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THURSDAY, JANUARY 27, 1870

DUST AND DISEASE

PROFESSOR TYNDALL'S lecture last Friday night at the Royal Institution, which we give *in extenso* in another column, has excited unusual interest, not only on account of the Professor's beautiful demonstration of the presence of organic dust in our London atmosphere, but from the manner in which he has sought to connect this presence with certain theories of disease held by part of his audience.

The revelations made by an ordinary sunbeam passing through a hole in the shutter are familiar enough; and the changes produced on the beam by a candle flame or a red-hot poker (an experiment used long ago by Dr. Wollaston for another purpose) will show in a rough manner the nature of the investigations.

Their practical result may be thus stated: London air contains a large amount of organic particles powerfully reflecting a light thrown upon them; and these particles cease to reflect light when the air containing them is submitted to the action of a high temperature, or when it is passed through a strainer or filter. Some of the appearances produced by certain modifications of the experiments are striking. Such, for example, as the disappearance of reflected light when air of a temperature below the incandescent point is made to rise through the beam showing the dust; or when certain gases are used instead of warmed air. In these cases the reflection of light from the dust particles disappears, and blackness takes its place. We are not, however, convinced that the Professor's explanation of this striking phenomenon is quite tenable. It presupposes a rapidity in the gas and air currents greater than can be followed by the more sluggish dust particles; so that these are left behind, or thrown to one side, and the rarefied air or gas deprived of dust particles enters the beam, and becomes invisible from absence of reflecting particles.

One would suppose that, sooner or later, the particles must follow the air in which they float, unless the heated air or gas become so much lighter than the particles that the latter will tend to fall downwards. This point, however, is one which admits of further demonstration.

The microscope, on its side, has not been behindhand in the same field, and has told us something more about this organic air dust. It is found to vary in character according to the objects from which it proceeds, and according to the degree of ventilation in an apartment. A microscopist, with his air analyser, would very likely have told Professor Tyndall's audience how they were breathing fragments of each other's clothes, and the scurf skin of each other's hands and faces, besides other matters brought into the Institution by the listeners, or wafted in through the windows; and if a whiff of sewer air had entered the room, living *vibriones* would probably have been among the subjects of the microscopist's demonstration. Chemistry also has been at work in the same direction, and by means at her disposal she has been able to estimate approximately the amount of organic matter in air; and this application of chemical methods is now in common use for determining the state of ventilation in inhabited buildings, as well as the comparative purity of the air in town and country. We are glad that Prof. Tyndall has enlisted optical analysis in the same useful field of inquiry. His lecture, from the perfection of his experiments, was well adapted to impress the advantages of pure air on the minds of his audience and the public at large.

We cannot help feeling, however, that it would have been well if the able lecturer had confined his statement strictly to the scientific aspects of his subject. The germ theory of disease has nothing in common with it, and yet it was referred to as if to show that the fact of organic dust existing in the air rendered the existence of "disease germs," as they are called, more probable than they were before. In scientific subjects we cannot accept mere theories for facts. Let the advocates of disease germs first prove their existence, and then possibly optical and microscopic analysis will throw light on their mode of conveyance.

In imperfect science, as in other imperfect things, the first false step may lead anywhere, as the following extract from Dr. Bryden's singularly interesting report on Indian cholera, one of the supposed germ diseases, will show:—

"The facts of the first European invasion showed that aërial transmission did not account for all the phenomena observed, such as the transmission of cholera by fomites, and the occasional infection of attendants on the sick. Hence there was initiated (*a*) the doctrine that cholera might be propagated by human intercommunication, and, as the latest phase of this doctrine, we find the confident assertion promulgated as a truth, that cholera is always and not occasionally so propagated. To prop up this assertion it was necessary to make a second assumption or theory. And hence arose the doctrine (*b*) that cholera is multiplied in the human economy. But this also must have stood alone and unsupported, unless it could be shown how and where the multiplication took place. And this led on to the starting of the third theory (*c*), which asserts that cholera is multiplied in, and is spread around by, the intestinal evacuations of those already suffering from the disease. But even this, although urged in the most forcible manner, did not meet all difficulties; and there arose the demand that it should be supplemented by a fourth theory. In relation to this demand, the latest theory (*d*) alleges that the evacuations of an individual in whom cholera has not become apparent, and never will appear, may be the means of spreading cholera around."

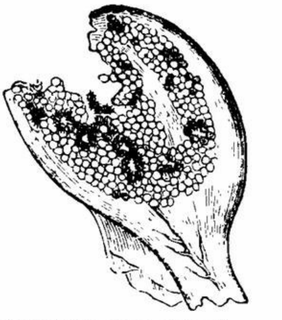
By endeavouring without observation to attain to knowledge which can only be arrived at by observation, theory has, in this instance, walked round in a circle and left science outside. In great questions affecting the health and life of nations, theories are quite out of place. They do no good, cost money, and bar scientific progress.

Practically, so far as health is concerned, Professor Tyndall has given us a scientific account, not only of certain optical properties of impure air, but likewise of the benefit of several popular practices, such, for example, as lighting fires during epidemics to purify the air, the use of gauze curtains in malarious districts as a protection against fever, covering the mouth with a cloth during sleep in fever countries, and the like. He has further given us an additional means of estimating the purity or impurity of the air we breathe. He has shown that heat purifies, more or less, impure air; and that impure air can be deprived of its suspended impurity by filtering it, as is the case with water. On the real proximate aërial cause of disease, if such there be, no new light has been yet thrown either by the optician, the microscopist, or the chemist.

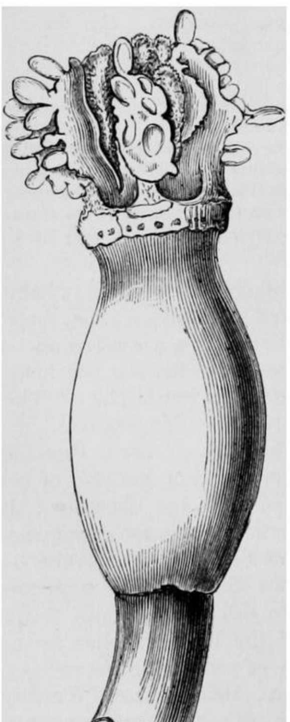
VEGETABLE MONSTROSITIES

Vegetable Teratology: An Account of the Principal Deviations from the usual Construction of Plants. By Maxwell T. Masters, M.D., F.L.S. With numerous illustrations by E. M. Williams. (Published for the Ray Society. 1869.)

