the centre of a horizontal ring in which eight tubes are placed lightly, all pointing to the centre. From whatever direction a horizontal shock comes it will drive out one of these tubes. The tube driven out will show the direction of the shock, and the distance to which it is driven will show the intensity. This is also shown in plan (Fig. 3). The hole for the tube is also represented. There is also shown in section a cup of mercury, placed at the foot of the pillar G (Fig. 1), which has eight holes in its circumference just above the surface of the mercury. When a shock occurs mercury is driven out into the hole corresponding to the direction of the shock. The quantity of mercury determines the intensity. The battery is shown in Fig. 4, and needs no explanation.

In the same room there is apparatus for detecting and measuring atmospheric electricity. A gold leaf electroscope and a bifilar electrometer are observed regularly. These are successively put in connection with the conductor. This consists of a disc of metal above the roof of the house connected with an insulated metallic rod, supported vertically, and capable of being rapidly raised by means of a cord passing over a pulley. When not in use this rod is in connection with the ground. In making an observation the rod with the disc attached is quickly raised, thereby disconnecting it from the ground. The electricity of the atmosphere at the point where the disc is fixed affects the electroscope and electrometer. Prof. Palmieri prefers the conductor above-described to a conducting point or a flame, because he considers that these do not give comparable results, an objection which is not supported by all observers. He considers the same to be true of the method of dropping water.

After having made careful observations on atmospheric electricity for about a quarter of a century in a country where meteorological changes are more regular and less capricious than in our own island, there is no one whose deductions are more deserving of our attention; the more so as he considers that he has combined his researches into a definite law. His first fact is this:—*If within a distance of about fifty miles there is no shower of rain, hail, or snow, the electricity is always positive.* The single exception is during the projection of ashes from the crater of Vesuvius. During a shower he finds the following law universally to hold good:—At the place of the shower there is a strong development of positive electricity; round this there is a zone of negative, and beyond this again positive. The nature of the electricity observed depends upon the position of the observer with respect to the shower, and the phenomena will change according to the direction in which the shower is moving. Sometimes negative electricity may be observed during a shower; but this is always due to a more powerful shower farther off. These conclusions have been supported by means of telegraphic communication with neighbouring districts. It appears, then, that except when the moisture of the air is being condensed, there is no unusual development of electricity. These results are in accordance with the experiments of Palmieri and others, which show that

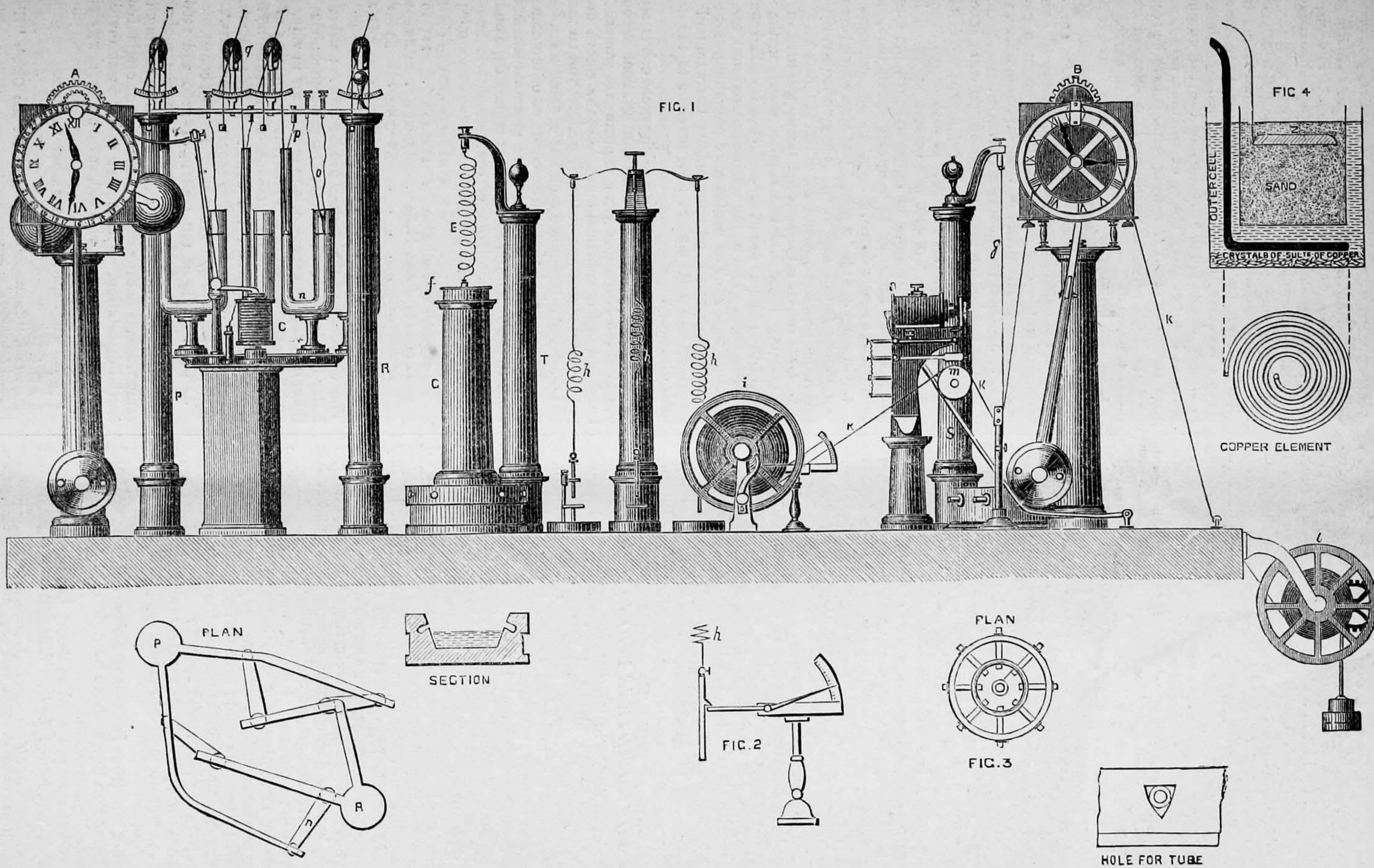


FIG. I
FIC 4
OUTER CELL
SAND
CRYSTALS OF SUL'TE OF COPPER
COPPER ELEMENT
PLAN
SECTION
FIG. 2
PLAN
FIG. 3
HOLE FOR TUBE

aqueous vapour in condensing developes positive electricity. No unusual development of electricity has ever been detected by him in a cloud when no rain is falling.

The above results, though falling short of what has to be done to complete the theory, are yet definite, and hence valuable, the more so if supported by other observers placed in equally favourable situations. But of the variations *in intensity* of positive or negative electricity nothing has been said.

Besides the fixed instruments at the Observatory others are used on the mountain. Gases are collected from cracks in the earth's crust, tubes being let down into them, and the gas sucked up by a kind of bellows to be examined at leisure. A portable spectroscope is also used during eruptions, and there is a larger one by Hoffman in the Observatory. From this Observatory we have received valuable information, and it is much to be regretted that equally efficient observatories have not been established in different parts of the world. Many portable and cheap instruments have been invented, most of which are described by Mr. Mallet in the "Admiralty Manual of Scientific Inquiry;" but there ought to be three or four as delicate as that on Mount Vesuvius. It is a pity that no observatory has ever replaced the ancient one of Empedocles near the summit of Etna, or even at Nicolosi, where the valuable services of Dr. Gemellaro might have been obtained. This would have been the more interesting, as Palmieri can detect shocks caused by that volcano, though the distance is enormous. With a third observatory, say in the Philippine Islands, we could not fail to increase our knowledge enormously.

From long practice Palmieri is able to predict eruptions. We remember well when we were enjoying his hospitality at the beginning of last year how he said, "This is a small eruption, but there is going to be a great one; I do not say it will be soon, it may be a year, but it will come." In almost exactly a year the great eruption did come.

GEORGE FORBES

ON THE DISINTEGRATION OF COMETS

THE main design of the following paper is to present at one view the historical evidence of the gradual disintegration of periodic comets. A few preliminary remarks, however, in regard to the received theory of comets and meteors, may not be destitute of interest.

The fact that in several instances meteoric streams move in orbits identical with those of certain comets was first fully established by the researches of Signor Schiaparelli. The theory, however, of an intimate relationship between comets and meteors was proposed and advocated by the writer several years previous to the publication of Schiaparelli's memoirs. In an article written in July 1861, and published in the "Danville Quarterly Review" for December of that year, it was maintained—

1. That meteors and meteoric rings "are the *débris* of ancient, but now disintegrated comets whose matter has become distributed around their orbits."*
2. That the separation of Biela's comet as it approached the sun in December 1845 was but one in a series of similar processes which would probably continue until the individual fragments would become invisible.
3. That certain luminous meteors have entered the solar system from the interstellar spaces.†
4. That the orbits of some meteors and periodic comets have been transformed into ellipses by planetary perturbation. And—
5. That numerous facts—some observed in ancient and some in modern times—have been decidedly indicative of cometary disintegration.

