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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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25, Devonshire Place, Portland Place, London,
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THURSDAY, FEBRUARY 3, 1870

THE ATMOSPHERIC-GERM THEORY

WE have heard much during the last week or two concerning the presence of organic matter in the atmosphere, and the degree to which this is filled with "germs" of living things. It would have been better, perhaps, had it been always pointed out more distinctly that the two expressions were by no means uniformly convertible. There is unquestionably much mere organic *débris* in the atmosphere which nobody could regard as germs of living things.

The transition, in the minds of many at the present day, however, from the idea of organic matter in the atmosphere, to the identification of this with germs of actual animal and vegetable organisms, occurs only too easily. The air is supposed by them to be teeming with potential living things most varied in kind. Each square foot of atmosphere is thought to contain representatives of innumerable varieties, which are only awaiting the advent of suitable conditions in order to commence their growth and development. Men talk most glibly about germ-theories of disease, and the share which germs take in the origin of epidemics, as though these were proven facts of science rather than, as they are at present, mere questionable hypotheses. And, just as these germ-theories concerning epidemic disease have grown out of the more general *panspermic* doctrine, so did this doctrine itself grow out of the great "spontaneous generation" controversy somewhat more than a century ago. As it was with the derived, so was it with the original doctrine: in each case it was produced, not so much on account of any *direct* evidence in favour of the existence of such germs, but rather on account of the inherent difficulty in the explanation of the subject to which it referred. Previous to the period mentioned, however, no such doctrine had been started. There were, of course, the old pantheistic doctrines of Anaxagoras and his followers—the notion of the universal diffusion of an active principle or νοῦς pervading all things, which was itself the cause and source of all the life on our globe: there was also the doctrine of Leibnitz, concerning "Monads," as centres of force and life existing in all things; but anything like the present "panspermic" doctrine was still wanting.

The Aristotelian notions concerning the "spontaneous generation" of even complex living things, received a severe blow by the experimental demonstration of Rédi, in 1638, before one of the Italian academies. He showed that the larvæ found in putrefying flesh had been deposited there by flies, and had not been engendered (as had been previously supposed) by changes taking place in the flesh itself. Hence a very desirable modification of their views was necessitated on the part of the heterogenists. It was not, however, till about a century after this that the "spontaneous generation" doctrines were again prominently brought before the scientific world. Then, too, they appeared in a form more suited to our present notions. The long controversy carried on between Needham, the English champion of heterogeny, and the Abbé Spallanzani, resulted in the promulgation by the latter of the celebrated "panspermic" doctrine. The question pressing for solution was, What is the mode of origin of the myriads of the lowest forms of life which so soon teem in organic solutions? According to Needham many of these lowest living things had been evolved *de novo* owing to changes taking place in the organic matter of the infusion; according to Spallanzani they had been ultimately derived from "germs" which, floating everywhere in the atmosphere, had, in spite of all precautions, gained access to the solutions. Spallanzani did not pretend that he had seen these "germs," their existence was a mere postulation and no other evidence of their reality was alleged than the occurrence of the very phenomena which their presence was supposed to explain. His position was interpretable in this way. He seemed to think that such new evolution of life was impossible. If living things occurred, therefore, they *must* have originated from pre-existing germs. Against unchangeable convictions of this kind, occurring either then or now, of course no amount of experimental evidence would be of any avail. Spallanzani preferred to believe that the atmosphere carried with it everywhere myriads of germs of elementary organisms, or, at all events, sundry *principes préorganisés*, invisible and imaginary though they might be. On this subject he says* :—"The infusorial animalcules undoubtedly take origin, in the first place, from certain *principes préorganisés;* but these *principes*, are they eggs, germs, or other similar corpuscles?" To which he most honestly adds :—"If it is necessary to offer facts in reply to this question, I frankly acknowledge that we have no certain knowledge on the subject."

Bonnet was the contemporary of Spallanzani, and he was also the advocate of a doctrine similar in its tendency, though infinitely more extravagant. Bonnet's leading notion of the "embôitement des germes" is thus illustrated in one of the earlier chapters of his work † :—"The sun, a million times larger than the earth, has for an ultimate constituent a globule of light, of which several thousand millions enter at once into the eye of an animal twenty million times smaller than a flesh worm .... But reason can penetrate even further. From this globule of light it can see issue another universe having its sun, its planets, its plants, its animals, and amongst these last an animalcule which is to this new world what that, of which I have just spoken, is to the world which we inhabit." Now, it would certainly be wrong to restrain any man in the exercise of his fancy, but it surely is deplorable when we find the results of such exercise—such mere figments of the imagination as this—warping the reasonings of succeeding generations when they come gravely to argue about questions of fact.

Such, then, has been the origin of the "panspermic" doctrine. Its first supporters commenced with assumptions, which could only be supported by the occurrence of the very phenomena that were the subjects in dispute, and to explain which the assumptions had been started. This was the doctrine of which M. Pasteur first attempted the experimental verification. How far he succeeded in the attempt is another question. On the part of those who first promulgated the "panspermic" doctrine, there certainly was nothing but mere fancy and hypothesis.

* Opuscles de Physiques, animale et végétale. Pavie, 1787. Tom. I. p. 230.

† Considerations sur les Corps Organisées. Amsterdam: 1772.

ENGLISH SPORT IN THE FIFTEENTH CENTURY

The Debate between the Heralds of France and England. Translated and edited by Henry Pyne. (London: Longmans and Co. 1870.)

IT is not easy to obtain an accurate knowledge of the fauna of England before the sixteenth century, or to ascertain with anything like precision the distribution of wild animals throughout our country. Contemporary authorities are few, and allusions in them to the facts of Natural History are vague and scanty: vague enough to whet our curiosity, and rare enough to augment the interest attaching to them. We are, therefore, grateful when we can derive from any fresh and well-accredited source a side-light upon this obscure subject, and such we seem to have found in a few incidental remarks that occur in a very early tract, bearing the unsuggestive title of "A Debate between the Heralds of France and England." This debate, now for the first time translated into English, appears from internal evidence to have been written by Charles, Duke of Orleans, about the year 1460, and to have been first published in Paris in 1500. Its author, taken prisoner at the battle of Agincourt, was detained in England for some five-and-twenty years, dividing the period of his captivity between London and the Castles of Windsor, Pontefract, Ampthill, Bolingbroke, and Wingfield. To a man of quick observation, as the Duke undoubtedly was, this lengthened exile gave ample opportunity of forming a tolerably correct opinion of the relative merits of the land of his birth and the land of his captivity. Patriotism has, of course, occasionally coloured his views, but on the whole his judgments are wonderfully impartial, and his statements may be accepted with very little qualification. It must, however, be borne in mind that the Duke's acquaintance with England was almost wholly confined to the eastern side, which has very little in common with the rest of the country, and has probably undergone far fewer changes in later times. Thus, in his estimate of the capabilities of our country for sporting purposes, he makes the English Herald say: "England is a level country, well cultivated, and not covered with trees or bushes, which might hinder the game from being easily found and caught; and it has also many partridges, quails, and other birds, as well as hares in great abundance. And with regard to the sport of fowling, no one can imagine a more delightful country, for there are numerous little streams which flow into the great rivers, where it is a fine thing, during the season, to see what a profusion there is of wild fowl." This description is true enough of the eastern counties, especially if we understand the term "wild fowl" to include snipes, plovers, bitterns, and other fen-haunting birds. But, in lauding the superior merits of French sport, the Duke gives some further details, which are not without their value, as illustrating what we may call the antiquarian side of Natural History. "In France," he remarks, "we have not only all the wild animals which you (English) have, as stags, roes, and deer, but we have many other animals for the chase besides these: for we have wild boars or wild black swine, and we have also wolves and foxes, while you have none." Now, it is hardly necessary to observe that the popular story of the extermination of wolves in England by Edward I. must be received with some reservation. There seems some ground to believe that in the valley of the Findhorn, in Scotland, wolves have bred as late as the seventeenth century, and that even in the wilder parts of England—the fells of Yorkshire, and the forest of Dartmoor—they have existed in the fifteenth, and perhaps in the sixteenth century, if we are to give any credence to local traditions. Certain it is that in 1280, John Giffard, the Baron of Brimsfield, had license from King Edward to hunt wolves with dogs and nets in all forests in England; we have also little doubt that a diligent search through the public records would disclose similar grants of later date. Some ten years ago a young wolf was caught in a vermin-trap at Ongar, in Essex, but its occurrence was explained by the fact that the master of a neighbouring hunt had recently imported some fox-cubs from France, and that the wolf had been included in the hamper by mistake. The comparatively small amount of woodland and covert in the East of England would render the breeding of wolves, to any extent, an impossibility, and in a less degree the same remark applies to foxes also. Fox-hunting, in the modern sense of that term, is a sport of recent growth, and such a thing as the preservation of foxes for hunting purposes cannot boast of any antiquity at all. Gervase Markham, indeed, classes the hunting of the fox and the badger together, and describes them as "chases of a great deal lesse use or cunning than stag and hare-hunting, because they are of a much hotter scent, and are not so much desired as the rest,"—an observation which may be balanced by the French Herald's remark, that wolves and foxes "are bloodthirsty animals, so that it requires persons of great courage to overcome them." Wild swine in England were either destroyed or domesticated at a very early period. Pannage was too valuable a privilege to be otherwise than jealously guarded against such unwelcome intruders. Charles I. turned out in the New Forest some boars and sows which he had imported from Germany, and fifty years ago their descendants might be recognised by the smallness of their hind-quarters and greater development of sinew.

The next position asserted by the French Herald in the Debate, if true at the time, has since been curiously reversed. He claims precedence for his country, not only in respect of hares and game-birds generally, but especially or exclusively for the great red-legged or Grecian partridges, and an abundance of pheasants. Hares have always been common in England, but the prevalence of red-legged partridges throughout the stubbles has not been a long-standing grievance to the Suffolk sportsman, for, if Pennant be right, they were introduced from France, as late as the year 1770, and, perhaps from their dislike to a humid soil and atmosphere, have never spread themselves far inland.

With regard to English pheasants, the Duke's experience must, we think, have been exceptionally unfortunate. The bills of fare, in olden times, invariably make mention of them, and Mr. Pyne refers to a statute passed in 1494, prohibiting their destruction by unlicensed persons, and clearly implying that they were common enough. Goshawks and tercelets for hawking purposes were, no doubt, imported from France in the fifteenth century—the accuracy of the French Herald's statement on this point being confirmed by several passages in the *Paston Letters*.

We are rather surprised that no mention of the bustard should be made in the tract. Though now extinct in England, the bird was by no means uncommon in the open country at the commencement of the present century. We can only suggest that the cause of its omission from the Herald's list and its gradual extinction in England, is one and the same. Being slow to take wing, the bird is of little or no use for sporting purposes, and nature has implanted in it a rooted aversion to those enclosures which, in a land like ours, alone are sacred to game.

CHARLES J. ROBINSON

POPULAR LECTURES ON PHYSIOLOGY

Populäre Physiologische Vorträge gehalten in Akademischen Rosensaale zu Jena, 1867-1868-1869. Von Prof. Dr. Joh. Czermak. (Wien, 1869. London: D. Nutt.)

THE daily increasing recognition of the importance of Physiology as an element of liberal culture, no less than as a distinct branch of science, may be said to be intimately connected with the gradual displacement of the old vitalist conception. The old conception of Life as something essentially mysterious and removed from out the circle of natural causes, has been set aside in favour of the conception of Life as something more complex, indeed, but not otherwise more mysterious than other natural phenomena, and dependent upon the physical and chemical agencies recognised in operation in other provinces of research. The consequence of this changed view has been to disclose the need of an incessant application to Physiology of those instruments and methods which have enlarged and given precision to our views of Nature; and a further consequence has been that the problems are found to be more capable of popular exposition, that is to say, the great results of research can now be *shown* to an intelligent public, and made thus to form an element in general culture.

It is under this second aspect that Prof. Czermak's Lectures call for remark. Himself an original investigator and inventor, he here gives excellent examples of the kind of teaching that may become generally effective—namely, an intelligible exposition, for the laity, of the painfully-acquired results of science. This exposition is not confined to an oral statement of results, which statement might be imperfectly apprehended and quickly forgotten, but is accompanied by a visible demonstration which fixes it in the memory as with a burin. In his first lecture he expounds the action of the heart, and the influence of the nervous system on the circulation. In this he exhibits the instrument formerly invented by him, the Cardioscope, which enables an immense audience to *see* the rhythmic beating of the heart when taken from the body; and by the directions and the plates here supplied teachers and private students may easily furnish themselves with the ingenious contrivance. Indeed, we may say at once that this volume will be especially useful to teachers who will gain from it several effective aids for their lectures.

The second lecture is on the Ear and Hearing, and gives a lucid account of all but the very latest discoveries (the startling and *disturbing* discovery which robs the Corti apparatus of its former significance being subsequent to the publication of this volume). The third and fourth lectures are on the Voice and Speech, accompanied like the others with excellent and instructive diagrams, and a very intelligible explanation of the instrument with which Prof. Czermak's name has been carried all over Europe and America—the Laryngoscope.

A mere glance at this volume, and the elaborate application of mechanical aids which it suggests, will indicate at once the great change which has come over physiological investigation in the last twenty years. Not to speak of such works as those of Richerand and Majendie in France, or of Elliotson and Mayo in England, where there is scarcely a trace of the physical and chemical investigation now considered indispensable, let even the great work of Müller be opened at the places where the heart, the ear, and the voice are treated, and compared with these lectures, addressed, be it observed, to a miscellaneous audience of ladies and gentlemen, not to students in a class-room, and the contrast will, as the French say, leap at the eyes. The changed direction is one that attempts to reduce physiological problems to visible and measurable processes, which leave no room for vagueness or inexactness, and which, without getting rid of the mystery of Life, reduces the phenomena of Life to what Prof. Huxley finely names the "realm of *orderly* mystery."

GEORGE HENRY LEWES

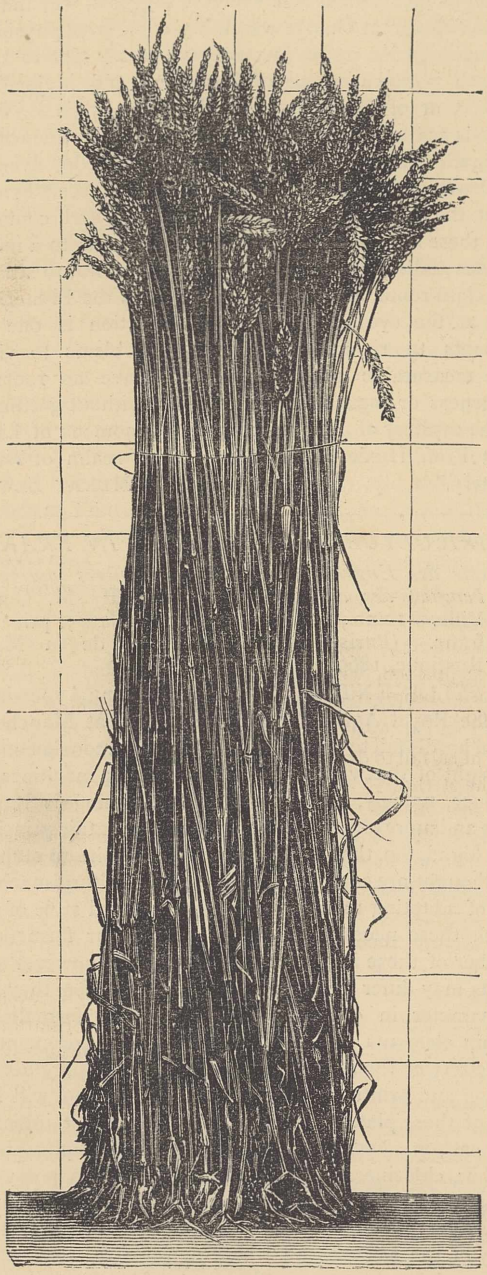
AGRICULTURAL CHEMISTRY IN FRANCE

L'Ecole des Engrais Chimiques; premières notions de l'emploi des agents de fertilité. By M. Georges Ville. 12mo., pp. 108, with one plate; price one franc. (Paris: Libraire Agricole de la Maison Rustique, 1869.)