IN the volume before us we have the most complete account that has yet been given to the public of the various aberrant forms which are from time to time presented by the different organs of which plants are composed. Such investigations are no mere idle amusement for the leisure hours of naturalists, but have an important scientific bearing. Since botanists have attempted to arrange the vegetable kingdom in a classification possessing a higher claim to the title of "natural" than that proposed by Linnæus, it has been acknowledged that the true, though hidden, relationships of a genus may often be indicated by an abnormal or monstrous variety. A new interest has been given to these inquiries by the theory, now so generally adopted by naturalists, that affinity in structure is but an indication of consanguinity in descent; these exceptional forms or "sports" being regarded as frequently reversions to an ancestral type of structure. Apart, however, from such modifications as are of importance in systematic botany, there are others which are noteworthy as throwing light on controverted points in morphology, and on the relation to one another of the different organs. Among these are the exact morphological character of the carpel, viewed in the light of a metamorphosed leaf; and of the so-called "inferior" ovary, whether its covering is to be considered as a united calyx-tube, or as a modified continuation of the axis. The occasional substitution of one organ for another is carried to a far greater extent in the vegetable than ever appears, even occasionally, in the animal kingdom. As Dr. Masters remarks, the animal physiologist would regard as an incredible monstrosity the replacement of sperm-cells by germ-cells, or the converse; although these are comparable to abnormal growths, of which several are recorded, where ovules are borne by stamens, or pollen is produced inside ovules. The two wood-cuts which we give illustrate these remarkable transformations.



Pollen within Ovule of Passiflora



Ovule-bearing Anther of Cucurbita

De Candolle was the first systematic botanist to draw attention to the importance of Vegetable Teratology: he was followed by Moquin-Tandon, Morren, and others; and the great work of Moquin-Tandon has been followed by Dr. Masters in the main in classifying the phenomena under discussion. This classification of an enormous number of isolated facts presents considerable difficulties. It might at first sight appear as if the most natural arrangement would be to arrange under one head all the known modes of malformation or aberration of each organ: but this plan would involve much repetition, from the frequency with which it happens that similar organs are abnormally affected in the same manner; as when the parts of the calyx and corolla are both unduly increased in number. The plan adopted by Dr. Masters, though very artificial, possesses the advantage of clearness and of easy reference. He divides the phenomena of Teratology into four sections: 1, Deviations from ordinary Arrangement; 2, Deviations from ordinary Form; 3, Deviations from ordinary Number; 4, Deviations from ordinary Size and Consistence; each with several subdivisions. Until we know more of the cause of these variations from ordinary structure, a more scientific classification would appear to be hopeless.

The work is essentially one of reference, a collection of facts rather than a statement of theories. An enormous number of instances, illustrative of every conceivable variation from normal structure, has been collected with unwearied assiduity from English and foreign records, and with that personal knowledge of the subject which few possess to so great a degree as the author. Where Dr. Masters so far strays from his subject as to enter into morphological questions, as when he discusses the irregular monandrous flower of Orchids, we are tempted to wish that he had permitted himself more digressions of the kind.

The text is illustrated by a large number of woodcuts, original and copied, which add greatly to the clearness of the descriptions; and not the least valuable portion of the work is the long list of references at the end of each section to papers and monographs bearing upon it, forming a complete bibliography of the subject. In the lists of plants which are mentioned as peculiarly subject to each description of malformation, we could wish that more care had been taken not to designate the same plant by more than one scientific name, a practice confusing to the student. We also regret that in some instances recognised botanical terminology has been departed from; as in the distinction drawn by Lindley and Oliver between "regularity" and "symmetry." These, however, are but minor defects in a work which we can cordially recommend to all students of botany, who are interested in the real structure of the various organs of plants. A. W. BENNETT

ATTFIELD'S CHEMISTRY

Chemistry: General, Medical, and Pharmaceutical, including the Chemistry of the British Pharmacopœia. By John Attfield, Ph.D., F.C.S. 1 vol. pp. 624. (London: Van Voorst.)

THIS book is mainly intended to supply the want of a manual more expressly suited to the requirements of students of medicine and of pharmacy. A work of this nature necessarily differs in many particulars from the ordinary run of chemical manuals, and it would be unjust, therefore, to judge of it altogether by the standards generally employed in determining the degree of excellence of such books. It is obviously impossible for the medical student to make himself acquainted with the multitude of organic compounds known, the greater portion of which are simply interesting to the scientific chemist on account of the theoretical opinions based on them; and hence it would manifestly be absurd to censure the author of this book for the fact that much of what constitutes modern organic chemistry is either but cursorily treated, or altogether omitted. Nor, on the other hand, should he be blamed for giving what might otherwise appear undue prominence to the descriptions of substances which are simply interesting from the fact of their application as remedial agents, but of which the chemical constitution is either entirely unknown or but imperfectly understood. In all that concerns the most immediate objects which the author had in view in its compilation, his book is a faithful record of the present state of the science. Thus, on page 353 we notice a very complete description of the method of preparing the newly-discovered alkaloid apomorphia, the remarkable physiological effects of which have lately attracted so much attention. The plan of the work is entirely novel. The author commences with some very pertinent advice to candidates as to the best method of studying the book in order to fit themselves for examination by the various boards. After the usual introduction, the student passes on to the practical study of the general properties of the non-metallic elements, and when he has familiarised himself with the various manipulative processes, and acquired a certain amount of chemical knowledge, he proceeds to the study of the general principles of chemical philosophy. The properties of the various metallic elements, their official preparations, and the tests employed in their detection, next engage his attention; after which he is put through a systematic course of qualitative analysis. The student next occupies himself with the study of the compounds of vegetable and animal origin, with the reactions of the alkaloids and of some other organic principles, and of the various substances which the author distinguishes as Galenical, and which can only fairly be regarded from a pharmaceutist's point of view, many of them being "not yet brought within the grasp of the chemist." The principles of toxicology, and the various methods employed in the examination of morbid urine and calculi, are then explained, and the different classes of official, Galenical, and chemical preparations enumerated. A course of quantitative analysis, sufficiently comprehensive for the student's requirements, and consisting of both gravimetric and volumetric processes, next follows. Several of the gravimetric methods are, however, in our opinion not the best at the disposal of the analyst. Thus, for the estimation of nitric acid Frankland and Armstrong's method of determining the amount of that acid in potable waters, is the only one recommended. This method, although doubtless excellently adapted to the purpose for which it was devised, is not, however, generally applicable. We would recommend the method of Vernon Harcourt to Dr. Attfield's attention. With some slight modifications, this method is pronounced by Professor Bunsen, of Heidelberg, in whose laboratory it is constantly used, to be by far the best of the many processes hitherto proposed for the estimation of nitric acid; and in the laboratory to which the writer is attached it is frequently employed with the most satisfactory results. The account given of the processes for the ultimate analysis of organic substances also appears to be somewhat defective, and the statement that the best combustion-furnace is that known as Hofmann's is open to dispute. The furnaces of Eslenmeyer and of Donny as modified by Glaser are certainly to be preferred; indeed, we understand that the Berlin professor has already renounced the use of the furnace which bears his name. Dr. Attfield is surely in error, also, in recommending (page 460) that the boiling point of a liquid should be determined by inserting the bulb of the thermometer in the heated liquid. Kopp pointed out long ago the errors incidental to this method of procedure. These, however, are defects of but minor importance, and may easily be remedied in future editions. We have derived much satisfaction from the perusal of Dr. Attfield's book: it is eminently practical in its character, and is written with a just appreciation of the small amount of time for the study of chemistry at the disposal of the student in medicine and pharmacy.