What was thus proposed as theory has been since confirmed as undoubted facts. When the hypothesis was originally advanced, the data required for its mathematical demonstration were entirely wanting. The evidence, however, by which it was sustained was sufficient to give it a high degree of probability.

* The name of *cometoids* was accordingly proposed for luminous meteors.
† Others, it was supposed, might have originated within the system—a view which the writer has not wholly abandoned.

The existence of a divellent force by which comets near their perihelia have been separated into parts, is clearly shown by the facts enumerated in the following lines. Whether this force, as suggested by Schiaparelli, is simply the unequal attraction of the sun on different parts of the nebulous mass, or whether, in accordance with the views of other astronomers, it is to be regarded as a cosmical force of repulsion, is a question left for future discussion.

1. Seneca informs us that Ephoras, a Greek writer of the fourth century B.C., had recorded the singular fact of a comet's separation into two distinct parts.* This statement was deemed incredible by the Roman philosopher, inasmuch as the occurrence was then without a parallel. More recent observations of similar phenomena leave no room to question the historian's veracity.
2. The head of the great comet of 389 A.D., according to the writers of that period, was "composed of several small stars" (Hind's "Comets," p. 103).
3. On June 27, A.D. 416, two comets appeared in the constellation Hercules, and pursued nearly the same apparent path. Probably at a former epoch the pair had constituted a single comet.†
4. On Aug. 4, 813, "a comet was seen which resembled two moons joined together." They subsequently separated, the fragments assuming different forms.‡
5. The Chinese annals record the appearance of three comets—one large and two smaller ones—at the same time in the year 896 of our era. "They travelled together for three days. The little ones disappeared first, and then the large one."§ The bodies were probably fragments of a large comet which, on approaching the sun, had been separated into parts a short time previous to the date of their discovery.
6. The third comet of 1618.—The great comet of 1618 exhibited decided symptoms of disintegration. When first observed (on November 30), its appearance was that of a lucid and nearly spherical mass. On the eighth day the process of division was distinctly noticed, and on the 20th of December it resembled a cluster of small stars.||
7. The comet of 1661.—The elements of the comets of 1532 and 1661 have a remarkable resemblance, and previous to the year 1790 astronomers regarded the bodies as identical. The similarity of the elements is seen at a glance in the following table:—

	Comet of 1532.	Comet of 1661.
Longitude of Perihelion	111° 48′	115° 16′
Longitude of Asc. Node	87 23	81 54
Inclination	32 36	33 1
Perihelion Distance	0·5192	0·4427
Motion	Direct	Direct

The elements of the former are by Olbers; those of the latter by Mechain. The return of the comet about 1790, though generally expected, was looked for in vain. As a possible explanation of this fact it is interesting to recur to an almost forgotten statement of Hevelius. This astronomer observed in the comet of 1661 an apparent breaking up of the body into separate fragments.¶ The case may be analogous to that of Briela's comet.

8. The identity of the comets of 1866 and 1366, first suggested by Prof. H. A. Newton, is now unquestioned. The existence, then, of a meteoric swarm, moving in the same track, is not the only evidence of the original comet's partial dissolution. The comet of 1866 was invisible to the naked eye; that of 1366, seen under nearly similar

* "Quæst., Nat.," lib. vii., cap. xvi.
† Chambers's "Descr. Astr.," p. 374.
‡ Ibid. p. 383.
§ Ibid. p. 388.
|| Hevelius, "Cometographia," p. 341. See also Grant's "History of Physical Astronomy," p. 302.
¶ "Cometographia," p. 417.

circumstances, was a conspicuous object. The statement of the Chinese historian that "it appeared nearly as large as a Tow measure,"* though somewhat indefinite, certainly justifies the conclusion that its magnitude has greatly diminished during the last 500 years. The meteors moving in the same orbit are doubtless the products of this gradual separation.

9. The bipartition of Biela's comet in 1845, as well as the non-appearance of the two fragments in 1865, when the circumstances were favourable for observation, are too well known to require more than a passing notice.

The comet of Halley, if we may credit the descriptions given by ancient writers, has been decreasing in brilliancy from age to age. The same is true in regard to several others believed to be periodic. The comet of A.D. 1097 had a tail 50° long. At its return, in March 1840, the length of its tail was only 5°. The third comet of 1790 and the first of 1825 are supposed, from the similarity of their elements, to be identical. Each perihelion passage occured in May, yet the tail at the former appearance was 4° in length, at the latter but 2½°. In short, instances are not wanting of this apparent gradual dissolution. It would seem, indeed, extremely improbable that the particles driven off from comets in their approach to the sun, forming tails extending millions of miles from the principal mass, should again be collected around the same nuclei.

The fact, then, that comets and meteors move in the same orbits is but a consequence of that disruptive process so clearly indicated by the phenomena described. In this view of the subject, comets—even such as move in elliptic orbits—are not to be regarded as permanent members of the solar system. Their *débris*, however, thus scattered through space, and subject more or less to planetary perturbation, may casually penetrate the atmosphere, producing the phenomena of sporadic meteors.

DANIEL KIRKWOOD

* Williams's "Chinese Observations of Comets," p. 73.

NEWTON'S MANUSCRIPTS AND BIRTH-PLACE

ALL Trinity men will, like myself, regret that Lord Portsmouth's gift, recorded in NATURE of June 6, should have been made to the library of Newton's University instead of to that of Newton's College. Surely for many reasons Trinity library is the most fitting depository for the Newton manuscripts. A catalogue of these papers is given in Collet's "Relics of Literature, 1823," pp. 190-194, consisting of eighty-two manuscripts, said to cover nearly eight thousand pages, mostly quarto or folio, besides six note-books, and many letters to Newton in English, French, and Latin. Unfortunately many of these papers relate to biblical or theological subjects.

When Dr. Pellet, by request of Newton's executors, examined these papers with a view to publication, he condemned all but five. These were :—

α 56 half sheets in folio, *De Motu Corporum.*

β 31 half-sheets in folio, being paradoxical questions concerning Athanasius (*sic*).

γ 12 half-sheets folio, an abstract of chronology, and 92 half-sheets folio, the chronology.

δ 144 quarter-sheets, and 95 half-sheets folio, being loose mathematical papers.

ϵ 40 half-sheets folio, the "History of the Prophecies," in ten chapters, and part of eleventh unfinished.

Of these γ was to have been printed, and α, β, and parts of δ and ϵ were to be reconsidered.

While on this subject, permit me to add an account of the present state of Newton's birthplace sent me by a lady at Stoke Rochford, where Newton attended a dame's school before going to the free school at Grantham :—

"Woolsthorpe, the birthplace of Sir Isaac Newton, is about half-a-mile westward from Colsterworth, and nine miles from Grantham. It has been thoroughly repaired by its present owner. At the top of the staircase, in a room to the left, Newton was born on Dec. 25, 1642. Over the fireplace is a small white marble slab recording the fact, with the well-known lines parodying Genesis i. 3. The only things in this room which remain unchanged since Newton's time are the door, which is massive, and rather ornamental in its workmanship, and a small cupboard close to the fireplace, the door of which is curiously carved. In another room a singular piece of furniture, made of wainscoting, stands in one of the corners, which looks like a small apartment taken from the main room. It is said to have contained Newton's books, instruments, &c. Above the door in front there is a shield with cross-bones, and a few words to denote that the house was the birthplace of Newton. The sun-dial which Newton made and put upon the south side of his house was sent to the British Museum some thirteen or fourteen years ago."

C. M. INGLEBY

NOTES

WE are informed that Dr. Sharpey, who has for so many years filled with such great advantage to Science and personal distinction the post of Biological Secretary to the Royal Society, has recently sent in his resignation of that appointment. There is a very general hope among Fellows of the Royal Society that Prof. Huxley may allow himself to be nominated as his successor.

AT the meeting of Convocation of the University of Oxford, held last week, the honorary degree of D.C.L. was conferred on the following gentlemen :—Samuel David Gross, M.D. and LL.D., Professor of Surgery in the Jeaffreson Medical College of Philadelphia ; Sir Benj. Collins Brodie, Bart., M.A., F.R.S., late Waynflete Professor of Chemistry ; George Burrows, M.D., of Caius College, Cambridge, F.R.S., President of the Royal College of Physicians of London, and formerly President of the General Medical Council.

THE choice of the electors of the Waynflete Professorship of Chemisty at Oxford, vacant by the resignation of Sir B. C. Brodie, has fallen on Prof. Odling, F.R.S., who at present holds the position of Fullerian Professor of Chemistry to the Royal Institution, and Examiner in Chemistry to the University of London.

MR. EDWIN RAY LANKESTER, B.A., late junior student, Christ Church, has been elected to a Natural Science Scholarship at Exeter College, Oxford. Mr. Lankester was elected to the Burdett Coutts Scholarship in 1869, and to the Radcliffe Travelling Fellowship in 1870. There were four candidates.

IN accordance with the intimation which we gave last week, Mr. G. B. Airy has been gazetted a K.C.B., a graceful acknowledgment of the claims of representative men of science to recognition by the State.