INTELLIGENT visitors to the last annual meeting of the Royal Agricultural Society, held at Manchester, could hardly fail to draw one very important conclusion from the sight of that vast array of implements, machines, and produce, namely, that year by year farming is brought more and more under the application of scientific principles. It is found, too, that farm labourers, not quite so stupid as was thought, are capable of managing agricultural machines, and of adapting themselves to the improved style of work which these necessitate. And among our farmers the number of those increases who know that, however much plants may differ in appearance and properties, they have a character in common, and owe their formation to certain elements which suffice for their requirements, as the letters of the alphabet suffice for the requirements of writing and printing. And their sons at school will learn that of these plant-forming elements, the more important are, carbon, hydrogen, oxygen, nitrogen, phosphorus, sulphur, chlorine, silicon, calcium, magnesium, potassium, sodium, and iron. Schools and colleges have taken the subject in hand, and have demonstrated that the study of the sciences on which agriculture may be said to depend, involves no small amount of intellectual culture—a culture fraught with lasting interest.

On the Continent, also, a movement has begun for the improvement of agriculture, and it will be instructive to look at what has been accomplished in France. Among the subjects on which lectures are given by the able professors at the Museum of the Jardin des Plantes is *la chimie agricole*—agricultural chemistry—and, in the hands of Prof. G. Ville, who has been experimenting

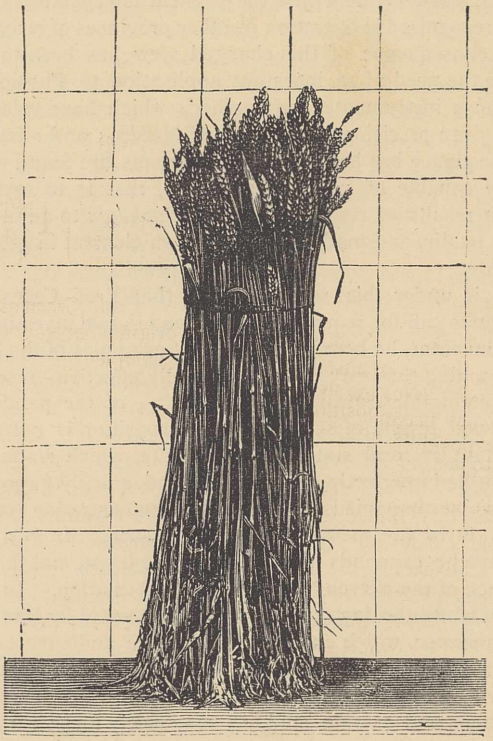
thereon for the past twenty years, it has produced surprising results. Having established his propositions single-handed, and at his own cost, a portion of the imperial domain at Vincennes was allotted to him, as an experimental farm, and the crops he there produced, the *Conférences Agricoles* he there held among the crops, together with his numerous



Comparative size of wheatsheaf produced from soil completely manured.

published works, and the effect of his lectures, have made such an impression, that his method of cultivation has been adopted at more than five hundred places in France; while from Spain, Portugal, Italy, Belgium, and Germany, and from the French colonies, farmers and cultivators have resorted to Paris and to the experimental farm at Vincennes, to acquaint themselves with the method which under ordinary circumstances would more than double their harvest.

Prof. Ville's method of instruction is as simple and definite as his method of fertilisation, as if he had in view the large number of small peasant proprietors in France, and wrote and lectured for their especial benefit. You may sow wheat, he says, in soil wanting nitrogenised matter, phosphates, potash, and lime, and it will grow and bear grain, but the stalk will be very short, thin, and weak, and the vegetation precarious. Mix with the soil a substance composed of carbon, hydrogen, and oxygen, the result will be exactly the same. And why? Because plants take all the carbon they need from the carbonic acid of the atmosphere, and hydrogen and oxygen from water. Hence, to add these three elements to the soil is useless; on the other hand, if some substance containing assimilable nitrogen be added, a salutary effect is at once produced, for plants derive nitrogen partly from the soil, partly from the air. But this effect is as nothing compared with that of manures affording certain mineral elements: mix these as well as the nitrogenised matter with calcined earth, and its fertility will equal that of the richest soil. The vegetation, no longer thin and starved, acquires vigour and activity, the plant grows straight and strong, is of a rich green colour, and produces a well-formed ear filled with large and heavy grain.



Comparative size of wheatsheaf produced from soil without manure.

Many of the mineral elements required by the plant already exist in the soil; consequently, to render it productive the farmer has to discover which of the fertilising elements are wanting, and apply them to his field. To do this, it is not necessary that he should be an analytical chemist: the vegetation of his fields will do it for him. He has only to sow or plant a small experi-

mental plot, and the appearance of the crop will tell him what is deficient in the soil. And in making his experiments, he is not restricted to any special kinds of plants, for the method may be applied to the sugar cane, the sorgho, and other natives of the tropics, to maize, beetroot, the Jerusalem artichoke, the potato, colza, and other varieties of cabbage, and to flax and hemp. Whether saccharine, or oleaginous, or fibrous, or starchy, the plants may be developed to the utmost limit of their productiveness by proper treatment.

The complete fertiliser, or *engrais complet*, as Prof. Ville calls it, varies slightly in composition according to the nature of the crop required. For wheat it is composed of acid calcium phosphate (superphosphate of lime), potassium nitrate (nitre), ammonium sulphate, and calcium sulphate (gypsum). These materials require to be skilfully mixed, so as to retain all their beneficial properties when spread upon the land. The "engrais complet" is to be regarded as the typical fertiliser—the standard by which to judge of the value and the lastingness, so to speak, of any chemical manure. A field properly treated with this will return three or four crops of wheat in immediate succession, without further manuring in the meantime.

For the proper carrying on of the experiments, it would be necessary to have—

1. The complete compost.
2. Compost without potassium salts.
3. „ without phosphates.
4. „ without nitrogenised matter.

These are to be applied to the experimental plots of land. If then, number 2, or 3, or 4 produces the same effect as number 1, it is evident that the substances represented by those numbers exist already in the soil, that the vegetation has profited by them, and that there is no need to apply them to the land. If, on the contrary, the compost in which any one of the four substances is wanting produces a deficient crop, there is a certain proof that this particular element of productiveness is deficient in the land.

Here, then, is a method of analysing the soil of fields and gardens which even an intelligent labourer might practise. He could hardly continue the practice without learning, better than from books, the true principles of agricultural science. He could determine step by step the loss of any one or more of the fertilising elements, and consequently ascertain which was required to restore the fecundity of the soil.

In support of this argument, Prof. Ville brings forward some remarkable results obtained on his experimental farm at Vincennes, giving tables showing the cost of cultivation and the yield of each experimental plot. He also places before his readers a series of figures in which the results obtained with wheat are displayed in a most forcible manner. The two examples here given indicate the limits of his experimental system. The sheaves are represented on the same scale, so that the results of good feeding and starvation may be readily compared. The intervening figures may be imagined.

If the experiment be made with peas, there is a marked difference in the result: the nitrogenised matter which tells in the culture of wheat has little or no effect on leguminous plants; the reason being that wheat takes up from the soil most of the nitrogen it requires, and that leguminous plants derive it mainly from the air. To produce a good crop of the latter we must employ a manure containing an excess of potassium salts, and for turnips we require an excess of calcium phosphate.

These are important facts, for they show that besides variation and rotation of crops, there can be also rotation of manures. Take any average field, and apply to it the materials which act on the required crop, and so pass from one to the other until the land shall have received all the materials which make up the complete compost.

This, briefly sketched, and with omission of certain practical details, is Prof. Ville's system for the improvement of agriculture in France. The five million small proprietors, who own each from 3 to 14 hectares, have herein a simple and efficacious method, by which their profits may be largely increased, and their own condition materially ameliorated. To quote the Professor's words, "The tiller of the soil is always in presence of a Power superior to himself. Seasons, temperature, rain, and sunshine, which enter so largely into the success of his labours, are above his influence. He learns that skill, foresight and economy are required of him; he knows also that when he has done all that depends on himself he must be resigned and wait. By temperament, as much as by condition, he becomes above all the friend of order, and, in case of need, its firmest support."

OUR BOOK SHELF

Landwirthschaftliche Zoologie. Von Dr. C. G. Giebel. 8vo. pp. 827, with 510 woodcuts. Glogau, 1869. Carl Fleming. (London: Williams and Norgate.)

ALTHOUGH the study of economic zoology ought to be of some importance in this country, we can point to very few English publications relating to it,—Curtis's "Farm Insects," and the translation of Kollar's "Garden Insects," by Westwood and Loudon, being almost the only special works on the subject that we can call to mind, and these are now of old date. This may perhaps be due to the fact that the advantage of attempts at the practical application of zoological and, especially, of entomological knowledge, is very frequently doubtful, but it is certain that in many cases an acquaintance with the natural history of animals must be most valuable to the farmer or gardener, by enabling him to distinguish beneficial from injurious creatures; it is therefore much to be regretted that we possess no good treatise which would place the necessary knowledge within reach of our English agriculturists.

There is another point of view in which the study of economic zoology is of great importance. From the very nature of their relations to man and the organisms which he has taken under his care, useful and injurious animals acquire a remarkable prominence, and thus the phenomena of their existence are brought near to us and, as it were, magnified in such a manner as to render their investigation comparatively easy. Accordingly, there can hardly be any better means of acquiring a practical general knowledge of natural history and, especially, of the intimate and intricate relations of organised beings, than the study of the enemies and benefactors of the farmer and gardener.

Dr. Giebel seems to have taken this view of the matter, and his "Agricultural Zoology" is really a complete natural history of terrestrial animals, the illustrative examples being drawn from domestic animals, or from species which exert a more or less direct influence upon the results of agricultural operations. Of all these, the natural history is given by Dr. Giebel in considerable

detail, and in this way we get most of the families of mammalia, birds, insects, and arachnida legitimately illustrated, whilst by stretching a point here and there, even the classes of reptiles, fishes, and mollusca are more or less represented. Under the Vermes we find a good account of the curious natural history of the Entozoa. Either as a practical guide for the cultivator, or as a first book in the study of general zoology, Dr. Giebel's volume will be found of great value, and we can only repeat our expression of regret that we have nothing like it in our own language.

History of Meteors and of the Grand Phenomena of Nature. By J. Rambosson. (London and Edinburgh : Williams and Norgate.)

NOTHING is more sublime, or more engrossing, or at times more awful than the phenomena of Nature on the large scale ; and yet nothing is more prosaic than most of the treatises on meteorology.

Those terrible statisticians with their columns of figures have certainly succeeded marvellously in creating a distaste for the study of the greater natural phenomena. A cyclone at sea is one of the most glorious and awful sights that can be imagined, yet in the pages of meteorological journals it becomes a barometric difference of an inch and a half, a wind equal to 12, and a sea disturbance equal to 9.

But M. Rambosson, with that talent for elucidation which so many Frenchmen and so few Englishmen possess, has spared neither trouble nor expense in order to produce a thoroughly popular and, at the same time, thoroughly scientific description of the grander class of natural phenomena.

THE AURORA BOREALIS

Being himself a traveller as well as a man of science, his descriptions and illustrations are particularly good ; and no student can read the book without retaining a vivid impression of the aspects of Nature in her wilder moments. Some of the illustrations remind one of Gustave Doré ; that of a hurricane on land and sea, Fig. 3, is particularly good ; so is that of a hurricane in the desert, Fig. 33 ; also his pictures of lightning, Figs. 62, 63, 64 ; on the other hand, some few illustrations might possibly be dispensed with.

It has often occurred to us that while the student of chemistry and of physics is made acquainted in the laboratory with the forces with which he reasons, so the student of meteorology should, if possible, like the author of this book, be brought face to face with the grander class of natural phenomena ; failing which, a well illustrated book, like that of M. Rambosson, is an admirable substitute for the personal experience which few can have. B. S.

Our Own Birds. A Familiar Natural History of the Birds of the United States. By W. J. Baily. Revised and edited by G. D. Cope, Cor. Sec. Acad. Nat. Sci. With illustrations, after Audubon. (London: Trübner).

ALTHOUGH the object of this work is not to treat ornithology scientifically, but rather to present the subject in a pleasing manner to the young, Mr. Cope's name on the title-page is a guarantee of its scientific accuracy ; and although the birds of the United States are mainly dealt with, the volume will doubtless be welcome to many young naturalists in this country, for the habits and interesting peculiarities of each bird are fully dwelt upon. Some of the illustrations are admirable.

MR. BATES'S ADDRESS TO THE ENTOMOLOGICAL SOCIETY

IN printing the following extracts from the address delivered to the Entomological Society on the 24th ult. by the President, Mr. H. W. Bates, we must express our regret that we cannot find space for the insertion of the whole of that able and interesting discourse.

Referring to the "Transactions" of the Society, Mr. Bates remarked:—The volume for the past year comprises twenty-seven memoirs, of which twenty-five belong to the department of systematic or descriptive Entomology, and two only—welcome contributions from Mr. Jenner Weir and Mr. Butler, on the selection of insects as food by insectivorous animals—to other branches of the science. To those who might object that too large a share of our work is occupied by mere descriptions, I would remark that many original and valuable observations on relationships, geographical distribution, and other deeply interesting philosophical questions, are contained in some of our descriptive papers. In fact, it is not at all a necessary consequence that a descriptive treatise should be nothing more than a string of dry definitions. It will become, I hope, more and more the practice of entomologists to give, together with their descriptions, the new data on relationships, distribution, comparison of faunas, &c., which the handling of such subjects most usually brings forth.

In speaking of the contributions of importance which have been made to the science in this country outside the Entomological Society, the President alluded to the examination by Mr. Wollaston of the largest collection of insects which has yet been made in St. Helena. This suggested the following observations: As you are aware, the great interest which attaches to the fauna and flora of oceanic islands arises from the problems involved in the modes in which they obtained their species of animals and plants, and those are rendered more complicated by the existence on some island of anomalous forms, representative, it is considered, of types ages ago extinct on continents. Such islands, however, differ greatly from each other as to degree of peculiarity in their productions; and it often happens that species identical, or nearly so, with those found in the nearest continent, form nearly the whole of their present inhabitants. Thus the investigation of the origin of their faunas and floras is necessarily exceedingly complex. Geology has to be invoked to ascertain whether the islands are of recent or ancient elevation above the sea-surface, and whether the supposition is admissible of a recent connection with the nearest continental land. Oceanic hydrography, deep-sea soundings, and the force and direction of currents and winds, have to be studied in reference to the depth of the surrounding seas; for these must all be taken into consideration in discussions on the probable derivation of the curious mixture of forms which is often found on these isolated spots. On the other hand, it must be noted that the fauna and flora themselves throw light on the geographical and geological relations of the islands to the nearest land. In fact, the classification of islands into oceanic and continental, is founded quite as much on resemblance or difference in organic productions, between islands and the mainland, as on relative proximity. Thus Great Britain is classed as a continental island, quite as much because its fauna and flora are nearly identical with those of continental Europe as because it is separated only by a shallow sea, and is now known to have been actually connected in recent geological times. In these investigations entomology is now generally admitted to have great importance, owing to the large number and variety of species which it offers, as elements in the elaborate comparisons which have to be instituted.

The following remarks well illustrate the high scientific importance of studying the geographical distribution of insect life:—The idea of the value of localities in connection with specimens or species, with some entomologists, I am afraid does not reach very far. They like to know in what countries the different forms are found, and perhaps, as in French collections, show the distribution by writing the specific names in their cabinets on labels coloured according to the part of the world the species inhabit; the primary divisions of the world, as Europe, North and South America, Africa, Australia, perhaps the West Indies, and so forth, being considered sufficient. This brings out the leading facts of distribution very well, such as the restriction of many genera and groups of genera to each of the great divisions, and the distinctive facies which all the products from one region possess; but we seldom see it carried further, and it remains a pretty association of geography with natural history, and no more. Results infinitely more suggestive are brought about if the student labels each *specimen* with its locality, instead of recording it on the ticket which bears the specific name placed below all the specimens, and if he is fortunate enough to be able to amass a large suite of specimens, accurately so ticketed, of genera abounding in local varieties and closely-allied species, indications of the conditions under which varieties, local races, and perhaps species, are formed in nature, are revealed by this method, and a field of investigation is opened which connects the study of a few insect species with some of the most difficult problems that are now engaging the attention of philosophers. The most common event that happens, when a student works at a series of species in this way, is the discovery that even the most constant species vary in some parts of their area of distribution; the next, that a small well-marked difference in a species is generally a local difference, and embraces all the individuals of the district in which it occurs. As the collection increases, further curious facts come out. It is found, for instance, that some highly-variable species give rise to one set of varieties in one area, another distinctly different set in another area, and so on; and further, that in some areas one, or perhaps more, of these variations will be better marked than, and preponderate in number over, the other varieties of the same species. Still further, it is found that in some districts one such variety alone occurs, having apparently prevailed over all the others. To be properly impressed, however, with the great truth and reality of these facts, the student should himself have travelled as an entomological collector over an extent of country embraced by many local varieties of variable species; otherwise his attention will not be sufficiently excited to the curious facts nature presents to him, and he will not take the trouble to amass and obtain the exact localities of numerous specimens of common variable species. Perhaps the most important result of this attention to distribution of varieties is, that a fine gradation of forms or degrees of variation will be found, from the "sport" or variety, such as is liable to be produced in the same brood, to the well-segregated race living in company with another race referable to the same stock. As such, most authors, perhaps rightly, consider these latter as good and true species; and thus the formation of species out of mere variations is illustrated by the facts of geographical distribution.