T. E. T.

OUR BOOK SHELF

Japanese Shells.—*Japanische Meeres-conchylien.* By Dr. C. E. Lischke. (Cassell: 1869.) Quarto, with 14 coloured plates.

JAPAN is not less remarkable for the works of its people than for its natural productions. Its sea-shells are of a mixed character, arctic and tropical. Some species range to the Mediterranean; for *Verticordia granulata* of Seguenza, from the Sicilian tertiaries, which I have now discovered living in the Gulf of Egina at a depth of 130 fathoms, was lately dredged by Mr. A. Adams in the seas of Japan, and is described by him as *V. multicostata.* Another species of *Verticordia*—or perhaps more correctly *Hippagus*—the *H. acuticostatus* of Philippi, a Calabrian and Sicilian fossil (which occurs also in our Coralline Crag, under Sowerby's name of *V. cardiiformis*), is the *V. Deshayesiana* of Fischer, and *V. Japonica* of A. Adams, from China and Japan. The only other known living species of *Hippagus* (*Trigonulina ornata*, D'Orbigny = *H. novemcostatus*, Adams and Reeve) is common to the West Indies and China. Unfortunately we know far too little of the former and present course of those great currents which traverse the ocean in every direction, to be able to explain satisfactorily the geographical distribution of the marine fauna. Nevertheless, although physical data are wanting, zoological facts are accumulating; and Dr. Lischke, as well as Mr. Arthur Adams, have rendered great assistance by their investigation of the Japanese mollusca. The present is not a complete treatise on the subject; but it shows great care and critical acumen, and it is beautifully illustrated. The author is Oberburgomeister of the large manufacturing town of Elberfeld, and finds time not only for his onerous public duties, but also for good scientific work; so that in other countries besides our own, writers on natural history are not confined to the class of paid professors.

J. GWYN JEFFREYS

Chemistry for Schools.—*An Introduction to the Practical Study of Chemistry.* By C. Haughton Gill, Assistant Examiner in Chemistry at the University of London. (London: Walton.) 8vo. pp. xv. and 315. 1869.

DURING the last few years the subject of science teaching in schools has occupied so much attention that a special class of manuals has been originated for the schoolmaster's use. Those which treat of Chemistry have been in some cases experiments, seldom remarkable for true appreciation of their professed purpose, or, perhaps, merely the pecuniary speculation of an ignorant writer. Under such circumstances, it is gratifying to meet with a book of this kind, which really is what it was intended to be—"a sufficient manual of chemistry for schools and junior students, and an aid to teachers wishing to introduce the science into the ordinary course of school study."

Mr. Gill's experience as teacher of chemistry and experimental physics at University College School appears to have been embodied in his book, if we may judge from its decided and perspicuous tone, and an evident intention that the pupil should strive, not only to *know*, but to *reason*. It is, indeed, precisely at this point that the power and merits both of teacher and writer become most apparent; and we have no hesitation in saying that it is in this sphere that the efficacious distinction between one manual and another lies.

After "Directions to the Reader" (as to the most advantageous mode of using the book), a list is given of the chapters into which the subject is divided. An exhaustive series of questions follows each chapter. A great deal of space has been gained by printing many of the necessary comments and descriptions in a smaller type than the more important text; and the illustrations, though generally of diminutive size, are no doubt large enough, and certainly distinct enough, for most readers. It need hardly be said that the province of *Chemistry for Schools* is comprised within the limits of the "metalloids" and their immediate allies. The nomenclature employed is throughout what has been termed "Berzelian," but is, in fact, derived from the hereditary Latin forms, which have been common for centuries to the whole of natural science, and are still the only ones which can be legitimately adapted to the requirements of our own language. The writer terminates with some useful appendices, not the least valuable of them being a list of necessary apparatus and chemicals, with approximate cost.

Mr. Gill has fairly earned the thanks of scientific chemists; nor will the schools be slow to appreciate a manual which has been thus well devised and executed by an author who has himself been a successful school teacher.

The Blow-Fly.—*A Monograph on the Anatomy and Physiology of the Blow-Fly.* By Benjamin Thompson Lowne. (Van Voorst.)

IN this little volume, just issued by Van Voorst, Mr. Lowne has treated the subject of his monograph very exhaustively. The text is judiciously divided into two parts, the first comprising a neat popular sketch of the organisation of this familiar and pertinacious little companion of domestic life; and the second containing the more technical and elaborate account of the author's own dissections and investigations, which are illustrated by ten very beautiful plates, engraved by his own hand from his own microscopical demonstrations.

The book is eminently satisfactory, as being a clear and complete statement of what is known of fly-organisation. But it has also, in some degree, the stamp of original research upon it, and represents a remarkable amount of labour and industry. The nature of the all-embracing integuments, and the way in which they are modelled to form the external organs and implements of the creature's active life, are in the first instance dwelt upon. The theme then passes on to the examination of the digestive and assimilative apparatus, the arrangements for circulation and respiration, the nerve structure, and the organs of special sense. It would be possible to pause upon matters of particular moment and interest in each one of these departments of the treatise. But it must, for this occasion, suffice to draw attention to the explanation of the way in which the so-called false tracheæ of the trunk are modelled into an exquisite strainer, to enable the fly to draw off the finer and more nutritious parts of the half rotten pulpy matters that are used as food, and to the manner in which the terminal lip of this sucking trunk is furnished with supplementary rasping teeth and salivary pores, to allow such matters as loaf sugar to be broken down and dissolved into a juice also available for suction. The description of the manner in which the poisers, properly the abortion of the second pair of wings, are turned to account as ears which receive the vibrations of sound upon terminal knobs, instead of within trumpet cavities, is also most worthy of notice. But before all must stand Mr. Lowne's demonstration of the fly brain. He shows that the fly has a sense-centre, or cephalic ganglia, some thirty times larger than that of the most portly beetle, which sufficiently accounts for the energy and vivacity of the insect's life; and that it has also, in common with the bee and the ant, a small rudimentary convoluted brain, attached by a little footstalk to the larger and simpler nerve-mass of the head. Mr. Lowne holds that the fly clearly exhibits some trace of mental faculty, such as memory, in virtue of this shadowing forth of true cerebral organisation.