WE have to record the death, on the 16th inst., in his 82nd year, of Colonel W. H. Sykes, F.R.S., M.P. for Aberdeen. He was a distinguished officer of the East India Company, and occupied the post of chairman of its Board of Directors at the time of the surrender of its Imperial functions. Colonel Sykes was always a firm friend to scientific research, and was himself possessed of no mean scientific attainments.

MESSRS. C. F. J. YULE and W. J. Sollas have been elected to Foundation Scholarships for proficiency in Natural Sciences at St. John's College, Cambridge. Each has been twice placed in the first class in the College Examination in Natural Sciences, and Mr. Sollas obtained in 1870 the exhibition of 50*l.* per annum offered by the College for competition to students in Natural Science not yet members of the University.

THE following is the list of those who have passed the special examinations in Natural Sciences for the ordinary B.A. degree at the University of Cambridge:—Chemistry—First Class: Porter, Clare; Loder, Trinity; Dent, Trinity; Corbet, St. John's. Second Class (in alphabetical order): Flood, Jesus; Savary, Trinity; Thomas, St. John's; Winder, Christ's. Geology—First Class: Jesson, Trinity. Botany—First Class: Smith, Christ's; Gibb, Down; Standert, Corpus; Norcock, Corpus. Second Class (in alphabetical order): C. W. H. Evans, Caius; Gibson, Christ's; Hamilton, St. Peter's; Hughes, Clare; Norton, Clare; Tamberlain, Trinity. Ægrotat: Moore, Corpus. Zoology—First Class: Bird, Trinity. Second Class (in alphabetical order): Campbell, Caius; Clutton, Clare; Leatham, Trinity.

SCIENCE has sustained a great loss in the West of England by the death of Mr. J. S. Enys, of Enys, near Penryn. In conjunction with Sir Charles Lemon and Mr. Davies Gilbert, he assisted science in every way that one of the largest landed gentlemen in the county could do. The Falmouth Polytechnic, and most others of the Cornwall scientific institutions, will miss the support which Mr. Enys so largely and constantly afforded them. Many of his geological and kindred works have been printed in the "Proceedings of the Royal Cornwall Institution," and by liberal subscription he assisted scientific periodical literature. He died, an ardent nature-worshipper, in his 76th year.

MR. GREGORY, the new Governor of Ceylon, we are happy to hear, not only takes great interest in the wonderful archæological treasures of the island, but also intends to do his best to promote the course of science there. A regular curator, a zoologist, will probably be appointed to the Colombo Museum, and if he does half as much work amongst the fauna of Ceylon as Mr. Thwaites has done amongst the flora, biological science will profit in no small degree thereby. We wish Mr. Gregory all success, and hope he may secure a good man for the post.

THE great provincial meeting of the Royal Horticultural Society will be held next week at Aston, near Birmingham, and promises to be a brilliant one. The proceedings will be opened on Tuesday, the 25th inst., by H.R.H. Prince Arthur.

PROF. HUMPHRY commenced his course of three lectures on Human Myology at the Royal College of Surgeons on Monday last. He spoke of the various provisions, such as obliquity of the direction of fibres, insertion near the centre of motion, passage through loops, &c., which are for the purpose of lessening the range of muscular action required, and so of shortening the fibres of muscles. The mechanical disadvantage resulting from this is, he observed, more than compensated for by the greater number of fibres brought to bear by means of tendons upon given points, and by the convenience of massing the fibres in certain positions, as well as by greater strength in the muscle itself. He next entered into the morphology of the muscles of the trunk, neck, and head. All of these he regarded as modifications of, or derivatives from, that structure which in the fish forms the great lateral muscle; and he showed how the parts corresponding with the septa of the lateral muscle sometimes form inscriptions in the muscles, and sometimes become converted into tendons, as well as into osseous and cartilaginous structures. This subject was discussed at considerable length. In speaking of the intercostal muscles, he gave mechanical reasons for believing that both the external and the internal intercostal fibres are, in their whole extent, agents in inspiration, and that the internal intercostals do not, as they have been supposed to do by some authors, act as depressors of the ribs at parts of the intercostal spaces.

A PAPER will be read at the Horological Institute on Tuesday, June 25, on "The Compensation and Adjustment of the Hemispherical Cup Arms of Velocity Anemometers," by Mr. John James Hall, F.M.S.

THE Franklin Society of Mobile was organised in the year 1835, under charter of the State of Alabama, for the purpose of promoting intellectual culture, literary taste, and other kindred objects. Its operations were suspended a little before the outbreak of the late American war, in consequence of the destruction of its hall, furniture, and a part of its library by fire, and have only recently been resumed. A suitable building has been purchased and remodelled, and arrangements are in progress for the inauguration of lectures and for the extension of the privileges of the Society's library beyond its own membership to the public at large. The library, however, is as yet but limited, and the ability of the members of the Society has been well nigh exhausted in the expenses incident to the purchase and fitting up of a new building. At a recent meeting it was resolved that a committee be appointed to correspond with the officers of similar societies in the several States, and with such other persons as the committee may think proper to address, and to request donations or loans of books, manuscripts, paintings, engravings, or other works of art. We commend the movement to those who have the means and inclination to contribute to objects of this nature. Shipments may be made to the "Franklin Society, Mobile, Alabama," the freight on which will be paid by the Society; or parcels may be sent to W. E. Mickle, Secretary Mobile Franklin Society, care of E. Stock, 62, Paternoster Row.

THE Austrian Government steamer *Admiral Tegethoff* sailed from Bremen on Thursday last, on its North Pole expedition. A farewell banquet, at which Count Zichy and Dr. Petermann were present, was given to the members of the expedition at Geestemunde.

IT is reported that the Emperor of Russia is projecting the junction of the Black Sea with the Caspian by a short canal connecting the Manutch, an eastern tributary of the Don, with the Kerma, a river running into the Caspian. The total length of the communication will be 680 versts, or 90 German miles; but the length of the canal will be only about one German mile. The piercing of the mountain which separates these rivers will, however, be an engineering work of gigantic magnitude, and is calculated to require the labours of 32,000 workmen for six years, and to cost 81,000,000 roubles.

PROF. DAWSON, F.R.S., delivered the Annual Address to the Natural History Society of Montreal on May 18. In it he strongly attacked the Darwinian theory of Evolution, which, in its extreme form, he considered had a tendency to "prostitute natural history to the service of a shallow philosophy," and to lead to "the destruction of science, and a return to semi-barbarism." He held that his researches on the shells of the Gulf of St. Lawrence and the coasts of Labrador and Greenland, showed that it was impossible that any changes of the nature of evolution were in progress; but that all these species had remained the same, even in their varietal changes, from the post-pliocene period till now. Principal Dawson then referred to the controversy raised by Dr. Sterry Hunt with regard to the use of the names Cambrian and Silurian in geology; and concluded with a sketch of the recent operations of the society in dredging the Gulf of St. Lawrence.

THE members of the Australian Eclipse Expedition, if they were unsuccessful in the primary object of their voyage, saw some strange things along the shores to the north of the great continent of Australia. Mr. Foord tells a wonderful story, "amply attested by witnesses," of a fish with four hands. This extraordinary creature was found crawling on a piece of coral dredged up from the bottom of the sea. "The body was that of a fish," says Mr. Foord before the Royal Society on January 22, "but wonderful to relate, it had in the place of fins four legs terminated by what you might call hands, by means of which it made its way rapidly over the coral reef. When placed on the sky-light of the steamer, the fish stood up on its four legs, a sight to behold! It was small, and something like a lizard, but with the body of a fish!" It is to be hoped that a full and scientific description of this latest marvel of deep-sea dredging may soon be published, as the specimen appears to have been brought back to Melbourne. Mr. White, too, of the same Expedition, tells strange tales about the rats. "The little island," he said, "upon which we pitched our tent was overrun with them, and what was most extraordinary, they were of every colour from black to yellow, and some tortoise shell!"

AMONG other collections made by Prof. Marsh during his explorations in 1871 were additional specimens of the pterodactyl, first obtained in 1870. Portions of five individuals were procured; and among them nearly all the bones of the right wing of one, which exhibited the pterodactyl structure in its perfection. The teeth found with the other remains were somewhat similar to those of the pterodactyls of the Cretaceous of England, being smooth, compressed, elliptical, and somewhat curved. A second species, still larger than the other, was obtained in the Upper Cretaceous, near the Smoky River, in Western Kansas. The expanse between the tips of the fully-extended wings was probably as much as twenty-two feet. In all, Prof. Marsh has determined the existence of three species from the same region, which he characterises in the April number of the *American Journal of Science*. In the same journal Prof. Marsh refers to the interesting discovery that the body of mosasauroid reptiles was probably covered with plates, as in some crocodiles, the head itself being smooth. This fact has been ascertained in regard to specimens of all the American genera, so that probably all the species possessed it.