But it is not this branch of the subject with which we are so much concerned, when we wish to compare the productions of the different Andean valleys and their vertical ranges, as that relating to the nature of barriers to distribution. It has been received as a principle in zoological and botanical geography, that grand physical barriers, such as mountain ranges, form an impassable limit to the faunas and floras of the plains on each side of them. It is repeated, in almost every manual of physical geography, after Humboldt, who, I believe, was the originator of the statement, that the species are all different on the two sides of the Andes of South America. Such a fact, if well established, would be interesting in many ways. First, it would throw light on the geology of the country, as proving that the Andes must have existed as a ridge, sufficiently lofty to prevent the creatures of the plains crossing it, before the origin of the species which now people the plains on each side. Now, it is possible that this broad and important generalisation may have been made on a too slender foundation of facts. Of course, in those parts of the Pacific coast-region (two-thirds of the whole line within the tropics), where the conditions of soil, climate, and vegetation are totally different on the two sides of the Andes, no community of species is possible. A lofty mountain barrier would be here unnecessary, for a few steps of level road, in many parts of the world, would suffice to bring the traveller from the domain of one fauna to that of another—for instance, from an arid plain to a luxuriant forest along some river-valley. This would be a difference of "station," and not of area of distribution,—a distinction long ago recognised in Botany. The question is, then, limited to this: In those parts of the Pacific coast-region, such as Guayaquil, where a humid forest-country exists on both sides of the Cordillera, are the species of the two sides entirely distinct? This would test the efficacy of mountain-barriers better than almost any other case. For the species, at least of insects, which inhabit humid forests near the equator, are probably unable to exist at a higher altitude than 4,000 or 5,000 feet, and no pass over the Cordillera exists of half this depression, throughout the whole line of the Andes from Bolivia to the Isthmus of Darien. The species could not voluntarily pass over, nor by

gradual migration along the coast could they well double the end of the chain near the mouths of the Atrato and Magdalena, and so pass to the eastern side; for the Sierra Nevada bars the way.

Insects, I believe, would offer better data in discussing this question of barriers than almost any other group of land animals, or than plants; they are more limited in range than the species of birds, afford a much larger body of facts than reptiles, mammals and shells, and are not so much subject to accidental means of transportation as plants. But although many Entomological collectors have visited Guayaquil and the Cordillera, we have no published lists and no authentic information about localities. Mr. Buckley's journey offers us, then, the chance of obtaining the details so much required, since he collected assiduously all the way up from the level of the sea to the edge of the snow, and the same conversely on the opposite side, writing the locality on the envelope of every specimen.

I am inclined to think that the efficacy of physical or geographical barriers in limiting the distribution of animals and plants has been much over-estimated, and that this circumstance has vitiated much of the reasoning that has been employed in discussing various difficult problems in Natural History. By physical barriers, of course, are meant barriers of the inorganic world, such as a continuous mountain-range with regard to species of the plains and, conversely, a continuous plain with regard to species of the mountains (*e. g.* Parnassius, Erebia, Oreina, Nebria, &c.) The sea is thus a barrier to land-species, a water-shed to fresh-water species, a continuous tract of forest to species of the savannah or steppe, and so on. Barriers of the organic world, which of course are "physical" also, are quite a different set of agencies. They are the hindrances offered to the dissemination of a species by other species already in full possession of the domain and well adjusted to its conditions by constitution and habits. To this may be added the limitations to distribution observable without any physical obstacle being perceptible. There are certain classes of facts which seem to me to indicate that these less obvious kinds of barrier are far more effective than those more imposing ones of mountain, desert, sea, and so forth.

One set of these facts is exemplified by the well-known case of distribution of insects between the east and west in the southern part of our own island. I am not aware that comparative lists have yet been published; but it will not be disputed that many hundreds of species of Coleoptera, for instance, are known in the east, many of them abundant, which are totally unknown in the west, and a smaller number are known in the west which are not found in the east. In In cases like these a difference of climate may be the cause of the limitation. But there is another set of facts requiring quite a different explanation: this is the limited ranges of closely-allied species in the plains of Tropical America. I have already elsewhere recorded the fact that, in the forest plains of the Amazons, where there are no differences of level worth mentioning, and no physical barriers, the species of a large number of genera are changed from one locality to another, not more than 200 or 300 miles apart. This is most distinctly marked on the Upper Amazons, where the country may be mapped out into areas of a few hundred square miles each, every one containing numerous species of such genera as Ithomia, Melinæa, Eubagis, Doryphora, Erotylus, &c., &c., allied to but quite distinct from their representatives in the others. From what I have seen of Mr. Buckley's collections on the eastern side of the Andes, I think the same limitation of areas must occur there also; and judging from the few species I know as coming undoubtedly from the Guayaquil side of the Cordillera, the butterfly faunas of these areas in the uniform country of the east are pretty nearly as distinct from each other as the species east of the Andes are distinct from those west of the mountains. We here again feel the want of facts, such as Mr. Buckley collected, but which have not yet been published, to teach us exactly what species are found east and what west of the mountains, and how the great multitude of closely-allied species are distributed in the narrow tract explored on the east. My own observations in the level plain, a few hundred miles further east, show distinctly, however, that the most effective possible barriers are there opposed to the spread of hosts of species without any physical barrier which is perceptible by our senses. The explanation of the fact, I believe, is this, that there really are subtle differences of physical conditions from place to place, even in a uniform region; slight differences in soil, humidity, succulence of foliage, and so forth, which require in each a re-adjustment of the constitution of any new immigrants from adjoining areas; but that each area being kept well stocked with allied species already adjusted to its minute conditions, such migration rarely occurs. Thus a limit is put to the spread of species by species themselves, which produces similar results on the actual distribution of forms throughout the world, to those produced by mighty physical barriers such as the Andes.

There is yet one other consideration remaining. If these barriers are not required to explain the limitation of faunas, it does not follow that they do not act as barriers all the same; but it is, I think, difficult to prove it. If 1,200 miles of sea do not form a sufficient barrier against the stocking of the Azores with insects from Western Europe, I do not think sixty miles of mountain should be assumed to prevent for tens of thousands of years, the transport of species, in the egg state, by birds or currents of air, from one side to the other. I may add, in conclusion, that if the efficacy of barriers of this nature has been overrated, some important conclusions regarding changes on the earth's surface will have to be reconsidered; such, for instance, as that of the extension of a glacial epoch over nearly the whole earth—a hypothesis conceived by Darwin to explain the existence of the same genera and sometimes the some species in high latitudes, both in the northern and southern hemisphere, whilst absent from the intervening zones. I believe that, with some very obvious exceptions, such as Mammals and Batrachians, there can be no limit placed as to the dissemination of a species, provided there are unoccupied areas suitable to it, in any part of the earth, and provided also time sufficient be allowed for the process.

THE GRESHAM LECTURES

TWO lectures were delivered in Gresham College on the evenings of the 14th and 15th January, by Dr. Symes Thompson, the Gresham Professor of Medicine.

The first of these lectures embraced a theme admirably adapted to fulfil the popular object with which this City professorship has been established, and as eminently suited to the present season: it treated of "Catching Cold."

The Professor first described, by reference to large drawings, the structure and arrangement of the parts concerned in the disorder—laying open the arcana of the nose, frontal sinuses, throat, voice-box, and chest, and showing the intimate relations by which these parts are connected, and the way in which modern science has found means to bring their inmost recesses under observation, and contrasting the precise knowledge of the present period with the ante-Schneider days, when all catarrhal defluxions were held to be outpourings of the brain. It was demonstrated that the ordinary cold is simply, in the first instance, congestion of the warm, moist, blood-charged membrane, which lines all these cavities and is continuous throughout the series of them; but that this congestion is apt to pass on, under unfavourable circumstances, to inflammation, and to consequent derangement of structure. The congestion merely means that more blood is thrust upon, and retained in, the minute channels and vessels of the membrane, than those channels and vessels can healthily accommodate. The first cause of this forced engorgement is that cold is extensively applied to the internal skin, which then, under the constringing and contracting influence, drives its own blood out, partly into these surcharged tracts of mucous membrane. The injurious effect known as "cold" is now sure to be realised if this external chill is experienced when the general system is weakened by exhaustion. It is also, in some persons, more apt to be produced at certain regular periods.

The prevention of colds is to be accomplished by keeping the skin in a healthy and vigorous state, so that it may at once resume its proper and normal condition when chills have been suddenly applied to it: then the internal congestions are avoided or removed simultaneously with the external contraction and stagnation. The habitual use of cold bathing in the early morning is one very powerful means to this end: it trains the vessels of the skin to rise vigorously into renewed action after the application of a chill. The relaxing influence of over-heated apartments should be avoided, because that saps the power of vigorous reaction; but, in cold weather, the utmost care should be taken to have the entire skin efficiently protected by warm clothing. The powers of the system in periods prone to the production of colds, and most especially when the temperature of the external

air is between 32 and 40 degrees of Fahrenheit's heat scale (for that is the condition in which the danger is found to be most certainly incurred), should be most carefully maintained by the judicious use of sustaining food, and by the avoidance of every kind of injurious derangement or excess. When once internal congestion has been set up, and the cold has been "caught," the thing to be done is immediately to bring back vigorous circulation and exhalation in the skin. The Turkish bath is one of the most convenient and certain of all contrivances for ensuring this object: in its absence the vapour bath, or hot air bath may be employed. The action of the bath is to be reinforced by the administration of stimulants, first and foremost amongst which stands concentrated food. Indeed, the Professor's pet stimulant seems to be "Whitehead's Solid Essence of Beef," a New South Wales preparation, in which the nutritious principle of an ox is condensed into about nine pounds of easily transportable material, in which thirty pounds of beef are concentrated into one pound of little cakes, each about the size of an ordinary silver five-shilling piece, and weighing half an ounce. One cake is calculated to prepare two large breakfast cups of good beef-tea. This preparation differs from Liebig's Extract of Meat chiefly in containing the gelatinous as well as the fibrinous constituents of the flesh. The Gresham Professor scattered the little round cakes, out of neat half-pound cases, liberally to his audience, recommending them to begin at once to fortify themselves against the inclement atmospheric influences. He gave one very interesting instance of the value and power of this preparation by alluding to a case that had fallen within his experience on the very day of the lecture. A patient had been brought into the Brompton Hospital in a sinking state, resulting from inability to take food. He was at the time all but pulseless and cold, and evidently on the brink of the grave. He was placed in bed, and a cupfull of the beef-tea prepared from the "solid Essence" administered. The preparation was retained in the stomach, and in ten minutes from the time of its administration, there was steady warmth all over the skin, and restored circulation.

There is one expedient both for preventing and curing "colds." which was not alluded to upon this occasion, but which is nevertheless as powerful as any of the measures which were described, and it may sometimes be drawn upon in circumstances when those plans cannot be adopted, in consequence of the sufferer being compelled by the exigencies of life to continue to meet exposure to chilling influences. This is abstinence from drink, and liquid food of any kind, until the internal congestion is removed. The remedial action through the skin does its work by drawing away the superabundance of the circulating fluid from the overcharged part. But this desirable result is even more certainly ensured if the general bulk of the circulating fluid, or blood, is diminished by withholding supplies of the more liquid, or watery, ingredient; which may be done where the digestive power is unimpaired, without in any way diminishing the richer, or more immediately nourishing portion. The instant the general bulk of the circulating blood is diminished, the excess contained in the congested and overcharged membranes is withdrawn and the cold is relieved. Somewhat severe thirst sets in; but curiously enough, simultaneously with the occurrence of this thirst, the congested internal membranes grow moist, and exhale gently and naturally in consequence of the relief of the overcharged vessels. All that is then necessary is to keep the supply of drink down to the point which enables some measure of thirst to be maintained; and during its maintenance there is not the slightest chance of the recurrence of the cold. Dr. Thompson dwelt emphatically in his lecture, upon the fact that, whereas certain ailments, such as the eruptive fevers, bring with them an almost complete immunity from the recurrence of the affection, it is just otherwise with ordinary colds. The more frequently they occur, the more frequently they may be looked for. They bring with them increased susceptibility of the internal membranes to congestive derangements. Under such circumstances diminution of drink, sustained at the point of persistent moderate thirst, is the most powerful and certain preventive of congestive disorder, and the most sure remover of undue internal susceptibility, that can be adopted.

The second lecture was mainly devoted to a description of stimulants, and an experimental explanation of the way in which the amount of the saccharine, acid, and spirituous ingredients of wines may be ascertained. There is one very noteworthy peculiarity in all these lectures, which renders them peculiarly fit for the class of audience at which the Gresham College is aimed: Dr. Symes Thompson is a master of the art of giving a clear notion of the whereabouts of a fact, or principle, to popular apprehension. It is to be extremely regretted that in a vast metropolis like ours, and, indeed, in all our large towns, such courses as these Gresham Lectures are not more common. It is absolutely impossible to over-estimate the good they can do, not only, as in this case, in showing what "to eat, drink, and avoid," but generally in inducing thought and work. Further lectures of the Gresham course are announced for the months of April, June, and September.

LETTERS TO THE EDITOR

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

Where are the Nebulæ?

MR. PROCTOR'S interesting paper in your last number reminded me of an essay on "The Nebular Hypothesis," originally published in 1858, and re-published, along with others, in a volume in 1863 ("Essays: Scientific, Political, and Speculative." Second Series), in which I had occasion to discuss the question he raises. In that essay I ventured to call in question the inference drawn from the revelations of Lord Rosse's telescope, that nebulæ are remote sidereal systems—an inference at that time generally accepted in the scientific world. On referring back to this essay, I find that, besides sundry of the reasons enumerated by Mr. Proctor for rejecting this inference, I have pointed out one which he has omitted.

Here are some of the passages:—

"'The spaces which precede or which follow simple nebulæ,' says Arago, 'and, *à fortiori*, groups of nebulæ, contain generally few stars. Herschel found this rule to be invariable. Thus, every time that, during a short interval, no star approached, in virtue of the diurnal motion, to place itself in the field of his motionless telescope, he was accustomed to say to the secretary who assisted him, "Prepare to write; nebulæ are about to arrive."' How does this fact consist with the hypothesis that nebulæ are remote galaxies? If there were but one nebula, it would be a curious coincidence were this one nebula so placed in the distant regions of space as to agree in direction with a starless spot in our own sidereal system? If there were but two nebulæ, and both were so placed, the coincidence would be excessively strange. What, then, shall we say on finding that they are habitually so placed? (the last five words replace some that are possibly a little too strong). . . . . When to the fact that the general mass of nebulæ are antithetical in position to the general mass of stars, we add the fact that local regions of nebulæ are regions where stars are scarce, and the further fact that single nebulæ are habitually found in comparatively starless spots, does not the proof of a physical connection become overwhelming?"