Terrestrial Physics.—*Ueber die Lehre von den Meeresströmungen.* By Dr. Adolf Mühry. (Göttingen, 1869. London: Williams and Norgate.)

THIS is a very successful attempt to introduce something like order into the complicated phenomena of oceanic currents. The author sketches first the two well-known main systems, viz. (1) the great west-current which forms a belt of nearly 50° of latitude on both sides of the equator, and to which the earth's rotation, combined with the inertia of the ocean, is assigned as cause; (2) the great thermal circulation from the poles towards the equator, with its compensating current in the opposite direction; both are, according to the author, produced by the difference in density of cold and warm water, and he refutes, with great knowledge and sagacity, the opinions of previous writers, of Maury among others, who seek the cause in differences in the amount of evaporation and rain, the prevailing winds, and the amount of saline matter in the sea.

Then follows an *exposé* of comparatively local systems. Here the author supplants the incompleteness of known facts by his own speculations, which are neither always clear nor above the suspicion that doubtful points have been decided by the author, with a view of confirming his own hypotheses. Thus a very elaborate chapter on the currents in the North Polar Basin rests entirely on his assumption that sea-water behaves like pure water as regards the temperature at which it has the greatest density. He describes some very crude experiments made by him, which prove the fact in his opinion, but are contradicted by the well-known experiments of such a distinguished physicist as Despretz, who found different points of maximum density for different saline solutions.

B. L.

Protozoe Helvetica.—*Mittheilungen aus dem Berner Museum der Naturgeschichte über merkwürdige Thier-und Pflanzenreste der schweizerischen Vorwelt.* Edited by W. A. Ooster and C. von Fischer-Ooster. Part I. Basle and Geneva, 1869. 4to. pp. 14, map and two double plates. (London: Williams and Norgate.)

THIS is the first fasciculus of a series intended to illustrate the palæontology of Switzerland. The work is intended chiefly as a means of making known by descriptions and drawings a number of interesting fossils from the animal and vegetable kingdom, in part at least new to science. Most of these have been derived from the Swiss Alps, and are now preserved in the Bern Museum of Natural History. It is also intended to serve as the organ for shorter palæontological communications from the whole extent of Swiss territory, the several authors being answerable for their own views. The first part contains a short paper, just completed, "On the Red Limestone of Wimmis and its Fauna;" the next will contain plates and descriptions of various remarkable fossils from the Swiss Alps. Three, or at most four such parts will form a volume, when a title-page and index will be issued.

We need only add that the plates before us contain figures of fish-teeth (*Oxyrrhina*, sp.), mollusca (*Inoceramus Brunneri*, and an undetermined species), and echinodermata (*Collyrites Friburgensis* and *C. capistrata*), that the drawings are of large size, and, except for occasional flatness in the shading, well-executed.

H. B. B.

ARE ANY OF THE NEBULÆ STAR-SYSTEMS?

THIS may seem a bold question, for it is commonly believed that Sir William and Sir John Herschel—the Ajax and the Achilles of the astronomical host—have long since proved that many of the nebulæ are star-systems. If we inquire, however, into what the Herschels have done and said, we shall find that not only have they not proved this point, but that the younger Herschel, at any rate, has expressed an opinion rather unfavourable than otherwise to the theory that the nebulæ are galaxies in any sense resembling our own sidereal system.

Sir William Herschel, by his noble plan of star-gauging, proved that the stars aggregate along a certain zone, which in one direction is double. He argued, therefore, that presuming a general equality to exist among the stars and among the distances separating them from each other, the figure of the sidereal system resembles that of a cloven disc.

And as the only system from which he could form a probable judgment—I mean the planetary system—presented to him a number of bodies, widely separated from each other and each a globe of considerable importance, he reasoned from analogy that similar relations exist in the sidereal spaces. This being so, his cloven disc theory of the sidereal system seemed satisfactorily established.

Then, of course, those nebulæ which exhibit a multitude of minute points of light very close together, and those other nebulæ which, while not thus resolvable into minute points, yet in other respects resemble those which are, came naturally to be looked upon as distinct from the sidereal system. The analogy of this system, in fact, pointed to them as external star-systems, resembling it in all important respects.

Then there were certain other objects, which seemed to present no analogy either to the sidereal system or to separate stars. These objects Sir Wm. Herschel considered to belong to our sidereal system; for he could not put them outside its range without looking on them as objects *sui generis*, which would have been to abandon the argument from analogy. In order to explain their appearance, he suggested that they might be gaseous bodies, by whose condensation stars would one day be formed.

The value of Sir Wm. Herschel's work is not in the least affected even if science have to reject every one of these opinions. He himself held them with a light hand; he had once held other opinions; and he was gradually modifying these. Had he seen one sound reason for rejecting any or all of them he would have done so instantly. For it belonged to the strength of his character that he was never fettered by his own opinions, as weak men commonly are.

Sir John Herschel did for the southern heavens what his father had done for the northern. He completely surveyed and gauged them. It is commonly believed that the results of his labours fully confirmed the opinions which his father had looked upon as probable.

Let us see if this is so.

Sir W. Herschel thought the Milky Way indicated that the sidereal system has the figure of a cloven disc; Sir John Herschel judges rather that the sidereal system has the figure of a flattened ring. Sir Wm. Herschel thought the stellar nebulæ are probably external galaxies; Sir John gives reasons for believing that they lie within our system, and Whewell considered that these reasons amount to absolute proof.

It has been further believed and stated that the researches of the elder Struve go far to confirm the opinions put forward by Sir W. Herschel as probable.

Let us inquire how far this is true.

Struve found that the numbers of stars of given magnitudes exhibit nearly the same proportion in different directions. Thus supposing that in a given direction there are three times as many stars of a certain magnitude as there are of the next highest magnitude, then in other directions, also, the same relation is observed. This is a very striking law; but to make it serve as a proof of the opinion which Sir William Herschel had put forward as probable, it would be necessary that another law should be exhibited. For clearly, if that opinion were just, it would be easy to calculate what the relation should be between stars of different magnitudes. Had Struve been able to show that the numbers actually seen corresponded to the relations thus calculated, he would have gone far to render that view certain which Herschel always spoke of as merely an assumption.