THE second Report of the Geological Survey of Indiana, made during the year 1870, under the direction of Mr. E. T. Cox, State geologist, has just made its appearance, and, like its predecessor, appear to be a work of much scientific value. In addition to the series of reports upon the geology of the counties, it embraces a paper upon the Western coal measures and Indiana coal, and a paper upon palæozoic zoology, and closes with an extended manual of the botany of Jefferson County, Indiana, prepared by Prof. A. H. Young, of Hanover College. In this the total number of indigenous species is given at 537, those introduced numbering 72.

ANOTHER book of excellent typographical execution has just appeared from the public printing-office of the United States, in the form of the astronomical and meteorological observations made at the United States Naval Observatory during the year 1869, under the direction of the superintendent, Admiral B. F. Sands. This volume, forming a stately quarto of over 900 pages, is prefaced by a detailed account of the transit circle, the meridian transit instrument, the mural circle, and the equatorial of the observatory, and followed by a statement of observations made with these instruments. The volume also contains the meteorological observations for 1869, the positions of the sun, moon, and planets during that year, as made with different instruments, &c. The report of the total eclipse of December 22, 1870, which has already appeared as a separate memoir, is included in this volume, as also an appendix embracing the zones of stars observed with the mural circle in the years 1846, 1847, 1848, and 1849. The observatory is now in excellent condition, and includes in its working force some of the best astronomers and mathematicians of the country; among them Profs. Newcomb, Hall, Harkness, Eastman, &c. The completion of the gigantic telescope now in process of construction by Alvan Clark will constitute an important addition to the means of research, and will doubtless be turned to good advantage.

ON THE SOUND MADE BY THE DEATH'S HEAD MOTH, "ACHERONTIA ATROPOS"

THE singular cry produced by the Death's Head Moth has for a long time been known to naturalists, and the question of the exact method of its production has given rise to much discussion. To judge, however, from the latest writings on the subject, the matter is considered even now as being far from definitely elucidated. In the autumn, about six years ago, I was lucky enough to rear over a hundred imagos of *Acherontia atropos* from pupæ obtained from potato diggers in the neighbourhood of Bristol. I made then some observations on the production of the sound in question, but I did not consider them as sufficiently perfect for publication. I got no more specimens until last autumn, when I obtained a single imago from ten pupæ, but on this I made an experiment which I believe to be crucial in the matter. Absence from England, however, on the Government Eclipse Expedition, has prevented my giving an account of my experiment until now.

On looking into the literature of the subject, in which task I have been kindly assisted by Prof. Westwood and Prof. Rolleston, I found it in its extent far exceeding my expectations. The number of theories which have been invented to account for this apparently simple phenomenon is astonishing; and as the history of the question is really very interesting, I shall commence by giving as complete an account of what has been written on the subject as I have been able to obtain by reference to works in the Radcliffe, Bodleian, and Linnean Society's libraries, and in that of Prof. Westwood.

The earliest writer on the subject was Reaumur (Mémoire pour servir à une Histoire des Insectes, 1734-1742, vol. i. pl. 14), who suggests that the noise is most probably due to the same cause as in certain scarabei, which produce a sound by the rubbing together of certain of their scaly parts. Later on (*loc. cit.* vol. ii. p. 24) Reaumur states that he has made further experiments, and concludes that the sound is produced by the rubbing of the proboscis against the palps. He held the palps aside from contact with the trunk, and the sound ceased. But he is of opinion that air may have something to do with the matter, and makes his statement with caution. There is a membrane stretched at the base of the trunk, he says, which may have something to do with it; and finally, "Je ne me lasserai point de répéter que nous dévons nous attendre, que dans les plus petits sujets il restera toujours quelque chose que nous ignorons." It would have been well if some of the many subsequent writers on the subject had profited by this sage remark. The next authority is Roesel (Insecten Belustigung: Nürnberg, 1755, § 16), whose observations, according to Wagner, were very superficial; and who considers the sound due to friction between the opposed surfaces of the abdomen and thorax. Next comes Rossi (Istoria della Farfalla a testa di Morto, Opuscoli di Milano, Ann. 1782) who is the first to arrive at a correct result, and says the sound is due to expiration of air through the trunk. Schröten (Der Naturförscher, xxi. Stück: Halle, 1785) gives as a cause the rubbing of the trunk against the head. Engramelle, as quoted by Passerini, without special reference, makes the sound come from the part of the insect called the spallette.

There is now a considerable chronological interval, and then Godart and Dupronchel (Hist. Nat. des Lépidoptères de France. par M. Godart. tom. iii. pp. 18 et 19) report a letter from M. Lorey, retired army surgeon-major, who describes a peculiar pair of organs, situate on the sides of the abdomen, surrounded by long hairs, which, when the animal squeaks, may be seen to elevate themselves, and form a conical cavity leading to an opening. He considers the sound to be produced by the passage of the air through this opening. It will be seen further on that this peculiar pair of organs was subsequently described as a discovery by two writers ignorant of the literature of the subject.

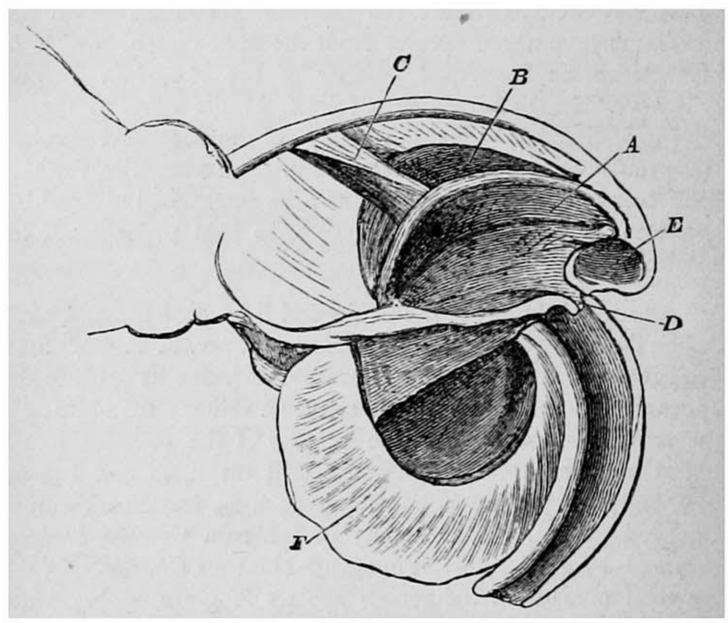
Next comes Passerini, who went to the very root of the matter, and explained the whole thing correctly and clearly in a monograph entitled, "Osservazioni sopra la Sphinx Atropos o Farfalla a testa di Morto," del Dottore Carlo Passerini: Pisa, 1828. He commenced his experiments in 1824. He first disposes of Lorey's theory by showing that his peculiar organs exist only in the male insect, whereas both male and female Death's Heads produce the sound; and further, that the same organs exists in the males of other moths, as *Macroglossa stellatarum*, and *Sphinx convolvuli*, which produce no sound. Next he records this startling experiment:—A moth may be divided in two through the middle of the thorax, and the anterior extremities will still continue to speak. This experiment disproves absolutely all theories which connect the sound with the abdomen in any way, by friction or otherwise. He next removed successively from a moth the palps, the trunk, and the spallette, and the insect nevertheless continued to squeak. He then cut away carefully with a sharp knife the horny top of the head of another specimen, and observed certain muscles rising and falling in rapid motion when the animal squeaked, but remaining quiescent as soon as the sound ceased. As long as these muscles were left intact the insect might be mutilated in almost any manner without the sound being stopped. If these muscles were divided longitudinally, or transversely, the power to emit sound was lost. In quite a fresh specimen, he says, in examination after death, the upper part of the head is found filled with an indurated cellular structure, and beneath this are found the elevating and depressing muscles. Beneath these is an inclined shining horny surface of triangular form leading to a narrow transverse aperture corresponding with the opening of the proboscis tube. At the back of the incline is a very fine aperture, leading into the body of the *Sphinx*. He concludes that the air enters the cavity of the head by the fine aperture, and is driven out through the narrow transverse one by the action of the muscles just described, and that thus the sound is produced.

Chavannes wrote on the subject (Act. Soc. Helv. Sc. Nat. 17; Isis: Genève, 1832, pp. 93-94), but I have not been able to refer to his memoir, nor to that of Rochebrune (Act. Soc. Scient. Bordeaux, 1832, t. 5, pp. 120-122, tab. 1.) Then we have H. Burmeister (Handbuch der Entomologie, Berlin, 1832), who after citing Reaumur and Rossi says, "This much is certain, the organ of voice is seated in the head." Then Vallot (*L'Institut*, 1834, II., No. 34, p. 7), who demolishes to his own satisfaction all previous theories, and adopts that of Johet, which considers the sound to be produced by the striking of the wings in rapid movement against certain parts. Wagner (Müller's Archiv für Physiologie, III., 1836, pp. 60-62), after satisfying himself that the sound came from the head, unrolled the trunk, and found that the sound became feebler, but did not cease. When he held the two halves of the trunk apart, or cut off one or both up to the base, the sound ceased. He found just in front of the true stomach a crop very large, distended with air, and filling up the whole anterior part of the abdomen, and opening into the posterior extremity of the œsophagus. He could not find any special apparatus in the trunk, but he says that there appears to be a fine slit-like opening at the basis of this organ on its inferior surface, formed by the imperfect closure of the two halves of the trunk. This may have something to do with the matter. He could not find Passerini's cephalic cavity. The proboscis has strong, transversely-striated muscles. Duges (Traité de Physiologie comparée, par A. Duges: Montpellier, 1838, vol. ii. p. 226) ascribes the noise to the rubbing together of the opposed edges of the two halves of the proboscis.