The reasonings of Humboldt and others proceeded upon the tacit assumption that differences of apparent magnitude among the stars result mainly from differences of distance. The necessary corollaries from this assumption I compared with the hypothesis that the nebulæ are remote sidereal systems in the following passage:—

"In round numbers, the distance of Sirius from the earth is a million times the distance of the earth from the sun; and according to the hypothesis, the distance of a nebula is something like a million times the distance of Sirius. Now, our own 'starry island, or nebula,' as Humboldt calls it, 'forms a lens-shaped, flattened, and everywhere-detached stratum, whose major axis is estimated at seven or eight hundred, and its minor axis at a hundred and fifty times the distance of Sirius fom the earth.' And since it is concluded that our solar system is near the centre of this aggregation, it follows that our distance from the remotest parts of it is about four hundred distances of Sirius. But the stars forming these remotest parts are not individually visible, even through telescopes of the highest power. How, then, can such telescopes make individually visible the stars of a nebula which is a million times the distance of Sirius? The implication is, that a star rendered invisible by distance becomes visible if taken two thousand five hundred times further off!"

This startling incongruity being deducible if the argument proceeds on the assumption that differences of apparent magnitude among the stars result mainly from differences of distance, I

have gone on to consider what must be inferred if this assumption is not true; observing that "awkwardly enough, its truth and its untruth are alike fatal to the conclusions of those who argue after the manner of Humboldt. Note the alternatives":—

"On the one hand, what follows from the untruth of the assumption? If apparent largeness of stars is not due to comparative nearness, and their successively smaller sizes to their greater and greater degrees of remoteness, what becomes of the inferences respecting the dimensions of our sidereal system and the distances of the nebulæ? If, as has lately been shown, the almost invisible star, 61 Cygni, has a greater parallax than α Cygni, though, according to an estimate based on Sir W. Herschel's assumption, it should be about twelve times more distant—if, as it turns out, there exist telescopic stars which are nearer to us than Sirius, of what worth is the conclusion that the nebulæ are very remote, because their component luminous masses are made visible only by high telescopic powers? . . . . On the other hand, what follows if the truth of the assumption be granted? The arguments used to justify this assumption in the case of the stars, equally justify it in the case of the nebulæ. It cannot be contended that, on the average, the *apparent* sizes of the stars indicate their distances, without its being admitted that, on the average, the *apparent* sizes of the nebulæ indicate their distances—that, generally speaking, the larger are the nearer, and the smaller are the more distant. Mark, now, the necessary inference respecting their resolvability. The largest or nearest nebulæ will be most easily resolved into stars; the successively smaller will be successively more difficult of resolution; and the irresolvable ones will be the smaller ones. This, however, is exactly the reverse of the fact. The largest nebulæ are either wholly irresolvable, or but partially resolvable under the highest telescopic powers; while a great proportion of quite small nebulæ are easily resolvable by far less powerful telescopes."

At the time when these passages were written, spectrum-analysis had not yielded the conclusive proof which we now possess, that many nebulæ consist of matter in a diffused form. But quite apart from the evidence yielded by spectrum-analysis, it seems to me that the incongruities and contradictions which may be evolved from the hypothesis that nebulæ are remote sidereal systems, amply suffice to show that hypothesis to be untenable.

37, Queen's Gardens, Jan. 31 HERBERT SPENCER

Kant's View of Space

IN answer to my invitation, Mr. Lewes now "freely admits that Kant nowhere speaks of Space and Time as 'Forms of Thought,'" but still contends that 'Kant would not have disclaimed such language, as misrepresenting his meaning.' As well might he argue that although Euclid never uses the word *epipedon* (our English word *plane* or *plain*), to signify a curved surface (επιφανεια), he would not have remonstrated against the use of the term *cylindrical epipedon* or conical *epipedon*, to denote the surface of a cylinder or cone, in a professed exposition or criticism of his Elements of Geometry, because in common life we speak of rough or undulating plains, or because a plane admits of being bent into the shape of a cylindrical or conical surface. I think the ladies who are getting up their Planes and Solids at St. George's Hall would be of a different opinion from Mr. Lewes in this matter, and with good reason on their side.

Mr. Lewes, reiterating a statement contained in his previous letter, goes out of his way to affirm that he "uniformly speaks of Space and Time as forms of Intuition in his pages of exposition" of Kant's doctrine in his "History of Philosophy." Were the fact so, it would not in any material degree excuse the inaccuracy of subsequently styling them "forms of Thought;" and, moreover, the real point at issue is not Mr. Lewes's general accuracy or inaccuracy, but whether a mode of speech which he, along with others, employs, is right in itself and ought to be persisted in.

However, as Mr. Lewes has thought fit to put in a sort of plea in mitigation of former wrong-doing, I have taken the trouble of looking through his *exposition* and *criticism* of Kant in his History (ed. 1867), and in no single instance have I come upon the phrase *forms of intuition* applied to Space and Time, either in the one or the other; although he states he has *uniformly spoken of them* as such in the former. I have marked the word *intuitions* as occurring once, and *forms of sensibility* several times, but *forms of intuition* never. If *form of sensibility* is as good to use as *form of intuition*, *form of understanding* ought to be as good as *form of thought;* but Mr. Lewes owns that the former is indefensible, whilst he avers that the latter is correct. If Mr. Lewes has ever called Space and Time *forms of intuition* in the History, it will be easy for him to set me right by quoting the passage where the phrase occurs, although that circumstance would not in any degree better his own position, and still less excuse the assertion of his *uniform* use of the term.

If Mr. Lewes cannot quote correctly from his own writings, it will surprise nobody that he misquotes the language of an opponent. He repeats, "Intuition without thought is mere sensuous impression," and adds, "Mr. Sylvester demurs to this." "My words are" (NATURE, Jan. 13, 1870): "To such a misuse of the word energy it would be little to the point to urge that *force without energy is mere potential tendency.* It is just as little to the point in the matter at issue for Mr. Lewes to inform the readers of NATURE that *intuition without thought is mere sensuous impression.*" So that, according to Mr. Lewes, to say that a proposition is *little to the point* is *demurring to its truth.*

I should not hesitate to say if some amiable youth wished to entertain his partner in a quadrille with agreeable conversation, that it would be *little to the point*, according to the German proverb, to regale her with such information as how

> "Long are the days of summer-tide,
> And tall the towers of Strasburg's fane,"

but should be surprised to have it imputed to me on that account that I demurred to the proposition of the length of the days in summer, or the height of Strasburg's towers.

In another passage, Mr. Lewes gives me credit for "saying correctly that Intuition and Thought are not convertible terms"—a platitude I never dreamed of giving utterance to; but that I am "incorrect in assuming that they differ as potential and actual"—words which, or the like of which, in any sort or sense, never flowed from my pen. Surely this is not fair controversy, to misquote the words and allegations of an opponent. It seems to me too much like fighting with poisoned weapons. I decline to continue the contest on such terms; and, passing over Mr. Lewes's very odd statement about *species and genus* with reference to Intuition and Thought, shall conclude with expressing my surprise at his and Mr. G. C. Robertson's confident assumption that Kant uses in the title of his book *pure reason* in a far wider sense than in the body of his work, simply because to arrive at the Pure Reason he has to *go through* the Critick of the Sensibility and of the Understanding. If in a history of the Reign of Queen Victoria the author should find it expedient to go back to the times of the Norman and Saxon conquests, would it be right to infer therefrom that he used in his title-page the name Victoria in a generalised sense, to include not only her most Gracious Majesty, but also the Tanner's daughter and Princess Rowena?

Perhaps by this time many of the Naturalistic readers of the journal who regard the human intelligence as forming no part of the scheme of Nature, wish Space at the bottom of the sea; but the more the subject is canvassed, and the greater the number of English authorities brought forward to back up Mr. Lewes in wresting the words of Kant from their proper scientific signification, the higher meed of praise seems to me to accrue to Dr. Ingleby for stemming the tide of depravation, and banishing, as I feel confident this discussion will have the effect of doing, from the realm of English would-be philosophy, such a loose and incautious way of talking as that of giving to Space and Time the designation which the Master has appropriated to the categories of his system, and to them alone. J. J. SYLVESTER

P.S.—I should be doing injustice to the very sincere sentiments of respect I entertain for Mr. Lewes's varied and brilliant attainments (which constitute him a kind of link between the material and spiritual sides of Nature), and of gratitude for the pleasure the perusal of his "History of Philosophy" has afforded me, were I to part company with him without disclaiming all acrimony of feeling, if perchance any too strident tones should have seemed to mingle with my enforced reply. In naming him in the original offending footnote (the fountain of these tears), my purpose was simply to emphasise the necessity of protesting against what seemed to me an unsound form of words, *apropos* of Kant, which went on receiving countenance from such and so eminent writers as himself and the others named; and I should be false to my own instincts did I not at heart admire the courageous spirit with which, almost unaided and alone (like a good knight of old), he has done his best to defend his position and maintain his ground against all oppugners.

J. J. S.

It is hardly possible to exaggerate the importance of the question now under discussion in NATURE, "What was Kant's view of Space?" A mistake there is simply fatal. I therefore rejoice to find the columns of that paper are so generously thrown open to those who, like myself, are not primarily concerned with physical science. But this question, like all others in philosophy, has a proclivity to indefinite expansion, and unless its discussion be rigidly restricted to the main issue involved in it, the conductors of NATURE will have to ostracise it. Their space is not an infinite form, but a quantum to be carefully economised. It is, for example, an unwarrantable waste of that commodity to make Hegel the exponent of Kant on a point where Hegel taught that Kant was wrong.

It is fortunate for our interests, as students of Kant, that Mr. Lewes, while committing the strange oversight of criticising Kant's Intuition from Hegel's standpoint, in his last letter (NATURE, Jan. 27) enables us to *démêler* the main issue from the mass of questions which entangle it. He evidently, if tacitly, slights the plank I threw to him, viz., that Thought, in its ultimate relation to Intuition, borrows, or has reflected on it, the forms peculiar to Sense. What are Kant's *Begriff vom Raume*, *Begriff der Zeit*, but this? (With these expressions, compare the following:—Also ist die ursprüngliche Vorstellung vom Raume Anschauung *à priori* und nicht Begriff. Transac. Æsth. s. 3, 4.) This reflection of form is not what Mr. Lewes is after. He maintains that, according to Kant, "the activity of mind is threefold—Intuitive Thought, Conceptive or Discursive Thought, and Regulative Thought." (Is not Regulative Thought discursive?) So, then, the main issue between Mr. Lewes and (I think) Professor Croom Robertson on the one hand, and Professors Sylvester and Huxley, Mr. W. H. S. Monck, and myself on the other, is plainly this. *Did Kant mean to teach that man has Intuitive Thought, i.e., Intellectual Intuition?* Now *that* I must be understood emphatically to deny; and in the event of the shortcomings of better men than myself, I hold myself prepared to establish the negative of that question, understanding by Thought the *genus* of which Understanding and Reason are *species*.

Ilford, Jan. 31 C. M. INGLEBY

Dust and Disease

THE extremely important discoveries brought to light by Professor Tyndall will call forth great exertions on the part of thinking persons to carry his plans into operation, and I have no doubt, when due precautions are taken to sift infected air as it passes into the lungs of those whose duties take them where contagion abounds, we shall have the happiest results.

So great will be the tide of interest in this direction, that I am anxious to cast into it a theory I have long held, in hopes that it may drift in some one's way to be turned to use; I commend it to the travelling portion of your readers especially.

Whilst travelling in some very unhealthy parts of Africa, more particularly amongst the marshes bordering on the Shiré and Zambesi rivers, it was often necessary to camp at night just where the canoe happened to be moored when daylight failed us. Reeds, rushes, and mud were never many feet off, and the accumulation of scum, decaying vegetation, &c., lodged in the sedge, made the situation as delightful to mosquitoes as it was trying to the constitution of the European.

Still, with all this, as long as it was possible to rig up a mosquito curtain, I am convinced that really less danger existed in thus sleeping in the midst of miasma than in other places where less of it was supposed to be present, but where the traveller felt no necessity to stretch this thin covering over him.

I have in this way done canoe journeys of twenty to twenty-five days in length without a day's illness from fever, and I could instance similar experiences on the part of others.

Now the reason I assign is this: the mosquito curtain is to miasma, what the Professor's cotton-wool respirator is to the poison of scarlatina, we will say.

The curtain, after being used once or twice, saturated with dew, folded up whilst damp and crammed into the limited space generally provided for it in the safest place, becomes just so much affected by this treatment that each thread loses its smooth glaze, and is soon fluffy and fuzzy for want of a better expression.

The little honeycomb holes in the fine "net" are now a series of small six-sided sieves, each covered over with the fine filaments of cotton which have got disturbed and frayed up. Dew, falling upon a surface of this kind, quickly turns it into an exquisitely fine strainer—in fact almost a film of water—through which all the air has to pass which is breathed by the person reposing beneath it.

Now, it is an old notion that the miasma which produces the bilious remittent fever (the pest of this part of Africa in question) and various other diseases of the tropics, cannot pass across water.

I believe that acting upon this theory, the Admiralty provides that boats' crews shall sleep in their boats anchored off shore in malarious rivers. However, be this as it may, I have a strong belief that the "wet sieve" *does* stop the poison in some way or other, and that it is a great safeguard to the voyager in these places.

The whole subject of miasm is in the dark; it is lawless as a cause of disease; it baffles the most astute, but the day may be coming when such hints as these of Prof. Tyndall's shall fit into an organised attack upon it, and we shall be able to overcome it in a measure.

A curtain, properly made, and taken care of with that instinct which alone is begotten by the buzz of mosquitoes, is perhaps the most valuable possession a man can have a gainst deadly attacks in the night whilst men are asleep: were its merits studied more, we should not find men stuffing their companions so perpetually with quinine, to the keeping up an unhealthy tone by this abuse alone, and to the confusion of this most invaluable medicine when it is really called in to do its duty upon the fever-stricken patient.

Chatham, Jan. 24 HORACE WALLER, F.R.G.S.

Scenery of England and Wales

THE willingness you have hitherto shown to give authors an opportunity of defending themselves against being misunderstood, induces me to hope that you will allow me to disclaim being the author of certain statements, and to deny the truth of other statements, on which an anonymous reviewer in your last number mainly founds the charge of boldness he brings against me for writing the work entitled "Scenery of England and Wales," &c.

In one part of the review *I am made to say* that I "purposely refrained from reading;" in another it is assumed that my reading has "consisted mainly of the recent journals and magazines;" and further on it is asserted that I wrote the book "without reading."

The facts are, that for many years I devoted more or less time to reading on the subject of Denudation, and that, as stated in the Preface, until lately I purposely refrained from "reading *very much* (a distinct thing from not reading) lest a bias should be given to my opinions."

My reason for not quoting the remarks of the late Principal Forbes on the glaciers of Norway, was not, as implied by your reviewer, because I underrated the *denuding power of glaciers*, but because Forbes said very little on the subject.

Mrs. Somerville's estimate of the velocity of the Rhone may be incorrect, and perhaps, likewise, her statement that the declivity of the river is 1 foot in 2,620; but this is no reason why your reviewer should leave the reader to suppose that I misquoted Mrs. Somerville. In other parts of the work I have referred to the velocities of many currents besides the one off the southern promontory of Shetland.

The argument against denudation by currents, derived from the non-displacement of *barnacles*, would, I think, never be brought forward by any one acquainted with the fact that sea-waves often remove stones and large blocks while barnacles in the immediate neighbourhood are left undisturbed—that waves and currents, by their insinuating, undermining, overturning, and removing action, can carry on the work of denudation within a few inches of an unabraded rock-surface—and that a certain amount of resistance to be overcome is necessary to enable all denuding agents to produce effects which can be immediately perceived. On the western shore of Morecambe Bay, sea-waves and currents detach and remove fragments of limestone rock by a lateral process, while the brink of the unremoved mass of rock retains its glacial polish; and many other instances illustrative of this subject might be stated.

The fact that for more than twenty years I have *confined* my observations to England and Wales, and devoted nearly my whole time to visiting, revisiting, and studying every part of the country, is no reason why I should not have ventured to write a work on the Scenery of England and Wales in connection with Denudation. The country stands almost alone as regards the variety and importance of its geological phenomena, including

surface-features and types of scenery. My work is not confined, as your reviewer asserts, to a defence of marine denudation, for more than a third of it is devoted to the consideration of the real or assumed effects of atmospheric agents; and instead of being put forward in a self-confident spirit, as your reviewer would likewise lead the reader to suppose, I have stated in the Preface that "my object will be gained if I have said enough to stimulate the geologist and intelligent tourist to further observation." D. MACKINTOSH

"Correlation of Colour and Music."