But Struve found no such law of stellar distribution. On the contrary, he found a law so different, that in order to force the facts into agreement with Sir William Herschel's views about the sidereal system, he had to invent his famous theory of the extinction of light in traversing space. Now, according to this theory, we cannot see to the limits of our sidereal system, even though we could increase the powers of our telescopes a million-fold; so that if the theory is true, the question which heads this paper is at once disposed of. Obviously, we cannot see galaxies beyond the sidereal system if we cannot see to the limits of that system. And I may note in passing that (independently of Struve's theory) the most powerful telescopes cannot render visible the most distant stars of our sidereal scheme; so that if the nebulæ are really external galaxies, the stars we see in them must be enormously greater than those in our galaxies, supposing Herschel was right in thinking these tolerably uniform in magnitude.

Before proceeding to exhibit the evidence which has led me to the conviction that the nebulæ belong to our sidereal system, I may mention some reasons for believing that if Sir William Herschel's labours in the sidereal heavens were to be begun now, not only would he not have been led to adopt as probable the view on which he formed his opinions, but he would have rejected it as opposed to known analogies.

He had argued that because the planetary system exhibits a definite number of bodies separated by wide distances, therefore analogy should lead us to regard the sidereal system as similarly constituted, though on a much larger scale. This was perfectly just. Despite the various differences which no one recognised more clearly than he did, this view was the only one he could safely adopt for his guidance, ninety years ago.

But would not he have been the first to reject that view if he had known what we now know of the solar system? If he had known that besides the primary planets, there are hundreds of minute bodies forming a zone between the orbits of Mars and Jupiter; that the rings of Saturn are formed of a multitude of minute satellites; that innumerable meteor-systems circle in orbits of every conceivable degree of eccentricity; that near the sun these systems grow denser and denser; that the comets of the solar system must be counted by millions on millions; that, in fine, every conceivable form of matter, every conceivable degree of aggregation, and every conceivable variety of size, exists within the limits of the solar system, —would he, then, have been led by analogy to recognise in the sidereal system only discrete stars and masses forming into stars?

From a careful study of all that Sir William Herschel has written, I feel certain, that in the case I have imagined, he would have been prepared, even before commencing his labours, to expect precisely that variety of matter, size, and aggregation, which modern observations, rightly understood, prove actually to exist within the range of the sidereal system.

The Herschels, father and son, discovered about 4,500 nebulæ. Other observers have brought up the number to about 5,400. When these are divided into classes, it appears that some 4,500 must be looked on as irresolvable into discrete points of light. But of these the greater proportion so far resemble resolvable nebulæ as to lead to the belief that increase of optical power alone is wanting to resolve them.

Taking these irresolvable nebulæ, however, as we find them, and marking down their places over the celestial sphere, we recognise certain peculiarities in their arrangement. In the northern heavens they gather into a clustering group as far as possible from the Milky Way. In the southern heavens they form into streams, which run out from a region nearly opposite the northern cluster of nebulæ; but the *extremities* of the streams are the region where nebulæ are most closely crowded. The Milky Way is almost clear of nebulæ.

This withdrawal of the nebulæ from the Milky Way has been accepted by many as clearly indicating that there is *no* association between them and the sidereal system. The opinion of the Herschels, if they had been led to pronounce definitively on this point, would have been different, however; for the younger Herschel quotes (as agreeing with it) a remark of his father's to the effect that the peculiar position of the northern nebular group is not accidental. If not accidental, it can only be due to some association between the nebular group and the galaxy. Every other conceivable explanation will be found to make the relation merely apparent—that is, accidental, which neither of the Herschels admit.

But yet stronger evidence of association exists; evidence which I do not hesitate to speak of as incontrovertible. Space will only permit me to treat it very briefly.

There is a certain well-marked stream of nebulæ in the southern heavens leading to a well-marked cluster of nebulæ. There is an equally well-marked stream of stars leading to an equally well-marked cluster of stars. The nebular stream agrees in position with the star-stream, and the probability is small that this coincidence is accidental. The nebular cluster agrees in position with the star-cluster, and the probability is still smaller that this second coincidence is accidental. Such are the separate chances. It will be seen at once, therefore, how small the chance is that both coincidences are accidental.

The cluster here referred to is the greater of the celebrated Magellanic Clouds. When it is added that the evidence is repeated point for point in the case of the lesser Magellanic Cloud, the indications of association appear overwhelmingly convincing. If the nebulæ really are associated in this manner with fixed stars, the question which heads this paper is disposed of at once.

But there is yet further evidence.

The nebulæ pass by insensible gradations from clusters less and less easily resolvable, to nebulæ properly so called, but still resolvable, and so to irresolvable nebulæ. Now clusters are found not only to aggregate in a general manner near the Milky Way, but in some cases (on which Sir John Herschel has dwelt with particular force) to exhibit the clearest possible signs of belonging to that zone. If they then belong to the Milky Way, can any good reason be given for believing that the various other classes of nebulæ are not associated with the sidereal scheme? Where should the line be drawn?

Again, some of the nebulæ are gaseous, and all the gaseous nebulæ exhibit the same spectrum. Now, two classes of gaseous nebulæ, the planetary and the irregular nebulæ, exhibit a marked preference for the Milky Way, and therefore we must admit the probability that they, at any rate, belong to the sidereal scheme. But then a large proportion of the irresolvable nebulæ are also gaseous, and as they are formed of the same gases, we see good reason for believing that they also must belong to our galaxy. This, however, brings in all the nebulæ, since the recent detection by Lieut. Herschel of the same bright lines in or rather *on* the continuous spectrum of a star-cluster, shows the great probability which exists that with more powerful spectroscopes all the nebulæ may be found to exhibit these bright lines, that is, to contain these particular gases. I pass over the facts, that many nebulæ are found to be closely associated with stars, and that if any doubt could remain as to the association being real and not apparent, it would be removed by a picture of the nebula M 17, as seen in Mr. Lassell's great reflector at Malta. The reader will be more interested by the following quotation, which I extract (by permission) from a letter of Sir John Herschel's :—

"A remark which the structure of Magellanic Clouds has often suggested to me has been strongly recalled by what you say of the inclusion of every variety of nebulous or clustering forms within the galaxy, viz., that if such be the case—*i.e.* if these forms belong to, and form part and parcel of the Galactic system—then that system includes *within itself miniatures of itself* on an almost infinitely reduced scale; and what evidence, then, have we that there exists a universe beyond—unless a sort of argument from analogy, that the Galaxy with all its contents may be but one of these miniatures of a more vast universe, and that there may, in that universe of other systems on a scale as vast as our galaxy, be the analogues of those other nebulous and clustering forms which are not miniatures of our galaxy?"