We have theories already in abundance, and there are more yet to come; but we now come on repetitions of previous theories by persons who either had not access to, or were too lazy to consult, the writings of the original inventors of them. Dr. Alex. V. Nordman (Bull. Acad. St. Petersburg, 1838, t. 3, pp. 164-193) says the sound is seated neither in head nor proboscis, but in the abdomen; and he then proceeds to give an elaborate description of the peculiar organs already discovered by M. Lorey, and he congratulates himself not a little on his having been able, after the failure of so many previous investigators, to make this discovery, and to set the matter finally at rest. We next have observations and experiments made by MM. Duponchel and Guerin (Ann. Soc. Ent. France, 1839, t. 8, pp. 59-65). To show that Passerini was wrong, they compressed the trunk at the base sharply with forceps, and also stopped the end of the proboscis, but apparently in a very insufficient manner. The noise did not cease. They had only one individual on which to operate, and therefore could not afford to mutilate it and repeat Passerini's experiments. Their conclusion is that the noise has the nearest analogy to that emitted by Longicorn beetles, which is produced by the rubbing of the prothorax against the smooth portion of the scutellum. Goureau (Ann. Soc. Ent. France, 1840, t. 9, pp. 125-128) says, "Reaumur, Passerini, Lorey, all are wrong. There is no special organ for the sound. The sound is of a double nature. There is one shrill part of the sound which proceeds from the vibrations of the thoracic rings; in another part a grating sound is caused by the rubbing of the shoulders against the thorax." Abicot (Ann. Soc. Ent. France, ser. 2, 1843, t. 1, Bull. p. 50) says that amputation of the trunk stops the noise, therefore Goureau is wrong.

Ghiliani (Ann. Soc. Ent. Fr., ser. 2, 1844, t. 2, Bull. pp. 72, 75) confirms Passerini. He cut off the head and the sound stopped. He held the trunk horizontally, and at various inclinations, the sound continued; he removed the palps, the sound continued; he dipped the head in oil, the sound ceased. He then amputated the trunk at its root. Three apertures were formed by the operation, one corresponding to each half of the proboscis, the other probably to the prolongation of the buccal cavity situate just above the trunk, and opened by the knife when amputating the trunk. A green liquid flowed from the third opening over the cut root of the trunk, in which liquid large bubbles were seen to be formed by the expiration accompanying each sound. On closing the apertures the sound ceased. He is unable to explain how inspiration and expiration are managed. Paris (Ann. Soc. Ent. France, ser. 2, 1846, t. 4, Bull. pp. 96, 142) thinks the noise due to a mucous fluid which the insect forces by aspiration, with the assistance of the palps up and down inside the proboscis, comparing the process to the flow and reflow of a liquid in a suction and force-pump.

J. Vander Hoeven (Tydschr. Entom. Neder Vereen, 1859, t. 2, Stück 4, pp. 117-122) says the source of the sound is certainly in the head. In an Indian species of *Acherontia* he has found tuberosities all over the basal portion of the trunk. The exterior surface is beset with stiff bristles. On rubbing this surface against the edge of a piece of paper or scalpel, a sound like that of the



Vertical section through the median line of the head of *A. atropos*, from a specimen preserved in absolute alcohol. *A*, dome-shaped cavity of Passerini. *B* and *C*, depressing and elevating muscles of this cavity. *D*, narrow orifice, leading from the cavity to the tube of the proboscis. *E*, palpus.

moth is produced. He considers the moth's cry to be due to friction of this kind.

Westmaas (Tydschr. Entom. Neder Vereen, 1860, t. 3, pp. 120-124) extended the trunk on a pin, and at the same time pulled the palps aside. He still heard the sound, though it became feeble. He cut off the palps without effect on the sound. He cut off the trunk bit by bit, as it was gradually shortened the sound diminished. A fluid exudation covered the end of the trunk, and he saw bubbles of air formed in this as each sound was emitted. He confirms Passerini. He notices that the insect produces a louder sound when the trunk is coiled up. The moth when emitting a sound elevates the front of its body, and uses an evident effort. He stopped the aperture of the trunk with grease, the crying ceased; he kept one specimen half a day with the trunk aperture thus closed, it emitted no sound, but on the grease at last being removed squeaked at once. In order to test Wagner's theory, he squashed out the abdomen quite flat, so as to destroy any air cavity it might contain; the animal still squeaked. Chapronnier (Ann. Soc. Ent. Belg. *Comptes Rendus*, pp. 16, 17) says that the noise produced by the larva is due to the snapping together of the mandibles. An imago, which he bred, which had a deformed head, emitted no sound; therefore the sound organ is situate in the head.

Finally, we find some discussion of the matter in the *Entomologist*. Edward Newman (*Entomologist*, August 1865, p. 284), says, "With regard to the sound produced by the pupa and imago, A vast deal has been written, but nothing worth repeating." But he surely cannot have sufficient grounds for this sweeping demolition of previous authors, and certainly cannot have consulted this "vast deal which has been written;" for he proceeds to enunciate the very theory first put forth in the year 1755 by Rossel, and subsequently revised by Duponchel in 1839.

E. A. Johnson (*Entomologist*, Nov. 1865, p. 325, *Field* newspaper, Oct. 24, 1865) describes the organs discovered by M. Lorey before the year 1828, and subsequently by Nordman, in 1838. W. H. Taylor (*Entomologist*, Nov. 20, 1865) brings forward Passerini's objection to Lorey's theory, viz, that males only have these organs, while both sexes squeak, and that other species which produce no sound possess these organs. Thus does history repeat itself. In the same page of the same journal we have some remarks on the subject from the Rev. A. Preston, who, when sticking his pin-point into an insect in killing it, and moving the pin up and down against a muscle, heard a sound just like that of the mouth. He does not, however, from this observation draw any very definite conclusion.

It will be found that there are eleven distinct theories in all which have been put forward to account for the cry of this moth. By far the greater number attribute the phenomenon to friction of various parts, two to an expiratory effort, one to rapidly repeated percussion, and one to the intervention of a fluid.

The foregoing contains the pith of all the writings on the subject which I have been able to meet with. I have no doubt there are many others in existence, but I hardly expect that I have missed any of importance.

I am not aware whether all known species of *Acherontia* emit a sound; but a closely allied species in Ceylon is described by Sir Emerson Tennant, in his "Natural History" of that island, as doing so; and I was lately, whilst in Ceylon, informed by several residents that such was the case. This Ceylon species is probably *Sphinx Acherontia Lethe*, described by Prof. Westwood in his "Oriental Entomology," p. 87.

I now pass to my own observations on the subject. I was killing a specimen out of the large number which, as stated above, I bred about six years ago, by means of a solution of cyanide of potassium, which I was using with a pen in the ordinary manner, the animal squeaking loudly under the operation. A drop of the fluid happened to fall on the extremity of the proboscis. I noticed that at each squeak a large bubble was formed, showing a forcible expiration from the organ. I repeated this experiment constantly with water, and always with the same result. I further extended the trunk with a pin during the emission of the sound, and noticed a modification in the tone of the cry, which varied with the amount of extension. These experiments convinced me that the sound came from the proboscis, and was produced by an expiration. I at that time knew nothing of the literature of the subject, and very little of anatomy, and I unfortunately made no further observation or experiments in the matter; and it was not till last autumn that I was able to pursue the subject further on a single specimen which I was fortunate enough to rear. This specimen was a very lively one, and it squeaked freely. I placed a tight ligature on the extremity of the proboscis whilst it was in the act of emitting its cry. The noise stopped instantly. I kept this specimen two days, and handled it constantly, bullying it in all sorts of ways to try and get it to squeak, but without result. After the proboscis had been thus ligatured for two days, I amputated the lower portion of the trunk just above the ligature. The animal instantly began squeaking, and continued to do so at intervals for two days more, when I killed it in order to examine the anatomy of its head. I fancied my experiments at the time quite novel, and it was only the other day that I found that similar ones had been made by Ghiliani and Westmaas; but the method of ligaturing the proboscis, with subsequent amputation above the ligature, is, I think, more satisfactory than Westmaas's use of wax.