ANALOGIES between tone and tint are a tempting subject; and sound and light have enough admittedly in common to make it rash to say that the connection may not extend to their effects on the ear and eye; but that your correspondents (Jan. 13th and 20th) are seeking for unity in a direction in which it is not to be found, seems to me to be rendered pretty certain by the very evidence to which one of them, Dr. de Chaumont, appeals (Jan. 20th); I mean by that of "the researches of Helmholtz and others."

I have often wondered at the small attention paid to the general law which these researches have established. Even M. Jamin, in his *Cours de Physique*, dismisses Newton's theory of compound colours as "empirical," and apparently of no significance. It is as much and as little empirical as the Newtonian astronomy; both consist of general laws applied by means of particular constants: the evidence for the laws is in both cases equally inductive, and the determination of the constants equally empirical.

Stated without reference to the geometrical and dynamical analogies which I suspect have had something to do with obscuring its significance and tainting it with "empiricism," the fundamental law of composition of colour is this:—

Of any four colours whatever, either there is one which may be matched by a compound of the other three, or there are two which may be compounded so as to match a compound of the other two.

It is obvious that if negative values of an ingredient can be admitted, these alternative cases are the same; and the geometrical and dynamical analogies depend on the fact that, if *addition of vectors* is substituted for *composition of colours*, the proposition remains true, becoming in fact a very elementary one. And it follows that all colours may be co-ordinated, by means of three independent variables, with reference to any three colours whatever.

Accordingly, when differently coloured lights reach the eye together, the combination produces a single resultant colour varying according to the proportions of the ingredients, and completely superseding them; whereas, when two sounds of different pitch are sounded together, we still hear both: and, though we hear certain other tones besides, these other tones have each a pitch determined by the pitches, but independent of the intensities, of the original sounds.

The truth is, that the ear and eye deal with impressions in totally different manners. The ear deals with a complex musical sound exactly as a system of resonators does; it sensibly decomposes the sound into certain simple tones, just as the complex harmonic motion which produced the sound is theoretically decomposed by Fourier's theorem into the simple harmonic motions which would produce the simple tones. In order to understand the manner in which the eye deals with a compound colour, we must turn our attention to that particular unidimensional series of colours which constitutes the spectrum. As this is what your correspondents have done, the issue will be all the closer.

By the law above stated, all the colours of the spectrum might be co-ordinated with reference to any three colours chosen in the spectrum or out of it. But it has been ascertained by Mr. Maxwell* (to whose labours we are chiefly indebted in England for what we know of the composition of colours) that there are three colours in the spectrum to which all the rest stand in relations giving these the character of "primary" colours. They are the particular red, green, and blue, whose wave-lengths are, in Fraunhofer's measure, respectively 2328, 1914, 1717: and they divide the spectrum into three parts, in each of which every colour, it appears, may be matched by a compound of the two (out of these three) between which it lies. This is very accurately the case between 2328 and 1914 and between 1914 and 1717: it is much less accurately the case on the red side of 2328 and on the violet side of 1717; but in this region observation is difficult, and various eyes variable; and it seems probable that, as Mr. Maxwell infers, every colour in the spectrum, and therefore every colour in nature, is, as felt by us, a compound of three elementary sensations of colour excited separately by those three rays.

* Phil. Trans. 1860, pp. 57–84. On the theory of compound colours and the relations of the colours of the spectrum.

Now it must be observed that this result does tend to justify so much of the anticipations mentioned by Mr. Barrett as Sir John Herschel and Mr. Grove had long ago committed themselves to: it shows that the spectrum, like the musical scale, does in a manner return into itself. Beyond this the analogy fails.

In the first place there are, in music, no fixed tones with reference to which other tones possess any general properties at all; much less the property of being matched by combinations of them. In relations of tone, the constant quantities are not constant tones, but constant intervals between tones. Still, no doubt, if the three primary colours stood (as Dr. de Chaumont seems to think they do) in the arithmetical relations of tonic, fourth, and fifth, the fact would be as remarkable as two numerical coincidences could make it. But the case is not so. The ratio 2328:1914 (or 1·211) corresponds not to the interval of a fourth, but to an interval about two-ninths of the way from a minor third (6:5 or 1·2) to a major third (5:4 or 1·25); and the ratio 2328:1717 corresponds not to the interval of a fifth, but to an interval about a third of the way from a natural fourth (4:3 or 1·333) to a sharp fourth (45:32 or 1·406); intervals which, I presume, one can make nothing of.

Mr. Barrett's principal argument depends upon Prof. Listing's demarcation of the colours answering to the names red, orange, &c.: much too vague a basis, I should have thought, for exact inference, even if Mr. Barrett had not been obliged to sacrifice a boundary to obtain his most important interval; moreover the, correspondences in Table IV. are somewhat exaggerated for orange and yellow by errors of computation. Mr. Barrett does certainly get a good fourth, fifth, and sixth; but these coincidences seem to me to offer a simpler and more effectual key than that which he has applied to the lock. What this key is will be evident on substituting for the numbers in Table I. or II., the reciprocals of the same numbers. Take Table I. and divide ten millions by each of the numbers. The results, with a column of differences, are as follows:—

1382·3	
	162·8
1545·1	
	162·6
1707·7	
	162·5
1870·2	
	162·7
2032·9	
	162·5
2195·4	
	162·5
2357·9	
	162·9
2520·8	

I suppose this speaks for itself. Professor Listing has simply divided his spectrum into seven equal parts upon some scale which varies inversely as the wave-length. Such a scale would of course be furnished by comparative rapidities of vibration; but it is no use guessing. Whatever led him to this particular measure, it is evident what his measure virtually was, and it nearly corresponds with the ratios of the musical scale because these approximately form a "harmonic" progression.

The other suggested analogies are less definite. "The juxtaposition of two colours nearly alike is *bad*," but surely not what would be called *discordant*, except for the sake of finding an analogy between colour and music. The fact probably depends upon the extreme sensitiveness of the eye to the effect called *relief*; a sensitiveness shared in a different degree by the ear, but shared also by all modes of feeling, even the least material, as men count materiality. The best results of juxtaposition are generally those given by complementary colours: but the relation between complementary colours is one which depends partly on relief and partly on the laws of composition above stated, and has nothing corresponding to it in music. But indeed I am surprised that anybody should even look for an analogy between the effect of *simultaneous* sounds and the effect of *contiguous*, not *coincident*, colours.

For these reasons I venture to think it is only by the unphilosophical restriction of the word *physical*, which excludes biological relations, that "harmony in colour and music" can be said to "have a common physical basis."

With regard to the coloured bands within the rainbow, it is not doubtless without solid reasons that Mr. Grove can have decided against identifying the phenomenon he describes with

the phenomenon usually attributed to the interference of the two rays which, distinct at incidence, coincide on emergence. However this may be, the fact can have nothing to do with the present question. It is impossible, when the most refined spectroscopic observation hardly gives us one octave, that the rainbow should disclose three or four.

Hyères, Jan. 25 C. J. MONRO

Flight of Birds

WITH reference to an abstract of a paper by Mr. Southwell on the flight of birds, which appeared in your paper a few weeks back, I venture to make the following note and inquiry.

A late brother of mine who had been round the Cape, and had frequently observed carefully the flight of the albatross, told me that though he had watched individual birds on several occasions for many minutes together, he had seldom or never seen any of them flap their wings. Has this fact been observed by other persons, and if it be authenticated, has Mr. Southwell any explanation to offer? J. H.

Turdus Whitei

IT may interest some of your ornithological readers to know that a specimen of White's Thrush, *Turdus Whitei*, was killed near here on the 7th inst. It was shot by Mr. J. Beadon, of Gotten House, and presented by him to Mr. Cecil Smith, of Lydeard House, for his collection. This is, I believe, the fourth occurrence of this bird in Britain; it is, I think, even less known on the Continent. W. BIDGOOD

Museum, Taunton, Jan. 15

THE SARS FUND

WE are glad to find that the appeal made in our pages by Mr. Gwyn Jeffreys, on behalf of the family of the late Professor Sars of Christiania, is being warmly seconded in Paris by M. Alglave, the Editor of the *Revue des Cours Scientifiques*. In the last number of the *Revue* Mr. Gwyn Jeffreys' article is reprinted *in extenso*, and an announcement made that subscriptions to the Sars Fund will be received at the office of that journal. But M. Alglave has not waited for the publication of his notice before beginning his good work; he has already collected the sum of 2,026 francs (81*l.*), and publishes with the notice a first subscription-list containing the names of many of the most eminent naturalists in France. We have now the pleasure of giving in our advertising columns a list of the contributions already promised to Mr. Gwyn Jeffreys. Sars belonged to the best type of scientific men, the genuine lover of science, contented to work in obscurity without thought of honours or reward. His family have a special claim to help, inasmuch as the distress in which they are left is not due to neglect or extravagance on the part of the lamented Professor, but is solely attributable to his having devoted himself to studies, which, notwithstanding the most self-denying labours, did not enable him to make any provision for the future. Those of our readers who have visited Norway, who know the genuine unworldly ways of the Norwegians, and who have enjoyed the enthusiastic welcome so readily given to the English, have now a graceful opportunity of reciprocating the kindly feeling shown them by the countrymen of Sars.

NOTES

LORD DE GREY will to-morrow receive the Council of the British Association, as a deputation to urge upon her Majesty's Government the desirableness of a Royal Commission being issued to inquire into the present state of Science in this country.

THE Royal Society and the Royal Astronomical Society have both appointed committees of council, to report upon the steps which it may be desirable to take in connection with the total eclipse of the sun, visible in Algeria, Spain, and Sicily, in December next.

IT is proposed to celebrate the fiftieth anniversary of the Royal Astronomical Society by a dinner on the day of the annual meeting, February 11.

THE lectures annually delivered in the theatre of the Royal College of Surgeons were commenced yesterday by Professor Erasmus Wilson, F.R.S., who will deliver six lectures on Dermatology. To illustrate that of yesterday, there were a large number of exquisitely-prepared models, duplicates of those prepared for the Hôpital St. Louis, Paris. Professor Wilson's course will be followed by Professor Flower, F.R.S., Conservator of the Museum, who will deliver 18 lectures introductory to the study of the anatomy of the class mammalia.

THERE is a terrible rumour to which we are compelled to refer, though it has reached us in a—let us hope—doubtful way. The news is, that Livingstone has been murdered and burnt ninety days' journey from the Congo.

THE Zoölogical Society of London have now in the press a memoir by Dr. O. Fiusch, of Bremen, one of their corresponding members, upon the birds collected by Mr. William Jesse during the recent Abyssinian expedition. This memoir, which was read before the society at one of their meetings last year, will appear in their quarto Transactions, and will be illustrated by coloured figures of the new or little-known species. It will be in the recollection of some of our readers that Mr. Jesse was selected by the Zoölogical Society, at the request of the Treasury, to accompany the Abyssinian expedition as zoölogist, and that the society undertook the task of bringing the results of his investigations before the public. Dr. W. Peters, of Berlin, has undertaken to prepare a memoir upon some of the rarer mammalia obtained by Mr. Jesse during the expedition, which will likewise appear in the Transactions. Mr. W. T. Blanford, who was sent out by the Indian Government as geologist to the expedition, is preparing a separate work upon his discoveries, which will be published by Messrs. Macmillan.

AT its last meeting (January 24), the Academy of Sciences elected a new correspondent for the Physical section in the place of the late Professor Forbes. M. Kirchhoff, who was the successful candidate, obtained forty votes; Mr. Lloyd and Sir William Thomson received one vote each.—A committee of the three sections of Astronomy, Geometry, and Navigation selected the following candidates for the vacancy in the *Bureau des Longitudes*:—1. M. de la Roche Poncié; 2. M. Gaussin.

IT is no secret that the present *régime* at the Observatory of Paris has been rather more autocratic than could be patiently endured, even in a country subjected to eighteen years of personal government. Matters have at length reached a crisis, and the Minister of Public Instruction is placed in the awkward position of having to dismiss from the public service one of the most eminent of modern astronomers, or accept the resignation of the whole of the rest of the staff of the Observatory.

THE chair of Chemistry at the University of Tübingen has been offered to Prof. Fittig.

WE regret to have to announce the death of a naturalist of great promise, Dr. Wilhelm Keferstein, Ordinary Professor of Zoölogy and Comparative Anatomy in the University of Göttingen. He died on the 25th ult., at the early age of 37.

THOSE of our readers who are acquainted with the grand series of ethnographical photographs contained in the four volumes already published of the work entitled the "People of India," will be glad to hear that four more instalments completing the volume are now in progress. The publishers are Messrs. W. H. Allen and Co., of Waterloo Place.

M. SCHAFARIK, Professor of Chemistry in the Polytechnic Institute of Bohemia, writing on the 15th ult. to the Bohemian newspaper *Politik*, announces the discovery of a diamond in a

granite quarry in the Dlaschkowitz estate of Count Schönborn, about eight miles north-west of Prague. The stone weighs 57 milligrams, is of a light yellow colour, and nearly cubtical in shape, the edges and corners being slightly rounded. In hardness it equals the East Indian, and exceeds the Brazilian diamonds. After the careful examination which the stone has been subjected to, by Prof. Schafarik, there can be little doubt of its genuineness ; but that simple fact will hardly justify us in believing that a diamond has really been found associated with basalt and other minerals of plutonic origin, seeing that the beds which, in other parts of the world, have yielded this precious stone, are all sedimentary deposits. The Dlaschkowitz diamond had confessedly passed through the hands of a lapidary (who was unable to polish it on account of its hardness) before it reached M. Schafarik, and it would be reasonable, for the present, to suppose that an East India diamond had by some accident become mixed up with the various Bohemian stones sent to the lapidary. It has been suggested in Bohemia that the Dlaschkowitz diamond is nothing but zircon ; but the stone does not agree with that mineral, either in specific gravity or in hardness. To those of our readers who are interested in the diamond discoveries in the Cape Colony we may commend an article "On the Diamond Regions of South Africa" in the *Field* newspaper of the 22nd ult.

In a recent number of NATURE we gave an account of some experiments by Lenz on the occlusion of hydrogen by electro-deposited iron. It will be remembered that in the discussion that followed the reading of M. de Jacobi's paper before the British Association at Exeter, Mr. W. Chandler Roberts stated that electro-type iron occluded at least twenty times its volume of hydrogen. The extraction of the gas was followed by a contraction of the metal. This Mr. Roberts considered important, from its connection with the behaviour of Palladium under similar conditions.

The last bulletin of the Association Scientifique de France publishes three accounts of shocks of earthquakes at Marseilles and Toulon on the morning of the 18th ult. M. Stephan, of Marseilles, speaks of a smart shock at 2.50 A.M., the direction of the oscillation being from north to south, and lasting three seconds. Another slight shock, having the same direction, was noted by him at 3.5 A.M. M. Ferrier, of Marseilles, observed prolonged oscillations at 2.45 A.M. According to him, there were twenty or twenty-five oscillations from south-west to north-east, the intensity of the oscillations being all equal, and the duration of each one-third of a second. M. Zurchen, writing from Toulon, mentions two violent shocks at 3.7 A.M. There was an interval of two seconds between them. The oscillations appeared to be from north to south.

In the matter of sewage, as in so many other particulars, the metropolis allows itself to be outdone by provincial towns. Leamington, for example, has had its sewage examined by Dr. Letheby ; one sample after it had passed the charcoal filter, the other before being subjected to that process, although it had undergone chemical influences. In one of these, Dr. Letheby found only 8·40 grains of organic matter (in solution), and in the other, only 9·40 grains; whereas, ordinary London sewage contains 15·08 grains. Again, of mineral or organic matters in suspension, the two Leamington samples yielded none whatever, but on the other hand, the London sewage contained 22·04 grains of mineral, and 16·11 grains of organic matters in suspension. Now, why is there this contrast? And yet nearly all the towns of the Thames valley are under strict orders from the conservators to discontinue draining into the river at a given time.

According to the *Avenir* of Auch, 130 tumuli, one of them containing a hundred skeletons, have just been discovered in the *landes* of Ossun.

Prof. R. S. Ball will commence a series of twenty lectures on Mechanics, in the Royal College of Science, Dublin, on the 7th of February. It is expected that this course will be found useful to artisans, as well as to students commencing the study of mechanics.