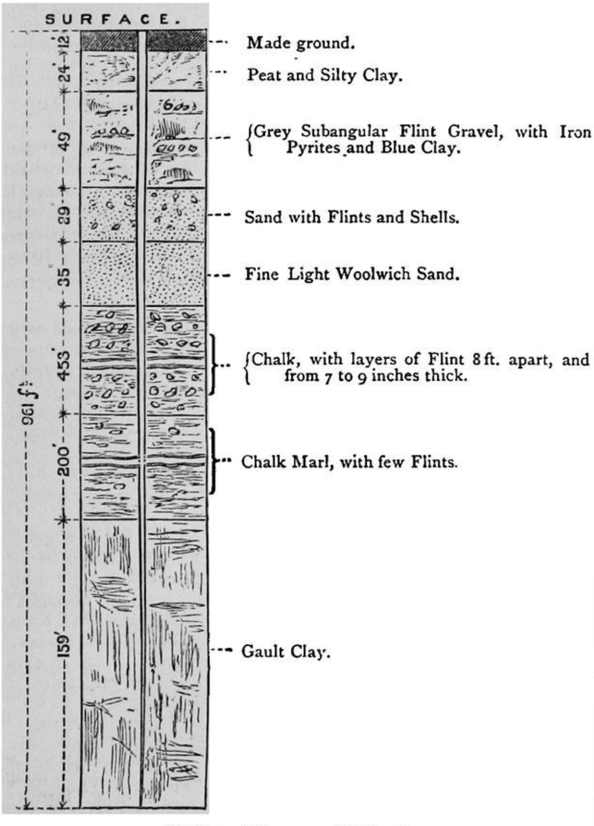
It will be seen that, while Sir John Herschel is quite ready (should the evidence require it) to adopt altogether new views about the nebulæ, he is *not* ready to forego the grandeur of those noble views of the universe which he and his father have established, thereby earning the well-deserved gratitude of every lover of astronomy.

And then with regard to the *actual* form of our galaxy or Milky Way, the figure introduced shows that its apparent one as projected on the heavens may really be due to an arrangement differing both from the cloven disc or flattened ring, a point to which I shall return in a subsequent article.

RICHARD A. PROCTOR

THE CROSSNESS WELL-BORING

THIS boring, which was commenced by the Metropolitan Board of Works for the purpose of supplying the engines and dwelling-houses at Crossness with pure water, has, as may be seen from the accompanying diagram, reached a depth of 961 feet without piercing the Lower Greensand, where it was expected a good supply would be found.

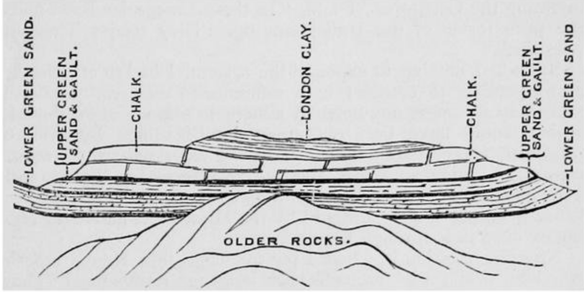


Section of Crossness Well-boring.

In consequence of the great difficulties attending the extraction of broken rods, &c., from the boring at that great depth, together with the uncertainty of the Lower Greensand being present, the boring has been discontinued. This is much to be regretted, as, if persevered with for a further depth of 40 or 50 feet, it would undoubtedly pass through the gault, which seldom exceeds 200 feet in thickness, 147 feet of which are now entered, and would thus add to the knowledge we at present possess of the substratum of London.

The chalk and gault clay at their outcrop to the north and south of London are underlaid by the Lower Greensand, which is from 150 to 400 feet thick; this, if continuous, as are the chalk and gault, would give a water-bearing strata of great capacity.

But it has been proved that its continuity is broken; as at Harwich, where after boring 1,000 feet through chalk, some carboniferous slates were found, and at Kentish Town, where beneath chalk and gault were found red sandstone and clay, though whether they belong to the Old or New Red Sandstone group, could not be ascertained. Mr. W. Whitaker very reasonably supposes that there is an underground ridge of older rocks crossing the London basin, which was an island when the Greensand was deposited, as the accompanying sketch shows, thus accounting for its absence in the places above mentioned.



Section across London Basin, showing Probable Position of Ridge of Old Rocks.

If the Crossness boring were continued, and the Greensand were not found, the direction in which this ridge runs would be ascertained, and thus would be prevented much fruitless outlay to those contemplating well-boring; in addition to which, some important facts connected with the London water supply would be made known; further, it might decide the question as to the existence of the coal measures beneath London, at a practicable depth, which, it will be admitted, is a question of universal interest. Under these circumstances, surely Government aid ought to be invoked, as the Board of Works are unwilling to proceed with the boring on their own responsibility.

UTILISATION OF SEWAGE

THE British Association Committee on the Treatment and Utilisation of Sewage has requested us to state that a number of towns and private individuals have already sent in or promised subscriptions for defraying the expenses of the contemplated investigation referred to in the circular published in NATURE of the 2nd of December, and that a Special Meeting of the Committee will be held on the 15th of February next to decide what further steps are to be taken in furtherance of the object in view.

The Committee therefore requests that town and district authorities who have not yet replied to the circular will at their earliest convenience communicate with the Committee, and state what sum will be subscribed; or, if it be decided not to subscribe, what is the reason for declining.

Should the total amount subscribed be insufficient for adequately continuing the inquiry, it is the intention of the Committee to return the subscriptions received.

The following Towns and Districts have subscribed, or signified their disposition to do so:—Stoke-upon-Trent, Exeter, Plymouth, Devonport, Paisley, Coventry, Oxford, Maidstone, Torquay, Wakefield, Dewsbury, Hereford, West Hartlepool, Kendal, Weymouth, Enfield, Penzance, Balsall Heath, Bromley, Bridport, Malvern, Abingdon, Atherton, Toxteth Park, and Walton-on-the-Hill.

LETTERS TO THE EDITOR

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

Kant's View of Space

ALTHOUGH I do not feel myself called upon to modify in the least what was said in my former letter on this subject, the three letters which appear to-day in answer to it are too important to be left unnoticed.

The case is briefly this: In the "History of Philosophy" I had to expound Kant's doctrine, and to criticise it, not only in itself, but in reference to the great question of the origin of knowledge. In the pages of exposition I *uniformly* speak of Space and Time as forms of Intuition; no language can be plainer. I also mark the distinction between Sensibility and Understanding, as that of Intuition and Thought. After enumerating the Categories, I add, "In those Categories Kant finds the pure forms of the Understanding. They render Thought possible."