After these experiments, taken in confirmation of what has already been done in the matter, I think there can be no doubt that the sound is produced by expiration through the proboscis. We have now to consider—How is this expiration effected? Whence comes the air expired? and Whereabouts in the proboscis or head is the actual spot where the sound is formed? I think it will be found that Passerini's explanation is in almost every respect satisfactory. I had no time to dissect my Death's Head whilst in the fresh state last autumn, but preserved it in absolute alcohol. The accompanying drawing of a preparation made from it may therefore need slight modification on further investigation; but in the main it will be found correct. The figure represents a magnified view of a vertical section along the median line of the head. A is the large dome-shaped cavity, evidently the one described by Passerini, and which R. Wagner could not find. This cavity has a hard chitinous floor, which is prolonged forward so as to project over the proximal extremity of the proboscis (seen here in section with its end amputated), and there ends in a sharp edge, which forms, with the anterior wall of the cavity, a narrow transverse slit, leading to the proboscis tube, just as described by Passerini. Resting on the roof of the dome-shaped cavity are Passerini's muscles, B and C, with some cancellar-like tissue between them and the external wall of the head. It would obviously be easy to expose these muscles as Passerini did with great ease and very little disturbance to the insect's functions, and I cannot see any reason to doubt that they would be found in action just as he describes.

Passerini does not figure the muscles or describe them accurately. He merely calls them elevating and depressing muscles. The muscle C must obviously on contraction raise the dome-shaped cavity, whilst B must depress it. An alternate action of the two muscles would cause the cavity to act as a bellows, and inhale and expire air through that aperture which allowed it to pass and repass most freely. Passerini believed that the air entered the cavity by the œsophageal opening at its hinder part, which he describes as very narrow (*loc. cit.*, p. 6) ("Da tutto cio che ha esposto credo poter concludere che l'aria dalli'nterno della sfinge vien portata alla cavita muscolare della testa per mezzo dell' esilissima tuba"), and was expelled then by the proboscis; but that this is really the correct explanation is scarcely probable. First of all, the posterior opening into the cavity must be very small indeed. I cannot find it at all in the present specimen, and it is highly probable that it may often be aborted altogether, as is the case in most butterflies. Moreover, how should the air get into the œsophagus? Certainly not from the abdomen as supposed by Wagner, for Westmaas showed that the animal could squeak after the abdomen was squashed out flat, and Passerini himself showed that an insect would emit the sound after it had been divided in two through the middle of the thorax, an experiment which of itself is sufficient to overthrow his own view. The opening into the proboscis being by far the largest leading into the dome-shaped cavity, the air probably finds ingress as well as egress by this aperture. And if I remember rightly the bubbles formed on the end of the proboscis in my experiments always collapsed between the squeaks, showing this to be the case. I think that there can further be no doubt that the note is formed at the narrow slit-like opening, the sound being modified by passage through the proboscis tube, and by vibrations therein set up, this latter to account for modifications produced by straightening the trunk or by gradually removing it bit by bit from below (Westmaas).

I further think it probable that there is a movement of the proboscis concerned in the production of the sound. If the base of the proboscis were drawn a slight distance directly forward at each inspiration, the upper opening of that organ would be brought more immediately beneath the narrow passage communicating with the dome-shaped cavity, and the ingress of air would be rendered more free. Then if at expiration the base of the proboscis were retracted again, the aperture of egress would be very much contracted, and the formation of the sound facilitated. It would be interesting to observe whether such a motion of the proboscis takes place. I think I remember to have noticed a slight movement of the trunk during the emission of the sound.

It is most extraordinary that the seat of the sound should ever have been imagined to be anywhere but in the head. One has merely to listen to the animal to detect at once where the sound comes from. It would seem as if many writers on the subject had commenced their observations with a determination to find some other seat for the cry. The animal is a large one, and one could as easily persuade oneself that a mouse's cry proceeded from the tip of its tail as that of a Death's Head from its abdomen. Should I obtain specimens of *A. atropos* this autumn, I hope to repeat Passerini's experiments, and also make certain whether inspiration as well as expiration takes place through the proboscis; I think also that Wagner's narrow slit on the under surface of the proboscis should be experimented on.

H. N. MOSELEY

SCIENTIFIC SERIALS

THE *Geological Magazine* for June (No. 96) opens with an interesting article by Mr. Dyer on some fossil wood from the Lower Eocene of Herne Bay and the Isle of Thanet, in which the author describes and figures the microscopic structure of the wood of a Dicotyledonous tree, showing the peculiar phenomenon known under the name of "tylose."—Mr. G. Poulett Scrope communicates some notes on the late eruption of Vesuvius.—From Mr. T. McKenny Hughes we have a note entitled "Man in the Crag," in which the writer discusses the interpretation to be given to certain crag sharks' teeth with holes bored in their substance, and sometimes through them from side to side, which have been supposed to be the work of human hands. Mr. Hughes is of opinion that there is no evidence to support this opinion, and that the cavities in question have been produced by boring mollusca.—Mr. A. R. C. Selwyn, Director of the Canadian Geological Survey, notices the occurrence of some fine fossil footprints in a stratum of dark shale belonging to the Carboniferous series of Nova Scotia, and these footprints are described and figured by Principal Dawson. The latter writer states that the principal footprints are of two kinds—a large one resembling the form described by him as *Sauropus sydnensis*, but having a strong claw on the fifth toe of the hind foot, which has left its mark strongly impressed upon the slab containing the prints, and a smaller impression, sometimes trifid, but occasionally showing the marks of four or five toes. The former (which he names *Sauropus unguifer*) he thinks may have been made by *Baphetes planiceps*; the latter perhaps by a species of *Dendrerpeton*.—Mr. James Geikie concludes his valuable series of papers on changes of climate during the Glacial epoch, and gives an important tabular view of the Quaternary deposits of the British Islands, with their equivalents in some other countries. Mr. G. H. Kinahan notices the supposed middle gravels of the drift of Ireland. The Rev. O. Fisher describes the occurrence of a worked flint in the brick-earth of Crayford. The Rev. T. G. Bonney has a paper on supposed Ice scratches in Derbyshire, which he regards as slickensides; and Prof. Traquair furnishes a supplementary note on *Phaneropleuron* and *Uronemus*.—Among the notices we may mention an account of the human skeleton lately discovered in a cavern at Mentone.

Revue Scientifique, Nos. 43-50.—No. 43 commences with an article by M. Wolf on the Transit of Venus in 1874, illustrated by five diagrams. Mr. Keith Johnston's paper read before the Royal Society of Edinburgh on the Lake-basin of Eastern Africa is translated. In subsequent numbers we find a continuation of M. Claude Bernard's course of lectures on Animal Heat. A paper presented by M. Ch. Grad to the Geographical Society of Paris on the resources of Alsace. Dr. Günther's paper on *Ceratodus Forsteri* is translated from NATURE. M. Dumas contributes an article on the higher instruction in Agriculture at the Central School of Arts and Manufactures in Paris. M. G. de Morillet on Cave-man; epoch of the Madeleine. M. Grandidier contributes a most interesting series of papers on his scientific voyage to Madagascar. Translation of the chapter on the evolution of religious ideas among savages, from Sir John Lubbock's "Origin of Civilisation." Translation of Captain Noble's lecture delivered at the Royal Institution on the Explosive Force of Gunpowder. Report of the meeting of the Congress of German Naturalists and Physicians at Rostock in Sept. 1871, department of Geography and Chemistry.—In No. 44 is a history of the Observatory of Paris. Biography of M. Pictet by Soret. We have besides in each number abstracts of the proceedings of the various scientific societies: the Académie des Sciences, Académie de Médecine, Société de Biologie, Société Chimique, Société Géologique, Société Botanique, Société d'Anthropologie, and of the foreign scientific societies at Vienna, Berlin, London, Palermo, &c.

THE *American Naturalist* for June does not contain so many original articles as usual. The longest is by Dr. R. H. Ward, on "Students' Microscopes," with particulars of the relative advantages offered by the instruments furnished by different makers.—Mr. J. A. Allen continues his "Ornithological Notes from the West," discoursing this time on the birds of Colorado.—There are two interesting shorter articles: by Mr. B. Pickman Mann, on the "White Coffee-leaf Miner" (*Cemiostoma coffeellum*), so destructive to the coffee culture of Brazil, with a plate; and by Prof. Sanborn Tenney, on the Remarkable Simulation of Death presented by the Hibernation of the Jumping Mouse (*Jaculus Hudsonius*) of the Western States.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 13.—"Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen, and on the Pulse and Temperature of the Body." By E. A. Parkes, F.R.S.

1. The elimination of nitrogen during exercise was unaffected by brandy; and since the experiments led to the same result in the former series during comparative rest, it seems certain that in healthy men on uniform good diet alcohol does not interfere with the disintegration of nitrogenous tissues.

2. The heat of the body, as judged of by the axilla and rectum temperatures, was unaffected by the amount given.

3. The pulse was increased in frequency by four ounces of brandy, and palpitation and breathlessness were brought on by larger doses, to such an extent as to greatly lessen the amount of work the man could do, and to render quick movements impossible. As the effect of labour alone is to augment the strength and frequency of the heart's action, it would appear obviously improper to act on the heart still more by alcohol. In this effect on the heart, and through it on the lungs, is perhaps to be found the explanation of the trainer's rule, which prohibits alcohol during exertion. Whether in a heart exhausted by exertion alcohol would be good or bad is not shown by these experiments; but it can hardly be supposed that to urge a heart which requires rest, as would then be the case, can be proper.