The Natural History Museum of the Royal Dublin Society has been open for the last few months on one evening in each week to the public. The success that has attended this experiment has been something quite unexpected. The artisan class have flocked into the Museum in such numbers as to inconveniently fill it ; the building, which can scarcely accommodate 2,000, being on at least one evening overcrowded with upwards of 3,000. In the meanwhile, the Department of Science and Art is greatly to blame in not increasing the number of porters, whose duty it is to regulate the movements of this great crowd as it circulates up staircases not four feet wide, and along the narrow slender galleries. If we except one porter, who acts as "turnstile" (counting the number of visitors), and another who takes the pennies for sticks and umbrellas, there are not *three* porters to do the duty of a dozen. The success of the experiment ought not to be endangered for the sake of a little expense.

On Saturday last M. Murez resumed his lectures at the College of France, on the mechanism involved in the flight of birds. His lectures of last year on this subject were published in the *Revue des Cours Scientifiques*.

Mr. Hull's paper read last week at the Royal Society has a value beyond that of recording the temperature of the strata through which the shaft of a coal-mine was sunk near Wigan. A mine 808 yards deep, nearly half-a-mile, is the deepest in the world, penetrates the "crust" of the globe farther than any other mine, and so has an especial interest for those who concern themselves about our supplies of coal. Geologists have told us that if we dig down through the "old red" we shall find coal-beds of greater extent than those which we have worked so profitably for the last two hundred years. This, however, did not comfort those uneasy people who looked forward to the exhaustion of coal ; for the "old red" is so thick, it would never pay to raise coal from such a depth ! And here the Rose Bridge Colliery, near Wigan, above referred to, becomes of especial importance. It may be regarded as an experiment towards a solution of the question of very deep mining. Already the proprietor finds that the cost of "getting" the coal is greater than when the mine was but 600 yards deep. This is the natural consequence of increase of temperature and increase of pressure. The temperature of the coal at the bottom of the mine, as stated in Mr. Hull's paper, is ninety-three degrees and a half ! How long will the timber props last in such a temperature and under such a pressure as they have to bear? If the mine yields a profit under such circumstances, then some enterprising coalowner may be tempted to go deeper.

We learn from the *Athenæum* that the sixth and concluding part of the first volume of annals of the Public Museum of Buenos Ayres has been issued, and that the work still bears the name of Dr. Burmeister as editor. The papers contained in this part, which is handsomely illustrated like its predecessors, are as follows :—"Descripcion de Cuatro Especies de Delfines de la Costa Argentina," and "Catologo de los Mamiferos Argentinos con los del Museo Público."

The election of the new Council (Commission Centrale) of the French Geographical Society took place on the 7th ultimo with the following result :—President, M. de Quartrefages ; Vice-Presidents, MM. d'Avezac and E. Cortumbert ; General Secretary, M. Mannoir ; Assistant Secretaries, MM. R. Cortambert and C. Delamarre.

SCIENTIFIC SERIALS

POLLI'S *Annali di Chimica applicati alla Medicina* (No. 1, 1870) has a long preface relating to *miasma palustre* and the use of febrifuges; these topics being discussed in view of the competition for which the Royal Institute of Lombardy has offered a prize in 1872. The competition is restricted to a discussion of the use of sulphites and hyposulphites in intermittent fevers. The editor adds to his preface a list of thirty-seven memoirs which have been published on these subjects between 1863 and 1869. Carlo Pavesi contributes a note on a speedy method of preparing mercurial ointment, in which the use of oil of turpentine as an ingredient is specially recommended. Belardi draws attention to the fact that pharmaceutical preparations of bismuth are liable to contain antimony. Pagano gives an illustration of the therapeutic value of magnesic sulphite; and Moretti records some clinical observations on the use of the same salt as well as sodic sulphite.

THE *Moniteur Scientifique* (January 15th) contains an unnecessarily tedious article on Sodic Bromide, by Casthélaz, which does not contain any original matter. M. E. Kopp contributes extracts from foreign journals (practical chemistry). M. J. Personne compares the process of Roussin for preparing hydrated chloral, which he condemns as imperfect, with that given in Dumas' *Traité de Chimie*, which he eulogises (it yields 185 per cent.) M. Jonglet reports ably on the progress of the sugar industry in France.

THE *Astronomische Nacrichten*, No. 1788, January 19, 1870, contains (1) Observations with the Reichenbach Circle at the Warsaw Observatory, by C. Deike, Second Assistant at the Observatory; (2) Observations of Comet III., 1869, by Argelander; (3) and (4) Elements and Ephemeris of the same Comet, by Bruhns and Von Littrow. Von Littrow states that the comet will hardly be visible after the end of January, as its brilliancy on 13th January only amounted to one-fifth the brilliancy at the time of its discovery. The fifth paper in the present number is by Peters, and gives Elements and Ephemeris of Felicitas (109) from January 30th to 22nd March. In the sixth and last paper Dr. Oppolzer communicates a definitive determination of the orbit of the planet (64) Angelina.

Annales de Chimie et de Physique, January.—M. Achille Cazin contributes a memoir on "internal work in gases." It contains a theoretical discussion and an experimental proof of such work, the latter being in principle a repetition of Joule's experiment, in which air is allowed to flow from a full into an exhausted receiver. M. Boussingault determines carbon in iron by mixing the filings with mercuric chloride and a little water, allowing the mixture to repose in contact with aqueous hydric chloride for about an hour, filtering and igniting the precipitate (carbon, mercurous chloride, &c.) in hydrogen. Successive ignitions in air and hydrogen then give the *combined* carbon; successive ignitions in oxygen and hydrogen next give the graphite. This number also contains an unfinished paper, by M. Vicaire, on the "temperature of flames and dissociation."

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 27.—The following papers were read: "Observations on the temperature of the strata taken during the sinking of the Rose Bridge Colliery, Wigan, Lancashire, 1868-69." By Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland. The manager of the Rose Bridge Colliery, Mr. Bryham, sensible of the value of observations of the temperature of the strata in what is probably the deepest colliery in the world, certainly in Britain, made a series of observations with as much care as the circumstances of the sinking of the shaft would admit, and entrusted them to Mr. Hull for publication. The mode of taking the observations was as follows:—On a favourable stratum, such as shale, or even coal, having been reached, a hole was drilled with water in the solid strata to a depth of one yard from the bottom of the pit. A thermometer was then inserted for the space of thirty minutes, the hole having been sealed and made air-tight with clay. At the expiration of the half-hour the thermometer was taken up and the reading noted. While the temperatures of the strata were being measured, observations were carried on *pari passu* on those of the open pit during the descent. These are given in the Table annexed. By a comparison of the results in the two columns, it will be observed that, as the depth increased, the differences between the corresponding temperatures in the pit and the strata tended to augment; in other words, the temperature of the strata was found to augment more rapidly than that of the open pit. The effects of the high temperature and pressure on the strata at the depth of 2,425 feet are making themselves felt, and cause an increase in the expense both of labour and timber for props. This colliery, in fact, will be in a position to put to the test our views and speculations on the effects of high temperature and pressure on mining operations. In order to obtain the average rate of increase of heat, as shown by the experiments at Rose Bridge Colliery, we may assume (in the absence of direct observation) the position and temperature of the *invariable stratum* to be 50 feet from the surface and 50° F., which is probably nearly the mean temperature of the place. With these data, the increase is 1° F. for every 54·57 feet, which approximates to that obtained by Professor Phillips at Monkwearmouth of 1° F. for about every 60 feet. If, on the other hand, for the purpose of comparison, the measurements for the *invariable stratum* as obtained at Dukenfield be adopted, the rate of increase is found to be 1° F. for every 47·2 feet as against 1° F. for every 83·2 feet in the case of Dukenfield itself. So great a discordance in the results is remarkable, and is not, in the opinion of the author, attributable to inaccuracy of observation in making the experiments. On the other hand, he suggests that it is due, at least in some measure, to dissimilarity in the position and inclination of the strata in each case.

THERMOMETRICAL OBSERVATIONS AT ROSE BRIDGE COLLIERY.

Date.	Depth, in yards.	Strata.	Temperature in open pit.	Temperature in solid strata.
			F. °	F. °
July 1854	161	Blue shale	...	64·5
August 1854 . . .	188	Warrant earth	...	66
May 1858	550	Blue shale	...	78
July 1858	600	Warrant earth	...	80
May 18, 1868 . . .	630	"Raven" coal	73	83
July 24, 1868 . . .	665	Linn and wool	75	85
April 29, 1869. . .	673	"Yard Coal" mine	76	86
November 18, 1868	700	Strong Blue Metal	76	87
February 22, 1869	736	Do.	76	88½
March 12, 1869. .	748	Shale	77	89
April 17, 1869 . .	762	Linn and wool, or strong shale	78	90·5
May 3, 1869 . . .	774	Strong shale	80	91·5
May 19, 1869. . .	782	Blue metal	79	92
July 8, 1869 . . .	801	Strong blue shale	79	93
July 16, 1869 . . .	808	Coal (Arley mine)	79	93½

Remarks.—All holes vertical in solid at bottom of pit drilled with water one yard deep, and thermometer remained thirty minutes in hole made air-tight with clay.

"On the Theory of Continuous Beams." By John Mortimer Heppel, Mem. Inst. C.E. Communicated by W. J. Macquorn Rankine, F.R.S. The chief object of this communication was to remedy some acknowledged defects in the theory of the above-mentioned subject. The principal steps by which it has reached its present state of development were also noticed, and may be briefly recapitulated as follows:—The great defect in the theory up to the present time has been that, in order to avoid an inextricable complexity, it has been necessary to consider the load in each span as uniformly distributed over it, and the moment of inertia of the section as uniform throughout each span. The method now given treats these conditions rigorously; and although the equations obtained are such as necessarily require some laborious computation to obtain numerical results, they are by no means inextricable.

"Remarks on Mr. Heppel's Theory of Continuous Beams." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S. The author states that the advantages possessed by Mr. Heppel's method will probably cause it to be used both in practice and in scientific study. With a view to the instruction of students in engineering science, he proposes an abridged way of stating the theoretical principles of Mr. Heppel's method, considering at the same time that Mr. Heppel's more detailed investigation forms the best model for numerical calculation. He then uses Mr. Heppel's improved form of the "Theorem of the Three Moments" to test the accuracy of the formulæ which he obtained in another way, and published in "A Manual of Civil Engineering," for the case of an uniform continuous beam with an indefinite number of equal spans, the successive spans being loaded alternately with an uniform fixed load only, and with an uniform

travelling load in addition to the fixed load; and he finds the results of the two methods to agree in every respect.

"Remarks on the recent eclipse of the sun as observed in the United States," by J. N. Lockyer, F.R.S.

By the kindness of Professors Winlock, Morton, and Newton, I have been favoured with photographs, and as yet unpublished accounts, of the results of the recent total eclipse of the sun observed in America. I am anxious, therefore, to take the opportunity afforded by the subject being under discussion, to lay a few remarks thus early before the Royal Society.

The points which I hoped might be more especially elucidated by this eclipse were as follows:—

1. Is it possible to differentiate between the chromosphere and the corona?

2. What is the real photographic evidence of the structure of the base of the chromosphere in reference to Mr. W. De la Rue's enlarged photographs of the eclipse of 1860?

3. What is the amount of the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere?

4. Is there any cooler hydrogen above the prominences?

5. Can the spectroscope settle the nature of the corona during eclipses?

With regard to 1, the evidence is conclusive. The chromosphere, including a "radiance," as it has been termed by Dr. Gould (the edge of the radiance as photographed being strangely like the edge of the chromosphere in places viewed with the open slit), is not to be confounded with the corona.

On this subject, in a letter to Professor Morton, Dr. B. A. Gould writes:—"An examination of the beautiful photographs made at Burlington and Ottumwa by the sections of your party in charge of Professors Mayer and Haines, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere.

"This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable. There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity; indeed the great protuberance, at 230° to 245°, seems to have formed a southern limit to the radiance on the western side, while a sharp northern limit is seen on all the photographs at about 350°, the intermediate are being thickly studded with protuberances which the moon displayed at the close of totality. The exquisite masses of flocculent light on the following limb are upon the two sides of that curious prominence at 93°, which at first resembled an ear of corn, as you have said, but which, in the later pictures, after it had been more occulted, and its southern branch thus rendered more conspicuous, was like a pair of antelope's horns, to which some observers compare it. Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were. The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

2. The second point is also referred to in the same letter. I think the American photographs afford evidence that certain appearances in parts of Mr. De la Rue's photographs, which represent the chromosphere as billowy on its under side, are really due to some action either of the moon's surface or of a possible rare lunar atmosphere, so that it is not desirable to confound these effects with others that might be due to a possible suspension of the chromosphere in transparent atmosphere, if only a *section* of the chromosphere were photographed.

Dr. Gould writes:—"You will observe that some of the brighter, petal-like flocculi of light have produced apparent indentations in the moon's limb at their base, like those at the bases of the protuberances. These indentations are evidently due to specular reflection from the moon's surface, as I stated to the American Association at Salem last month. Had any doubt existed in my mind previously, it would have been removed by an inspection of the photographs."

Where the chromosphere is so uniformly bright that the actinic effect on the plate is pretty nearly equal, the base of the chromosphere is absolutely continuous in the American photographs; but in the case of some of the larger prominences, notably those at + 146 (Young) and − 130 (Young), there are strong apparent indents on the moon's limb.

3. I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere.

This is considerable; in fact, the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—"I examined the principal protuberances before, during, and after totality. I saw three lines (C, near D and F) before and after totality, and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*"

This effect was observed with two flint prisms and seven inches aperture. Professor Young, with five prisms of 45° and four inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality.

He writes:—"I had just completed the measurements of 2,602, when the totality ended. *This line disappeared instantly*, but 2,796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness."

These observations I consider among the most important ones made during the eclipse; for they show most unmistakably that, as I have already reported to the secretary of the Government-Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion.

On the 4th point the evidence is negative only, and therefore in favour of the view I have some time ago communicated to the Royal Society.

5. We next come to the question of the corona, a question which has been made more difficult than ever, in appearance only, I think, by the American observations.

I propose to discuss only the spectroscopic observations of Professors Young and Pickering in connection with Dr. Gould's before-quoted remarks.

[After this discussion, for which we have not space, the author continues:—]

I have first to do with the continuous spectrum, deduced from Professor Pickering's observations.

I think in such a method of observation, even if the corona were terrestrial and gave a dark line spectrum, the lines visible with such a dim light would in great part be obliterated by the corresponding bright lines given out by the long arc of chromosphere visible, to say nothing of the prominences, in which it would be strange if C, D, E, *b*, F, and many other lines were not reversed. This suggestion, I think, is strengthened by the statement that two bright lines were seen "near C" and "near E;" should we not rather read (for the "near" shows that we are only dealing with approximations) C and F, which is exactly what we might expect?

But even this is not all that may be hazarded on the subject of the continuous spectrum, which was also seen by Prof. Young under different conditions.

Assuming the corona to be an atmospheric effect merely, as I have before asserted it to be, in part at least, it seems to me that its spectrum should be continuous, or nearly so; for is it not as much due to the light of the prominences as to the light of the photosphere, which it may be said roughly are complementary to each other?

With regard to the aurora theory, I gather from Prof. Young's note that, if not already withdrawn, he is anxious to wait till the next eclipse for further facts. I consider the fact that I often see the line at 1,474, and often do not, is fatal to it, as it should be constantly visible on the proposed hypothesis. The observation of iron-vapour, as I hold it to be at this elevation, is of extreme value coupled with its simple spectrum, *seen during an eclipse*, as it entirely confirms my observations made at a lower level in the case, not only of iron but of magnesium.