But when, ceasing to expound the system, I had to criticise it, and especially to consider it in reference to the great question; there was no longer any need to adhere to a mode of expression which would have been obscure and misleading. I therefore *uniformly* class Space and Time *among the forms of Thought*, connecting them with the doctrine of Necessary Truths and Fundamental Ideas, which, according to the *à priori* school, are furnished ready-made—brought by the Mind as its native dowry, not evolved in it through Experience.

Now the question is, Have I put language into Kant's mouth which he would disclaim, or is such language misleading? That Kant would have said the language was not what he had employed, I freely admit; but that he would have disclaimed it as misrepresenting his meaning, I deny. I was not bound to follow his language when the task of exposition was at an end; but only bound not to translate his opinions into language which would distort them.

In classing Space and Time *among* the Forms of Thought I classed them *beside* the Categories of the Understanding and the Ideas of Reason, *i.e.*, the purely intellectual conditions existing *à priori* in the Mind. The Mind is said by Kant to be endowed with three faculties—Sensibility, Understanding, and Reason. The activity of the Mind is threefold—Intuitive Thought, Conceptive or Discursive Thought, and Regulative Thought. There could not be an equivoque in my using the word Thought in its ordinary philosophical acceptation as expressive of all mental activity whatever, exclusive of mere sensation; although Kant assigns a more restricted meaning in his technical use of the word, *i.e.*, what we call Logic. And that Kant *meant* nothing opposed to the ordinary interpretation is obvious. It is obvious because, as I said in my former letter, Intuition without Thought is mere sensuous impression. Mr. Sylvester demurs to this, so I will show it in a single citation:—"In the transcendental Æsthetic," says Kant, "we will first isolate Sensibility by separating from it all that the Understanding through its concepts thinks therewith, so that nothing but empirical Intuition remains. Secondly, we will lop off from this empirical Intuition everything relating to Sensation (*Empfindung*); so that thereby nothing will remain but pure Intuition and the mere form of phenomena, which is the one thing that Sensibility can furnish *à priori*. By this investigation it will appear that there are two pure forms of sensuous Intuition which are *à priori* principles of Cognition." ("Kritik," § 1. ed. Hartenstein, p. 61).

Mr. Sylvester correctly says, that Intuition and Thought are not convertible terms. But he is incorrect in assuming that they differ as potential and actual; they differ as species and genus; therefore whatever is a form of Intuition, though not a form of Logic, must be a form of Thought; unless intuitive Thought be denied altogether. How little Kant denied it is evident in every section of his work. In asserting that Space and Time as Intuitions belong to the subjective constitution of the Mind—*subjectiven Beschaffenheit unseres Gemüths* (p. 62)—he expresses this; but it is unequivocally expressed in the following definition:—"A perception, when it refers solely to the subject, as a modification of its states, is *sensation*, an objective perception is *cognition*. *this is either Intuition or Concept*, 'intuitus vel conceptus.'" ("Kritik," p. 294.) Is not thought implied in cognition? Again:—"The proposition 'I think' is an undetermined empirical Intuition, *i.e.*, Perception; consequently, it proves that Sensation, which belongs to Sensibility, must lie at the basis of this proposition......I do not mean thereby that the 'I' in the 'I think' is an empirical representation (*Vorstellung*), on the contrary, it is *purely intellectual because it belongs to thought in general*. But without some empirical representation which would give Thought its material there could be no such act of Thought as the 'I think'" (p. 324, *note*).

"Man is always thinking," says Hegel, "even when he has nothing but intuitions; *denkend ist der Mensch immer auch wenn er nur anschaut.*" (Encyclop. § 24.)

If, because Kant has a restricted use of the term Thought, all who venture on the more ordinary use are said to misrepresent his philosophical meaning, I must call upon those who criticise this laxity to refrain henceforth from speaking of Reason as Thought, since Kant no less excluded Reason from the province of the Understanding. If "the only forms of thought, in Kant's sense, are the Categories," this sweeps away Reason on the one side, as it sweeps away Sensibility on the other; and Ideas are not more correctly named Thoughts than Intuitions are. Kant, it is true, speaks of the concepts of Reason, and defines an Idea to be a "Vernunft begriff" (page 294); but Kant, equally and in a hundred places, speaks of the "concept of Space" (Begriff des Raumes). The truth is, as already intimated, that in spite of his technical restriction of Thought to the formation of concepts, he recognised intuitive and regulative Thought no less than discursive Thought; nor would his system have had any coherence without such a recognition. Why does he call his work the "Critik of Pure Reason," unless he intended to display the common intellectual ground of Sensibility, Understanding, and Reason? and does not the word Thought, in ordinary philosophical language mean this activity of the Intellect? When, by Sir W. Hamilton, Dr. Whewell, Mr. Spencer, and myself, the phrase Forms of Thought is used, does not every reader understand it as meaning Forms of intellectual activity?

In conclusion, I affirm that in the ordinary acceptation of the term Thought—the activity of the Mind—Space and Time as forms of Intuition are forms of Thought, conditions of mental action; and to suppose that because Kant's language is different, his meaning is misrepresented by classing forms of Intuition among the forms of Thought is to misunderstand Kant's doctrine and its purpose.

GEORGE HENRY LEWES

January 22.

DR. INGLEBY, I should think, is quite entitled to say not only that Kant might, but that he would, have disclaimed the phrase Form of Thought as applied to Space or Time taken simply. The remark of Mr. Lewes, that "intuition without thought is mere sensuous impression,"—or, as it might have been put, that phenomena of sense (constituted such in the forms of Space and Time) must further be thought under Categories of Understanding, before they can be said to be known or to become intellectual experience—cannot be a sufficient reason for making a Form of Thought proper out of a Form of Intuition.

There is, nevertheless (and Mr. Lewes does not fail to suggest it), a sense in which, when taken along with the Categories of the Understanding, and with or without the Ideas of the Reason, the Forms of Intuition may be spoken of as Forms of Thought: Thought being understood, with the same extension that Kant himself gives to Reason in the title (not the body) of his work, as equivalent to faculty of Knowledge in general. It is in this sense that Kant calls all the forms alike, *à priori* principles of Knowledge; and the ambiguity of the word Thought is so well recognised, that the English writers, arraigned by Prof. Sylvester, take no great liberty, when for their purpose, which commonly is the discussion of the general question as to the origin of Knowledge, they talk generally of Kant's "Forms of Thought." If, indeed, any of them ever speaks of Space as a "form of the Understanding," which was part of the original charge, the case is very different; Kant being so careful with his *Verstand*. But Mr. Lewes at least would never be caught speaking thus, even though his main reason for merging Intuition in Thought might seem to justify this also.

G. CROOM ROBERTSON

University College, January 22.