4. It seems clear, from the suddenness with which marked narcotic symptoms came on after the third dose was taken on each day, that the eight hours from 10 to 6 o'clock were not sufficient to get rid of the brandy taken at 10 and 2, and that in fact the body must have been still saturated at 6 o'clock.

The exact amount of brandy which commenced to lessen the labour the man could perform is not shown by these observations, and would require more careful modes of investigation. It was evidently some quantity more than 4 ounces which produced effects sufficiently marked to attract his attention, but I should not wish to affirm that every 4 ounces produced no effect in this direction. The man himself was of opinion that 4 ounces had no influence either way. He was quite certain it did not aid his work, but he could not see that it injured it. The second 4 ounces decidedly produced a bad effect.

5. That neither exercise on water nor on alcohol produced any effect on the phosphoric acid of the urine. The result is in accordance with that of the experiments recorded in No. 89 of the "Proceedings of the Royal Society."

The effect on the free acidity of the urine was also inappreciable. The free acidity may have been a little increased in the brandy period, but the change is so slight as to fall within the limits of normal variation.

The effect on the chlorine was not certain, as its ingress was not sufficiently constant.

As the action of alcohol in dietetic doses on the elimination of nitrogen and on the bodily temperature is so entirely negative, it seems reasonable to doubt if alcohol can have the depressing effect on the excretion of pulmonary carbon which is commonly attributed to it. It can hardly depress, one would think, the metamorphosis of tissues, or substances furnishing carbon, without affecting either the changes of the nitrogenous structures or bodily heat. It seems most important that fresh experiments should be made with respect to its effect on carbon elimination, as without a perfect knowledge on that point the use of alcohol as an article of diet in health cannot be fairly discussed.

Royal Geographical Society, June 10.—Major-General Sir Henry C. Rawlinson, president, in the chair. "On the New Hebrides and Santa Cruz Islands in the South-west Pacific," by Lieut. A. H. Markham. The paper described the topography, volcanic phenomena, and ethnology of these groups of islands, visited by him during the cruise of H.M.S. *Rosario*, under his command, between October 1871, and February 1872. He gave a history of the progress of discovery in this part of the Pacific, commencing from the voyage of Mendaña in 1568. All the various expeditions for three centuries did little more than sail through the groups and have deadly encounters with the natives. The islands lie in N.N.W. and S.S.E. direction, and contain some of the most continuously active volcanoes on the surface of the globe. The volcanic cones may be traced in a linear direction for 600 miles. The islands are remarkable for the absence of coral reefs around them, which is attributed by Dana to the destruction of the zoophytes by the heat produced by submarine eruptions. Lieut. Markham ascended the volcano Gasowa, in the island of Tanna, and watched an eruption from the edge of the crater. During the intervals between the explosions (sounding like broadsides from a line-of-battle ship) the sheets of liquid fire seemed to flow back to three distant openings in the bottom of the funnel-shaped crater; masses of scoriæ were hurled up vertically to a height of 1,000 feet. The Melanesian (black, curly-haired) and Polynesian (straight-haired) races appeared to be curiously dovetailed in their distribution throughout the northern portion of these archipelagos. This was explained, in the discussion which followed, by the Bishop of Lichfield, who gave to the meeting a most interesting account of his own experiences in these islands, and who showed that the wandering Polynesians, who peopled the greater portion of the Pacific area (including New Zealand), had been driven in their canoes by winds on some of the smaller islands of the group.

Geological Society, June 5 —J. Gwyn Jeffreys, F.R.S., in the chair.—1. "Notes on Sand-pits, Mud-volcanoes, and Brine-pits, met with during the Yarkand Expedition of 1870." By Dr. George Henderson. The author described some very remarkable circular pits which occurred chiefly in the valley of the Karakash river. These pits varied in diameter from six to eight feet, and were between two and three feet deep, the distances between the pits being about the same as the diameters. He accounted for the formation of the pits by supposing that the water, which sinks into the gravel at the head of the valley, flows under a stratum of clay, which prevents it from rising; the water in course of time, however, flowing in very varying quantities at different periods, gradually washes away small portions of the clayey band, when the sand above runs through into the cavity thus formed, leaving the pits described by the author. The mud-volcanoes at Tarl Dab he accounted for by supposing that after a fall of rain or snow the air contained in the water-bearing stratum would get churned up with water and mud, and be ejected as a frothy mud, sometimes to a height of 3 ft.; while the brine-pits in the Karakash valley he believed to be formed by the excessive rise and fall in the level of that river at various times, which alternately fills and empties the bottoms of the pits, and the water left in the pits gets gradually concentrated by evaporation until a strong brine remains. Mr. Prestwich pointed out that the pits seemed due to quite another cause than the pipes in the chalk and other calcareous rocks, as they did not appear to arise from erosion by carbonic acid. Mr. Thorp suggested an analogy between the phenomena in Yarkand and those at Nantwich, and thought that the pits might be due to solution of rock-salt below the surface.—2. "On the Cervidæ of the Forest-bed of Norfolk and Suffolk," by W. Boyd Dawkins, F.R.S. The author described a new form of *Cervus* from the Forest-bed of Norfolk, which he based on a series of antlers, and named *C. verticornis*. The base of the antler is set on the head very obliquely; immediately above it springs the cylindrical brow-tyne, which suddenly curves downwards and inwards; immediately above the brow-tyne the beam is more or less cylindrical, becoming gradually flattened. A third flattened tyne springs on the anterior side of the beam, and immediately above it the broad crown terminated in two or more points. No tyne is thrown off on the posterior side of the antler, and the sweep is uninterrupted from the antler base to the first point of the crown. The antlers differ in curvature and otherwise from those of *Cervus megaceros*, but there is a general resemblance between the two animals; and the *verticornis* must have rivalled the Irish elk in size. A second species of deer, the *Cervus carnutorum*, which had been furnished by the strata of St. Prest near Chartres, must be added to the fauna of the forest-bed. The Cervidæ of the forest-bed present a remarkable mixture of forms such as the *Cervus polignacus*, *C. Sedgwickii*, *C. megaceros*, *C. carnutorum*, *C. elaphus*, and *C. capreolus*, seeming to indicate that in classification the forest-bed belongs rather to an early stage of the Pleistocene than to the Pliocene age. This inference is strongly corroborated by the presenee of the mammoth, which is so charactsristic of the Pleistocene age.—3. "The Classification of the Pleistocene Strata of Britain and the Continent by means of the Mammalia." By W. Boyd Dawkins, F.R.S. The Pleistocene deposits may be divided into three groups—1st, that in which the Pleistocene immigrants lived, with some of the southern and Pliocene animals in Britain, France, and Germany, and in which no arctic mammalia had arrived; 2nd, that in which the characteristic Pliocene Cervidæ had disappeared, and the *Elephas meridionalis* and *Rhinoceros etruscus* had been driven south; 3rd, that in which the true arctic mammalia were the chief inhabitants. This third, or late Pleistocene division, must be far older than any prehistoric deposits, as the latter often rest on the former, and are composed of different materials; but the difference offered by the fauna is the most striking. In the Pleistocene river-deposits twenty-eight species have been found, the remains of man being associated with the lion, hippopotamus, mammoth, wolf, and reindeer. On examining the fauna from the ossiferous caves, we find the same group of animals, with the exception of the musk-sheep; and it is therefore evident that the cave-fauna is identical with that or the river strata, and must be referred to the same period. Some few animals, however, which would naturally haunt caves, are peculiar to them, as the cave-bear, wild cat, leopard, &c. The magnitude of the break in time between the prehistoric and late Pleistocene period may be gathered also from the disappearance in the interval of no less than nineteen species. The middle division of the Pleistocene mammalia, or that from which the Pliocene Cervidæ had disappeared, and been replaced by invading temperate forms, is represented in Great Britain by the deposits of the Lower Brick-earths of the Thames Valley, and the older deposits in Kent's Hole and Oreston. The discovery, by the Rev. O. Fisher, of a flint-flake in the undisturbed Lower Brick-earth at Crayford, proves that man must have been living at this time. The mammalia from these deposits are linked to the Pliocene by the *Rh. megarhinus*, and to the late Pleistocene by the *Ovibos moschatus*. The presence of *Machœrodus latidens* in Kent's Hole, and of the *Rh. megarhinus* in the cave at Oreston, tends to the conclusion that some of the caves in the south of England contain a fauna that was living before the late Pleistocene age. The whole assemblage of Pleistocene animals evinces a less severe climate than in the late Pleistocene times. The fossil bones from the forest-bed of Norfolk and Suffolk show that in the early Pleistocene mammalia there was a great mixture of Pleistocene and Pliocene species. It is probable also that the period was one of long duration, for in it we find two animals which are unknown on the Continent, implying that the lapse of time was sufficiently great to allow of the evolution of forms of animal life hitherto unknown, and which disappeared before the middle and late Pleistocene stages. The author criticised M. Lartet's classification of the late Pleistocene or Quaternary period by means of the cave-bear, mammoth, reindeer, and aurochs, and urged that, since the remains of all these animals were intimately associated in the caves of France, Germany, aud Britain, and, so far as we know, the first two appeared and disappeared together, and the last two lived on into the Prehistoric age, they did not afford a basis for a chronology. The latest of the three divisions of the British Pleistocene fauna is widely spread through France, Germany, and Russia, from the English Channel to the shores of the Mediterranean. The Middle Pleistocene is represented by a river-deposit in Auvergne, and by a cave in the Jura, in which the presence of the *Machœrodus latidens*, and a non-tichorine rhinoceros, and the absence of the characteristic arctic group of the late Pleistocene and of all the peculiar animals of the early Forest-bed stage, prove that that era muut be Middle Pleistocene. The early Pleistocene division is represented in France by the river-deposit at Chartres, being characterised by the presence of two non-Pliocene animals, *Trogontherium* and *Cervus carnutorum*. The Pleistocene mammalia of the regions south of the Alps and Pyrenees present no trace of arctic species, the mammoth being viewed as an animal fitted for the climatal conditions both of Northern Siberia and of the Southern States of America. It contains *Elephas africanus* and *Hyæna striata*. The fauna of Sicily, Malta, and Crete differ considerably from that described above, possessing some peculiar forms, such as *Hippopotamus pentlandi*, *Myoxus melitensis*, and *Elephas melitensis*. The Pleistocene mammalia may be divided into five groups, each marking a difference in the climate, the first embracing those which now live in hot countries; the second those which inhabit northern regions, or high mountains, where the cold is severe; the third those which inhabit temperate region; a fourth those which are found alike in hot and cold; and a fifth which are extinct. There were three climatal zones, marked by the varying range of animals. The northern, into which the southern forms never penetrated, the latitude of Yorkshire being the boundary of the advance of the southern animals; the southern, into which the northern species never passed, a line passing through the Alps and Pyrenees being the limit of the range of the northern animals, and an intermediate area in which the two are found mingled together. Two out of the three zones are proved by the physical evidence of the Pleistocene strata.