Geological Society, January 26.—Professor Huxley, LL.D., F.R.S., president, in the chair. Thomas Daniel Bott, Esq., 20, Osborne Villas, Talfourd Road, Peckham; Edwin Buckland Kemp-Welch, Esq., 3, Beaumont Terrace, Bournemouth; James Parkinson, Esq., F.C.S., Sarum House, Church Road, Upper Norwood, S.; Henry Sewell, Esq., Villa del Valle, Mexico; and Thomas F. W. Walker, Esq., M.A., F.R.G.S., Athenæum Club, London, and 6, Brock Street, Bath, were elected Fellows of the society. The Rev. Dr. Oswald Heer, of Zurich, was elected a foreign member of the society. The following communication was read:—"On the crag of Norfolk and associated beds." By Joseph Prestwich, Esq., F.R.S., F.G.S. The author commenced by referring to his last paper, in which he divided the Red Crag into two divisions—a lower one, of variable oblique bedded strata, and an upper one of sands passing up into the clay known as the Chillesford clay. In 1849 he had alluded to the possibility of this clay being synchronous with the Norwich Crag. He has since traced this upper or Chillesford division of the Red Crag northwards, with a view to determine its relation to the Norwich Crag. He has found it at various places inland, but the best exhibition of it occurs in the Easton Bavant Cliffs. He there found in it a group of shells similar to those at Chillesford, and under it the well-known bed of mammaliferous or Norwich Crag, with the usual shells. The author also showed that in this cliff and the one nearer Lowestoft traces of the Forest-bed clearly set in upon the Chillesford clay. He next traced these beds at the base of Horton Cliff, and then passed on to the well-known cliffs of Happisburgh and Mundesley. He considered the Chillesford clay to pass beneath the Elephant bed, and to represent some part of the Forest-bed. The same clay may be traced to near Weybourne. The crag under these beds he referred to the Chillesford sands. Mention was then made of the sands and shingle above the Chillesford, to which the author proposed the names of "Southwold Sands and Shingle." These usually are very unfossiliferous, but at two or three places near Southwold the author found indications of an abundance of shells (*Mytilus*, &c.) and Foraminifera in some iron sandstones intercalated in this series. In the Norfolk cliffs these beds contain alternating seams of marine and freshwater shells. The inland range of the beds to Aldeby, Norwich, and Coltishall was next traced, and the Chillesford clay shown to be present in each section, and the sands beneath to be referable to the Chillesford sands, as already shown by other geologists on the evidence of the organic remains. Mr. Gwyn Jeffreys, who had carefully examined the shells of the Norwich Crag for the author, stated that a considerable number of Arctic species were found in the Norfolk Crag which did not occur in Suffolk. While, therefore, the Norwich Crag seems to be synchronous with a portion of the Suffolk Crag, that portion is the upper division, and, therefore, the triple arrangement proposed by Mr. Charlesworth and advocated by Sir C. Lyell, together with the fact of the setting in of a gradually more severe climate, pointed out by the late Dr. Woodward and by Sir C. Lyell, are confirmed. Mr. Prestwich then referred to the origin of the materials of the Southwold shingle, and showed that, with few exceptions, they came from the south. In it he had found a considerable number of worn fragments of chert and ragstone from the Lower Greensand of Kent. He considered this a convenient base-line for the Quaternary period; as then commenced the spread of the marine gravels over the south of England, and soon after commenced the great denudations which give the great features to the country. Mr. Gwyn Jeffreys observed that no littoral shells occur in the Coralline Crag, while in the Red Crag they abound. In the Norwich Crag there is also evidence of littoral conditions, but in certain places the shells exhibit a deep-water character. In the Norwich Crag, after eliminating as derivative or extraneous certain species (as had already been done by the late Dr. Woodward), he finds, exclusive of varieties, 140 species, of which 123 are living, and 17 are supposed to be extinct. Of these 123, 101 still live in the British Seas, 12 are Arctic and North American, 8 Mediterranean, and 2 Asiatic. The southern species were probably derived from the Coralline Crag. The two Asiatic species were the *Corbicula fluminalis* and *Paludina unicolor*. Twenty species in the Norwich Crag have not been found in the Red or Coralline Crag, and he therefore thought there was some difference in their geological age, the Norwich Crag being both more recent than the Red Crag, and its shells of an Arctic or more northern kind. *Tellina balthica* he regarded as significant of brackish water conditions. *Actæon Noæ*, a characteristic shell of the Red and Norwich Crag, had been found fossil by Prof. Steenstrup in Iceland. Sir Charles Lyell had been struck with the similarity of the beds at Chillesford and at Aldeby, in which also the shells, though 40 in one case and 70 in the other, were very similar in character; but in neither was *Tellina balthica* found, though common in the glacial beds. He called attention to the condition of the shells as they occurred at Aldeby, and suggested that where the two shells of a bivalve were found in contact, they would probably afford some evidence whether they were derivative or no. Mr. Searles V. Wood, jun., was inclined to differ to a large extent from the author, especially with regard to the beds above the Chillesford clay. The sands containing *Tellina solidula* he placed as the lowest member of the glacial series; the fauna they contain is different from that of the Chillesford bed. He regarded the sand-beds at Kessingland as above the lower boulder-clay and contorted drift of Cromer, and considered that it might be traced as occupying this position along a great part of the coast of Norfolk. He had, in company with Mr. Harmer, surveyed a great part of the Norfolk and Suffolk district, and they intended to place their maps and sections at the disposal of the Geological Society and the Survey. He recommended that any examination of the country should commence from the east rather than from the west. Mr. Boyd Dawkins, speaking of the fossil mammalia of the crag, mentioned that, at the base of the crag at Horstead, immediately on the chalk, was a bed exhibiting an old land-surface, and in this were found the principal perfect mammalian remains, whereas in the crag above they were water-worn. But though these bones occurred in the marine deposit, the animals had lived on the land, and there was no evidence but that they belonged to a much earlier period than that at which it was submerged. He thought that the facies of the Cervidæ found at Horstead was that of an early Pliocene age. The mammals of the London Clay had in some cases become confounded with those of the Suffolk Crag, but these he regarded also as belonging to an old Pliocene land-surface. He differed from the author in not regarding the Forest-bed as Quaternary, as the remains of *Rhinoceros etruscus*, *Ursus arvernensis*, and *Elephas meridionalis*, &c., had occurred in it, in many cases in fine condition. He could see no reason for splitting up the Cainozoic series into four divisions, as there was no break in the life between the Tertiary and Quarternary periods. Though there might be a break in England, the forms of life were continuous from the Miocene of Pikermi on the Continent. The President suggested that if we were to admit a Quaternary period we must go back to the Miocene, as the mammalian fauna of that period was the direct ancestor of that of the present day. Mr. Prestwich, in reply, remarked that he did not quite agree with Mr. Jeffreys as to the number of derivative species in the different members of the Crag. The fauna, however, required further investigation. With regard to the objections of Mr. Wood, he had not on this occasion intended going into details as to the beds above the Chillesford clays; his object had rather been to show that these latter extended over a large area, and contained in other places than Chillesford the same shells as those occurring there. He did not attach the same value to the presence of *Tellina balthica* as did Mr. Wood, it being a shell now living and found on the coast. He had not overlooked the importance of the mammalian remains, but, like Mr. Dawkins, he had felt the uncertainty which, in the case of the Crag, so often attached to their origin, and therefore had not much insisted on them. He thought the divisions of Miocene and Pliocene were well known and generally accepted; and though the division was arbitrary, he thought the setting in of the Glacial period a good epoch at which to commence the Quaternary period. If we were to go back to some break in the forms of life, we might go back indefinitely.

Ethnological Society, January 25.—Professor Huxley, president, in the chair. The following new members were announced at this and the preceding meeting:—The Earl of Dunraven and Mount-earl, K.P., Lord Rosehill; Messrs. J. W. Barnes, T. H. Baylis, D. Duncan, M.A., J. E. Edwards, J. F. McLennan, W. Morrison, M.P., and R. L. Nash. Dr. Hooker, C.B., exhibited a collection of figures in unbaked clay, modelled by a native Zulu; and Colonel Lane Fox exhibited some stone mullers used for pounding grain.—Mr. Borwick, F.R.G.S., read a paper on the origin of the Tasmanians geologically considered. The Tasmanians have now become almost extinct, an old woman being the only survivor of the race. They were related in manners and in general *physique* to

the neighbouring Australians, but were allied by black skin and woolly hair to the distant Africans, while they were assimilated by resemblance of language, customs, and habits of thought to many races scattered over vast areas. The author sought to explain this relation by constructing an ideal southern continent, whence all the dark-coloured races surrounding the Indian Ocean, and extending into the Pacific and Southern Oceans, may have radiated. He regards the Tasmanian as probably older than the Australian. Dr. Hooker, whose authority had frequently been quoted in the paper, pointed out the similarity and differences that obtain between the floras of Australia, Tasmania, New Zealand, South Africa, &c. It has recently been found that the flora of the Howe Islands is very unlike that of Australia, although so near to the coast. He protested, however, against the inference that the line of migration followed by plants is necessarily the same as that pursued by the higher animals. The President alluded to the great difference between the Australian and Tasmanian, especially in the character of the hair, and he regarded it as physically impossible that the Tasmanian could have come from Australia. He suggested that an interrupted communication, by a chain of islands, may have extended from New Caledonia to Tasmania, similar to that which now connects New Caledonia with New Guinea; and that by this means a low negrito type may have spread eastwards over this area.—Mr. Howorth's paper, "On a frontier-line of Ethnology and Geology," was then read. Siberia and North America form a well-defined province, botanically, zoologically, and ethnologically. North of the isothermal line forming the southern boundary of this province dwell the Ugrian races, whose conditions of existence were compared by the author with those of the prehistoric period. In Europe the isothermals have been gradually twisted to the north by the Gulf Stream; and the author believes that the gradual advent of the stream may be traced from no earlier period than about the twelfth century B.C.; Remarks on this paper were made by the President, Dr. Hyde Clarke, Dr. Richard King, and Colonel Lane Fox.—Mr. Atkinson read a "Note on the Nicobar Islands," and exhibited some grotesque figures carved in wood, taken from the Nicobars by Capt. Edge in 1867, and recently brought to this country by Capt. Mackenzie. These figures are to be placed in the Christy collection, and similar objects have been forwarded to the Museum in Edinburgh.

London Mathematical Society, January 13.—Prof. Cayley, president, in the chair. Mr. Walker gave an account of a paper "On the Equations of Centres and Foci of, and Conditions for, certain Involutions." In this communication it is shown that the three points corresponding to one having an assigned distance (x') from the origin, in one of three involutions determined by a quartic, and found from the equation—

$(abcd\,\aleph\,x', 1)^3$, $(bcde\,\aleph\,x', 1)^3$, $(cdef\,\aleph\,x', 1)^3$, $(defg\,\aleph\,x', 1)^3$, $(x, 1)^3 = 0$,

the sextic covariant of the quartic being written $(a\,b\,c\,d\,e\,f\,g\,\aleph\,x\,y)^6$, This form has been arrived at in carrying out a suggestion of the president, Prof. Cayley. It contains the equations giving the three centres of the involutions determined by the quartic, as well as that giving the six double points (the geometrical significance of which latter equation has been already pointed out by Dr. Salmon), by making x' infinitely great, and equal to x, successively. The case of the quartic breaking up into two quadratics is next discussed, when the roots of one correspond respectively to those of the other. The condition for two cubics $(u\,v)$ determining an involution, of such a kind that the three roots of one correspond each to a root of the other, is investigated. This has (since the meeting) been identified by the author with $\Delta\,\Theta'^2 - \Delta'\,\Theta^2 = 0$, Δ, Δ', being discriminants of u and v, while $\Theta\,\Theta'$ are the co-efficients of λ and λ^3 respectively in the discriminant of $u + \lambda v$. The equations for determining the centre and double points—or, more generally, that determining the point corresponding to an assigned one—in the involution determined by two cubics, satisfying the above condition, are also investigated. Dr. Henrici, Prof. Hirst, Mr. Clifford, and the President took part in a discussion on the paper. The President then made a statement of some results he had arrived at with reference to quartic surfaces. Mr. Roberts exhibited and explained diagrams of the pedals of conic sections which he had constructed by the methods described in his paper read before the society, January 14th, 1869.

Quekett Microscopical Club, January 28.—P. Le Neve Foster, Esq., president, in the chair.—A paper was read by Dr. Robert Braithwaite on the geographical distribution of mosses. The paper gave an abstract of the arrangement proposed by Prof. Schimper, who divides the whole of Europe into three areas in latitude—viz. (1), a northern zone, comprising N. Russia, Scandinavia, and N. Scotland; (2), a middle zone, extending south of this to the foot of the Alps, and including all central Europe; (3), a southern zone, reaching from the last to the Mediterranean. More important than these is their distribution in altitude, or height above the sea level, and for this five regions are indicated, each characterised by certain predominant species, and marked out by lines gradually falling in altitude as we pass northward to the pole, where the extreme limits, or that of the sea level and the line of perpetual snow, become coincident. Commencing at the sea level, the dimensions in altitude are—(1), the Campestral region, or that of the cultivated field and fruit trees, embracing the greatest varieties of soil and conditions of surface; (2), the Montane, or lower mountain region, marked at its upper limit by the disappearance of the beech; (3), the Sub-Alpine region, extending from the upper limit of the beech to the upper limit of the spruce fir; (4), the Alpine region, embraced between the commencement and termination of growth of the dwarf pine, and marked by the presence of dwarf birch; (5), the Supra-Alpine region, reaching above the limit of the trees named to the line of perpetual snow. A brief sketch was then given of the various *habitats* affected by mosses, and lists of the characteristic species given; these embraced the dwellers on walls, roofs, trees, heaths, bogs, and rocks. They were illustrated by a fine series of specimens of mosses and their associated flowering plants, prepared by the late Mr. N. B. Ward, who thus ingeniously attempted to work out the idea of representing the whole flora of a locality at one glance. Conspicuous among these were the sheets from Ben Lawers, Ben Voirlich, Heidelberg, Killarney, and Eridge Rocks.

Anthropological Society, January 18.—Annual General Meeting.—John Beddoe, Esq., M.D., president, in the chair. The report of the auditors showed the income of the society in 1869 to have been 1,091*l.* 9*s.* 5*d.*, the expenditure 964*l.* 9*s.* 8*d.*, and the balance in hand on the 31st December 126*l.* 19*s.* 9*d.* The report of the council was read and adopted. The president then delivered the annual address, including a full obituary notice of Dr. James Hunt, founder of the society. The ballot for the election of officers and council to serve in 1870 was taken with the following result:—President, John Beddoe, M.D.; vice-presidents, H. Beigel, M.D., Captain R. F. Burton, Dr. Charnock, J. Barnard Davis, M.D., F.R.S., Captain Bedford Pim, R.N., Dr. Berthold Seemann; director, Thos. Bendyshe, M.A.; treasurer, Rev. Dunbar I. Heath, M.A.; council, J. Gould Avery, J. Burford Carlill, M.D., S. E. Collingwood, Walter C. Dendy, George Harris, Jonathan Hutchinson, W. B. Kesteven, Kelburne King, M.D., Richard King, M.D., A. L. Lewis, St. George J. Mivart, F.R.S., Major S. R. I. Owen, Edward Peacock, F.S.A., J. Spence Ramskill, M.D., C. Robert Des Ruffières, John Thurnam, M.D., W. S. W. Vaux, F.R.S., C. Staniland Wake, Alfred Wiltshire, M.D., E. Villin.

Entomological Society, January 24.—Annual General Meeting, Mr. Frederick Smith, vice-president, in the chair. The Report of the Council for 1869, and an address by Mr. H. W. Bates, the retiring president, were read. The following thirteen members were chosen to form the council for 1870, viz: Messrs. Bates, Dallas, Dunning, Fry, Grut, M'Lachlan, Parry, Pascoe, Saunders, Stevens, Wallace, and Wormald. Mr. Alfred Russell Wallace was elected president for 1870; and the following officers were re-elected:—Mr. Stevens, treasurer; Messrs. Dunning and M'Lachlan, secretaries; Mr. Janson, librarian. The thanks of the society were voted to the officers and members of the council for their services during the previous year.

GLASGOW

Philosophical Society of Glasgow, January 12.—Dr. Francis H. Thomson, vice-president, in the chair. The president, Dr. James Bryce, F.G.S., gave an account of the "Geological Structure of Skye and the West Highlands." The difficulty and danger of thoroughly studying the peculiar geological structure of Skye were so great that no geologist, in the author's opinion, should undertake it without the company of an associate; and as he considered that no such person should ascend the mountains for the first time unaccompanied, the author specially recommended Angus Nicholson as a reliable guide. Dr. Bryce dwelt at some length upon the wonderful peculiarities in the structure of the Cuchullin mountains, which

form such a characteristic feature of the island in the vicinity of Glen Sligachan. They rise to a height of about 3,300 feet, the last 400 feet being absolutely inaccessible, owing to the peaked, spiry, or pinnacled structure. Thoroughly skilled Alpine climbers had been quite baffled by the Cuchullin mountains, unless they had gone to the ignominious extremity of using ladders to aid them in making the ascent. That structure was entirely due to the great abundance of one single mineral, a variety of augite known as *hypersthene*, the intense hardness of which was owing to the presence of large quantities of oxide of iron and oxide of manganese (*query*, silicate of iron and silicate of *magnesia?*). While referring to the rock-features of Skye, Dr. Bryce remarked that of late geologists have been too much led away by the attractiveness of palæontology as a means of geological research, overlooking the superior claims of mineralogy. He considered that in Skye there were two chains of mountains—one black, in which the hypersthene prevails, and the other red, owing to the presence of syenite—and that they had come up in two, if not even in four eruptions, through a liassic basin, the thickness of which is probably not less than 1,500 feet. Besides the lias, he found gneiss, sandstone, quartzite, marble, numerous whin dykes, and crystalline slates from 10,000 to 20,000 feet in depth. When the country was examined closely two syenites were found, one intrusive, the other disruptive. The granites and syenites of Skye were very closely approximated to each other, each rock shading off into the other. On the coasts there were beds of oölite and traps, but they were difficult of examination, and indeed there were many points in the geology of the island yet to be explored. Dr. Bryce referred to the separate examinations of Skye made by Sir Roderick Murchison and the late Prof. Edward Forbes, but he differed in opinion from some of the conclusions of those geologists on the geology of the West Coast, and he expressed himself inclined to support the crystalline schist view of Prof. Nicol, of Aberdeen, rather than the Silurian theory which Sir Roderick Murchison and Mr. Geikie had promulgated, although the latter was the popular theory. Dr. Young said he could not agree with Dr. Bryce in his remarks regarding the relative claims of mineralogy and palæontology in geological inquiries. He dissented from some of the conclusions arrived at by Dr. Bryce, and on other points he confessed and regretted his inability to understand him. Dr. Bryce briefly replied.

Chemical Section, January 17.—Alexander Whitelaw, Esq., vice-president, in the chair. Two papers were read,—the first by Mr. J. Wallace Young, on "Artificial Alizarine," recently obtained from anthracene, one of the coal-tar products. In reference to the question of price, a member having much experience, said that manufacturers felt quite satisfied that they could supply artificial alizarine in large quantities, in half tons if it was wanted, and at a price much under that of natural alizarine as extracted from madder.—The other paper read was, "On the estimation of iodine and bromine in the mother liquors from saltpetre and in kelp." By Dr. John Clark.

BERLIN

German Chemical Society, January 24.—F. Rüdorff showed a simple experiment to prove the expansion of water when freezing. Cylindrical bombs cast in iron, of half an inch in thickness, and measuring three inches in length, and one in diameter, were entirely filled with water, previously freed from air by boiling, and then closed by a tightly fitting screw. They were then covered by a mixture of pounded ice and common salt. In from ten to twenty minutes' time they exploded with a loud report like that of a pistol, breaking in different directions.—Mr. Holbein exhibited animals, from mussels up to small mammalia, which had been preserved in a solution of creosote in water (one of creosote to twenty parts of water). This process appeared particularly suited to the preservation of fishes.—A. Baeyer spoke on a third isomeric form of hydromellithic acid, adding interesting considerations on the transformation of isomeric bodies one into the other, which will not bear abbreviation.—A. Horstmann reported on the vapour density of acetic acid, which he found to be normal at low temperatures, when the acid vapour was diluted with air.—Prof. Lieben (of Turin) sent in a paper published conjointly with Rossi on normal butylic alcohol produced by the reduction of butyric acid.—Prof. Rose reported on the first diamond found in Europe. A small diamond, recognised as such by Schafarik in Prague, has lately been discovered in an alluvial formation in Bohemia, in which garnets, hyacinths, and sapphires have been found for years.

PRAGUE

National Museum of Bohemia, December 18, 1869.—Prof. Krejci continued his account of the Permian formation of the N.E. of Bohemia, and discussed the section of Stepanitz, near Paka, at Bélohrad. Three deposits are there distinguished. The lowest is schistose and rich in fossil fish; the next is composed of glauconitic grit, and contains trunks of auracaria *in situ*; the upper is calcareous, finely laminated, full of beautiful concretions of polished stones, of chalcedony and agate, often arranged in rows parallel to the stratification. This is the original deposit of the celebrated Psaronites (silicified trunks of *Marattiaceæ arborescentes*. M. Krejci calls this stratum the Kalva beds. This simple arrangement is disturbed by numerous faults and discolorations, united apparently by melaphyr. M. Krejci believes that all the numerous portions of melaphyr in this neighbourhood exhibited originally only a single level; he insists on the fact that in the sections frequently exposed of late in the course of railway operations; the melaphyr is intercalated conformably between the sedimentary deposits; thus leading to the supposition of a metamorphic change of schist or phyllite into melaphyr. He allows, moreover, that the mountain, Grand Levin, near Horka (Falgendorf) arose apparently through a cleft only one to two metres wide. As regards the band of melaphyr and red porphyry, which, at the southern foot of the superb basaltic mass of Mount Kozakov, rests against the very sloping beds of the cretaceous series, and extends towards Mount Jeschken for a distance of at least ten kilometres, M. Krejci prefers to the idea of an irruption, that of a fault into which the beds of porphyry and melaphyr have slid on the one side to reappear on the other.

January 17.—M. Schafarik announced the discovery of the diamond in Bohemia. A note on the subject will be found in another page. We may add here that the Dlaschkowitz diamond has been presented by Count Schönborn to the National Museum of Prague.—The Royal Society of Bohemia held no public meetings during the month of December.

BOSTON

Society of Natural History, November 17.—The president in the chair. Prof. N. S. Shaler read a note on the occurrence of the remains of *Tarandus rangifer* Gray, at Big Bone Lick, in Kentucky. At a previous meeting was presented the evidence in support of the conclusion that one of the large mammals of North America, the buffalo, had recently changed its limits, and had only ranged in the Ohio valley within the past few centuries. The same locality supplies us with evidence that the caribou existed in abundance in this river basin at a time anterior to the coming of the buffalo, and probably not very long after the disappearance of the *Elephas primigenius*. Since the coming of civilised man into America, the caribou has been confined to a narrow area in the north-east part of the continent; it is questionable whether it has ever ranged during this time south of the southern limit of the State of Maine. The position in which these remains were found leaves the precise relationship in time of this species to the mammoths and mastodons a little questionable. There is, however, little doubt in my mind that, if not in existence during the later part of the time of these pachyderms, it came immediately after them. Its bones are always found below the line of the buffalo and the Virginia deer. The remains of this latter species are found only among the most recent deposits of the swamp. The disappearance from this region of this eminently boreal animal immediately after the passing away of the ancient elephants from the Mississippi valley, goes to confirm the conclusion that the climatic change which closed the period of the mammoths was from cold to warmth, and not an alteration of the opposite character.

Section of Entomology, November 24.—Mr. Edward Burgess in the chair. The following paper was presented:—"American Lepidoptera. II. *Phalænidæ* Latr." by Charles S. Minot. At the June meeting of the section, I presented a paper entitled "American Lepidoptera, No. I." which contained descriptions of four new Geometridæ (*Phalænidæ* Latr.), and was published without further introduction. I should now like to say a few words on the intended purport of the series. I propose that it shall contain any papers of a miscellaneous nature which may aid in completing our knowledge of the natural history of the Lepidoptera; such as descriptions of new species, or of the metamorphoses and lists of insects found in particular localities or States, with their times of appearance, and perhaps ultimately anatomical communications. The descriptions of new species will, for

the present, be principally, if not entirely, confined to the Phalænidæ. Mr. W. H. Dall remarked that while passing over the Portage to the Yukon River, in Alaska, when the temperature was below zero of Fahrenheit, he shot a Canada jay, which had in its mouth the caterpillar of an Arctian; afterwards, when the thermometer was sixteen degrees below zero, he found one of the same caterpillars crawling upon the snow. On the middle of the frozen river, whenever the sun shone for a short time upon the crust, he saw upon the snow a species of *Lepisma* or *Podura* in great abundance, although the cold was intense. The caterpillar of *Vanessa Antiopa* was twice noticed alive during the winter, and the perfect insect was seen at Nulato, May 20th, when the nightly temperature was below freezing.

December 3.—The president in the chair. Dr. C. T. Jackson presented, in the name of Mr. Daniel McCain, specimens of native carbonate of magnesia from Greece, California, Maryland, and Kansas. These minerals are used by the Union Stone Company in making calcined magnesia, which is one of the ingredients of their artificial stone, serving, when combined with chloride of magnesium, as the binding material. Dr. Jackson gave a detailed account of the method of making the artificial stones, and of casting bas-reliefs, busts, and ornamental mouldings. He said the processes had been so improved that now artificial grindstones made of quartz-sand and of emery had been constructed, which were as solid and durable as any natural stone. The emery wheels made of these materials are vastly better than those made with a paste of vulcanised indiarubber, since they do not glaze, but wear away in such a manner as to always expose fresh particles of emery. He regarded this new manufacture as of great value for architecture and the mechanic arts, and as showing the importance of the mineral native carbonate of magnesia, which had been before used only for the manufacture of Epsom salts, of which a limited supply only is wanted. Professor N. S. Shaler offered some remarks on the relation of the rocks in the vicinity of Boston.

PHILADELPHIA

American Philosophical Society, January 7.—Mr. Pliny E. Chase made some observations on the comparison of different mechanical equivalents. He stated that recent determinations, by the different methods of Thomson and Farmer, fix the mechanical equivalent of light, in a wax candle burning 126½ grains per hour, at 13·1 foot-pounds per minute, the equivalent of one grain being 6·213 foot-pounds. According to Dubourg, the heat evolved during the combustion of one grain of olive oil in oxygen is sufficient to heat 9682 grains of water 1° C. According to Favre and Silbermann, one grain of oil of turpentine, burnt in oxygen, would heat 10,852 grains of water 1° C. It may therefore be presumed that the total heat given out by the combustion of one grain of wax is about sufficient to raise 10,000 grains of water 1° C., or 18,000 grains 1° F. This represents a mechanical equivalence of (18,000 × 772 ÷ 7,000 =) 1985·143 foot-pounds, which is 319·5 times as great as the corresponding equivalent of the light given out during the combustion. Tyndall, in his lecture on Radiation, states that the visible rays of the electric light contain about one-tenth of the total radiated heat. The relative luminous intensity of an electric lamp would, therefore, appear to be about 31·95 times as great as that of a wax candle. This ratio resembles that of solar to terrestrial superficial attraction, and the connection of electric and magnetic currents with solar radiation is so evident, that additional experiments, to furnish materials for a great variety of similar comparisons, seem desirable. While it is possible that the resemblance in the present instance may be accidental, the numerous harmonies which exist between the manifestations of cosmical and molecular forces render it at least equally possible that it may have a weighty significance.

Academy of Natural Sciences, August 3, 1869.—Professor Cope called attention to a thin slab of shale containing foot impressions of vertebrate animals found some time ago by Professor Gabb, from the subcarboniferous slate in Schuylkill County. The position of the slab was about 300 feet above the conglomerate. The impressions found by Dr. Isaac Lea some years back were from a position about 700 feet below the conglomerate, and, therefore, more ancient. Professor Cope thought that the impressions found by Dr. Lea were neither reptiles nor fishes, but air-breathing vertebrates—*Batrachians*. But these tracks were different from most *Batrachia*, showing slender digits and long tarsus. The fore-feet are smaller than the hind. They are probably referable to a Salamandroid animal.

DIARY

THURSDAY, FEBRUARY 3.

ROYAL SOCIETY, at 8.30.—On the Fossil Mammals of Australia. Part III. Diprotodon Australis Ow. : Prof. Owen, F.R.S.—Note on an Extension of the Comparison of Magnetic Disturbances with Magnetic Effects, inferred from Observed Terrestrial Galvanic Currents, and Discussion of the Magnetic Effects Inferred from Galvanic Currents on Days of Tranquil Magnetism : The Astronomer Royal, F.R.S.

LINNEAN SOCIETY, at 8.—Revision of the genera and species of capsular gamophyllous *Liliaceæ* : J. G. Baker, F.L.S.—On a new form of Cephalopodous Ova : Dr. Collingwood, F.L.S.

ANTIQUARIES, at 8.30.—On some Ancient Oaken Coffins discovered in Northumberland : T. W. Snagge, Esq.

CHEMICAL SOCIETY, at 8.

FRIDAY, FEBRUARY 4.

PHILOLOGICAL SOCIETY, at 8.15.

ROYAL INSTITUTION, at 8.—Verona and its Rivers : Prof. Ruskin.

ARCHÆOLOGICAL INSTITUTE, at 8.

GEOLOGISTS' ASSOCIATION, at 8.

MONDAY, FEBRUARY 7

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

ENTOMOLOGICAL SOCIETY, at 7.

MEDICAL SOCIETY, at 8.

LONDON INSTITUTION, at 4.

TUESDAY, FEBRUARY 8.

ROYAL INSTITUTION, at 3.—On the Architecture of the Human Body : Prof. Humphry.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY, at 8.30.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Discussion upon Mr. Harrison's Paper "On Railway Statistics and Expenditure."

PHOTOGRAPHICAL SOCIETY, at 8.—Anniversary Meeting.

ETHNOLOGICAL SOCIETY, at 8.—On the discovery of Flint Flakes under a Submerged Forest in West Somerset : W. Boyd Dawkins, F.R.S.—On Remains of Pre-historic man in the neighbourhood of the Crinan Canal, Argyleshire : Rev. R. J. Mapleton.

ROYAL MICROSCOPICAL SOCIETY, at 8.—Anniversary Meeting.

ARCHÆOLOGICAL ASSOCIATION, at 8.

WEDNESDAY, FEBRUARY 9.

GEOLOGICAL SOCIETY, at 8.—The Fossil Corals of the South-Australian Tertiaries. Prof. P. Martin Duncan, F.R.S., Sec. G.S.—Note on a very large undescribed Wealden Vertebra : J. W. Hulke, F.R.S.—Additional Observations on the Neocomian Strata of Yorkshire and Lincolnshire, with Notes on their Relations to the Beds of the same Age throughout Northern Europe : J. W. Judd.

SOCIETY OF ARTS, at 8.—On Loss of Life at Sea : Mr. J. W. Wood.

THURSDAY, FEBRUARY 10.

MATHEMATICAL SOCIETY, at 8.—Quartic Surfaces : Prof. Cayley.

ZOOLOGICAL SOCIETY, at 8.30.—On a new Cervine Animal from the Yangtze-Kiang : R. Swinhoe.—On the Size of the Red Corpuscles of the Blood of *Moschus*, *Tragulus*, *Orycteropus*, *Ailurus*, and some other mammalia, with historical notices : G. Gulliver.

BOOKS RECEIVED

ENGLISH.—The Body and its Health : E. D. Mapother (Falconer, Dublin).—Our Domestic Fireplaces : F. Edwards (Longmans).—Handbook of Ferns : K. M. Lyell (Murray).—Transactions of the New Zealand Institute, 1868 (Trübner).

FOREIGN.—29 Monographie du Genre Ostrea Terrain Cretage Atlas : Coquand (Williams and Norgate).—Fortschritte der Physik im Jahre, 1866 : Quincke Schwalbe and Wangern (Williams and Norgate).—Anthropolgie der Natur völker T. Waik (Williams and Norgate).—Physiologie des Menschen : E. Larisch (Williams and Norgate).—Einleitung in die Physik : Karsten, Harms, and Weyer (Williams and Norgate).—Histoire Generale de Paris. Planches : E. Belegrand.—Gebirgsschichten aus mikroskop. Bacillarien unter und bei der Stadt Mexiko : C. G. Ehrenberg.—Ueber die Macula Lutea des Menschen, &c : Fr. Mukel.—Anatomisch. System. Beschreibung der Alcyonarien : A. Kölliker.—Recherches sur la Faune de Madagascar : P. L. Polle, D. C. Van Dan.—Des Mollusques fossiles de la craie de Lemberg : E. Favre.—Recherches sur les Animaux Vertebr. vivant et fossiles : P. Gervais.

CONTENTS

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid ground*
Of Nature trusts the mind that builds for aye."—WORDSWORTH

CONTENTS

No. 10

No. 11

No. 12

No. 13

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