We see by the discoveries of Dr. Bryce, Mr. Jameson, and others that the Pleistocene mammalia must have invaded Europe during the first Glacial period before the submergence, for the reindeer and the mammoth have been found in Scotland under the deposits of Boulder-clay. Dr. Falconer and others have also discovered the latter animal in the preglacial forest-bed. The Glacial period can therefore no longer be looked on as a hard and fast barrier separating one fauna from another. If man be treated as a Pleistocene animal, there is reason to believe that he formed one of the North Asiatic group, which was certainly in possession of Northern and Central Europe in Pre-glacial times. The Pleistocene mammalia may again be divided into three groups, those which came from Northern and Central Asia, those from Africa, and those which were living in the same area in the Pliocene age. Had not the animals which lived in Europe during the Pliocene age been insulated from those which invaded Europe from Asia by some impassable barrier, the latter would occur in our Pliocene strata as well as the former. Such a barrier is offered by the northern extension of the Caspian up the valley of the Obi to the Arctic Sea. The animals of Northern and Central Asia could not pass westwards until the barrier was removed by the elevation of the sea-bottom between the Caspian and the Urals. The same argument holds good as to the African mammalia, which could not have passed into Sicily, Spain, or Britain, without a northward extension of the African mainland. The relation of the Pleistocene to the Pliocene fauna is a question of great difficulty. If the Pliocene fauna be compared with that of the Forest-bed, it will be seen that the difference between them is very great. The Pliocene mastodon and tapir, and most of the Cervidæ, are replaced by forms such as the roe and red-deer, unknown until then; but many of the Pliocene animals were able to hold their ground against the Pleistocene invaders, although they were ultimately beaten in the struggle for existence by the new comers. The fauna which the author adopted as typically Pliocene is that furnished by the lacustrine strata of Auvergne, the marine sands of Montpellier, and the older fluviatile strata of the Val d'Arno. Mr. Prestwich was hardly prepared to accept the proposed division of the Pleistocene mammalia into three groups; at all events so far as Britain is concerned. Neither could he draw that distinction between the beds at Erith and Grays and those higher up the Thames, which found favour with the author. The barrier offered by the river itself might to some extent account for the absence of reindeer; and though there was a difference in the fauna in the two cases, it seemed hardly enough to mark any great distinction in time. As to the hippopotamus, which occurred over the whole of Northern Europe, associated with the musk-ox and large boulders, he could not see how the conclusion was to be escaped of its having been able to withstand greater cold than its present representative. Though the winters might have been colder, there was evidence in favour of the summers having been warmer; and the flora seems to have been much like that of the present day. The probable migrations of the different animal groups had already been pointed out by M. Lartet, though Mr. Dawkins had carried his investigation of the subject further. He called attention to the fact of the mammoth having been found in Italy. Mr. Boyd Dawkins, in reply, stated that in forming his conclusions, he had not left out of view the evidence afforded by the classes of remains other than those of mammalia, but they threw no light on the classification. With regard to the middle of his divisions of the Pleistocene mammalia, he relied to a great extent on the presence of *Rhinoceros megarhinus*, and of a large number of stags, to say nothing of the absence of reindeer. He did not attach so much importance to the question of the level, as such discrepancies as those pointed out appeared to him by no means impossible. He gave his reasons for not regarding the mammoth as an exclusively arctic animal. His remarks with regard to M. Lartet's classification referred rather to the expanded views of his followers than to those of M. Lartet himself. He acknowledged his obligations to Profs. Gaudry, Fraas, Rütimeyer, and Nilsson for various facts of which he had made use.

Paris

Academy of Sciences, June 10.—M. Marie presented a memoir on the determination of the critical point at which the region of convergence of Taylor's series is situated; and M. A. Ribaucour a note on the theory of lines of curvature.—M. Yvon Villarceau exhibited and described to the meeting an isochronous regulator with vanes, constructed by M. Bréguet.—A note was read by M. E. Vial on a new mode of printing on stuffs by means of metallic precipitations, in which the author described a method of printing either by means of clichés or of copper or steel plates upon any textile fabric by the agency of nitrate of silver.—M. A. Clermont presented a note on the metallic trichloracetates, in which he described the preparation and characters of trichloracetate of ammonium, and of acid and neutral trichloracetate of thallium, and noticed the action of permanganate of potash upon hydrate of chloral in producing trichloracetic acid.—M. Wurtz communicated a note by M. Oré on M. O. Liebreich's experiments, from which the latter inferred that strychnine is an antidote to chloral. M. Oré shows grounds for the belief that M. Liebreich's experiments were inconclusive.—M. de Vibraye presented some further remarks on the spontaneous appearance in France of exotic plants in the track of the belligerent armies in the late war, in which he stated that the number of these plants introduced into the department of the Loir et Cher alone is 163.—In consequence of M. de Vibraye's statements, the sections of Botany and Rural Economy were instructed to prepare a scheme for the systematic introduction of Algerian forage plants suitable for the climate of France.—M. Decaisne presented a note by M. J. E. Planchon on the geographical distribution of the Ulmideæ.

BOOKS RECEIVED

English.—Contributions to Molecular Physics in the domain of Radiant Heat: J. Tyndall (Longmans).—Patterns for Turning: H. W. Elphinstone (J. Murray).—Symon's British Rainfall for 1871 (E. Stanford).—Erewhon, or Over the Range (Trübner).—The Principles of Geology, 11th edition, Vol. ii: Sir C. Lyell (Murray).

American.—Astronomical and Meteorological Observation made at the U. S. Naval Observatory, Washington, 1869.—The Science of Æsthetics in the Nature, kinds, laws, and uses of Beauty: H. N. Day.

Foreign.—Medizinische Jahrbücher, Heft i., 1872: S. Stricker.—Bulletin de la Société Impériale des Naturalistes de Moscou, iii. and iv., 1871.—Die Darwinsche Theorie: J. W. Spengel.

DIARY

THURSDAY, June 20.

Royal Society, at 8.30.—Volcanic Energy—an attempt to Develope its True Origin and Cosmical Relations: R. Mallet, F.R.S.—Preliminary Note on the Reproduction of Diffraction Gratings by means of Photography: Hon. J. W. Strutt.—On Voltaic Standard of Electromotive Force: Latimer Clark.—Pyrology, or Fire Chemistry: Capt. Ross, R.A.

Society of Antiquaries, at 8.30.—Hungarian Origin of the word Coach: A. Goldsmid.—On the Origin of the Christian Æra: G. Oppert.

Linnean Society, at 8.—On the structural peculiarities of the Bell Bird (*Chasmorhynchus*): by Dr. Murie, F.L.S.

Chemical Society, at 8.—On Deacon's Method of obtaining Chlorine, as illustrating some principles of Chemical Dynamics: H. Deacon.

MONDAY, June 24.

Royal Geographical Society, at 8.30.

WEDNESDAY, June 26.

Society of Arts, at 4.—Anniversary Meeting.

Royal Society of Literature, at 8.30.—On the Extent of Ancient Libraries: W. E. A. Axon.—On a Service Book of Strassburg use, containing Dramatic representations: Walter de Grey Birch.

THURSDAY, June 27.

Society of Antiquaries, at 8.30.

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