YOU will perhaps permit me to make a remark on a controversy at present going on in your columns. There has seldom, I believe, been a grosser or more misleading perversion of the Critical Philosophy than ascribing to Kant the view that Space and Time are in any meaning of the terms "forms of thought." One of his chief grounds of complaint against Leibnitz is, that the latter "intellectualised these forms of the sensibility" (Meiklejohn's Translation of the "Critick," p. 198): and lest the import of this assertion should be mistaken, he explicitly tells us that "Space and Time are not merely forms of sensuous intuition, but intuitions themselves" (Meiklejohn's Trans., p. 98): that is, *sensuous* intuitions, as he has been just before asserting that all human intuitions must be. It is precisely on this distinction of pure sensibility and pure thought that Kant founds the possibility of Mathematics—a science which could never be derived from a mere analysis of the concepts employed, but only from the construction of them in intuition. He ridicules, for example, the idea of attempting to deduce the proposition, "Two right lines cannot enclose a space," from the mere concepts or notions of a straight line and the number two. "All your endeavours," says he, "are in vain, and you find yourself compelled to have recourse to intuition, as in fact Geometry always does." (Meiklejohn, p. 39: see also his long contrast of Mathematical and dogmatical methods in the beginning of the "Methodology.") And not only is Kant's Mathematical theory founded on this distinction but his Physical theory also, since it is only by means of pure intuition that he connects pure thought with sensations (see the "Schematism" and still more the "General Remark on the System of Principles," Meiklejohn, pp. 174-7); and when he fails to make out this connection he regards the Ideas of Pure Reason as possessed of no objective validity (Transcendental Dialectic). In the first edition of the "Critick" he went still further, and in his remarks on the Second Paralogism of Rational Psychology he speaks of "that something which lies at the basis of external phenomena, which so *affects* our *sense* as to give it the representations of *space*, matter, *form*, &c." And while he abbreviated his discussion in the second edition he tells us in his preface that he found nothing to alter in the views put forward in the previous one.

I might quote whole pages of the "Critick" in proof of these views, but I ought rather to apologise for writing so much after the letters which you have already published. I believe the mistakes as to Kant's doctrine of Space and Time, his refutation of Idealism, and his discussion of the Antinomies of the Pure Reason, are almost without a parallel in the History of Philosophy.

Trinity College, Jan. 22 W. H. STANLEY MONCK

State Aid to Science

I OBSERVE that both in your leading article and in the correspondence upon Mr. Wallace's letter, the soundness of his theory of taxation seems to be conceded, though you quarrel with his inference that Science ought not to receive Government aid. But will his theory hold water for a moment? The theory as I understand it is this: "No money raised by general taxation ought to be applied for any purpose which does not directly benefit everybody." In other words, "It is not fair to take A's money and use it for the benefit of B." Why not, if at the same time you take a proportionate amount of B's money and use it for the benefit of A? Suppose you tax people who don't want gratuitous education for themselves, and spend the money on primary schools. This is expenditure for the direct benefit of one class only; and indirect benefits, according to Mr. Wallace, are not to be taken into account. This, according to the theory, would be an unfair application of public money. But if at the same time you apply a proportionate amount of public money for the benefit of all those who reap no direct good from gratuitous schools, you exactly redress the injustice; and, so far as it goes, expenditure on Science is an expenditure of this character.

If Mr. Wallace's theory were sound, there is no conceivable application of public money which it would not condemn. There is no public expenditure which directly benefits all. Take the payment of dividends on Consols, which eats up a third of our revenue. How does an agricultural labourer benefit by this? Not directly, certainly, and I am not sure that he does even indirectly. The only indirect good is, that it maintains public credit, and enables the Government to borrow again and to go to war on the strength of it. What good does that do to the labourer? Perhaps it may be said it is the fulfilment of a moral obligation. But whose moral obligation? Not Hodge the ploughman's. Even the least exceptionable of all outlay, that on police, is of very doubtful benefit to those who have nothing to lose. And the theory, if sound, must go a step further than Mr. Wallace carries it. If all public expenditure ought to benefit all, it ought by the same reasoning to benefit each in exact proportion to his contribution, and no system of taxation and expenditure even pretends to approach this condition.

Obviously Mr. Wallace could not have meant what he said. He must have meant this: "Public expenditure as a whole ought to benefit taxpayers in proportion to what they pay." Put in this way it is a fair doctrine, to which our actual adjustment of taxation and expenditure ought to approximate as nearly as may be. But this is quite consistent with special expenditure for the benefit of special classes, provided it is fairly balanced by other special expenditure for all other classes. If, on the whole, men of science are getting more than their share of the good things going, by all means stop the supply; if they are getting less than their share, give them something more. This is surely fair, and it is an intelligible working principle. Mr. Wallace's principle has only this to recommend it, that it would be impossible to find any object which would justify the levying of a single sixpence from your humble servant or any other TAXPAYER

P.S.—I hope that in discussing Mr. Wallace's argument on his own grounds, I shall not be supposed to agree with him that the direct and immediate benefit is the only thing to be looked to. If a man or a class gets a benefit, it does not lose its value by coming indirectly. And, as a matter of fact, expenditure on Science does, as you and others have sufficiently pointed out, confer indirect benefits on the non-scientific classes, incomparably beyond any little direct advantage to the scientific students whose work is promoted by it.

Use of the word "Correlation"

I OBSERVE in your last number you adopt the phrase of Mr. Barrett, "Correlation of colour and music." Will you and Mr. Barrett pardon a criticism on the application of the word "correlation?"

I believe I was the first who ever used the word at all as an English word, though the words "correlate," "correlative," &c., are to be found in Johnson. At all events, I stretched the meaning, and apologised for so doing in my essay on the "Correlation of Physical Forces." Wherever the word "correlative" was used to express a mutual and inseparable relation of two ideas, such as parent and offspring, height and depth, &c., I ventured, for want of a better term, to apply it, and the new substantive "correlation" to reciprocal relations of phenomena, such as heat and electricity, electricity and magnetism, &c.—not then (1842) supposed, except by me, to be relations of necessity, and not even now supposed to be inseparable in idea.

The application of the word has latterly been much extended, and we hear of correlation of growth, correlation of diseases, correlation of sciences, &c. I rather regret this; there is nothing of greater importance, especially for works on physical science, than accuracy, as far as may be, in the use of words: perfect accuracy is impossible.

Mr. Barrett has, I think, extended the import of the word beyond reasonable limits. There is no correlation between colour and music, further than there is a correlation between anything and everything. The word "analogy," used also by Mr. Barrett, is, in my humble judgment, far more accurate as applied to the classes of phenomena he treats of. I hope he will excuse a "parent" when complaining of ill-treatment to his "offspring," although the offspring may have had a little congenital deformity.

January 22 W. R. GROVE

Rainbow Colours

I AM reminded by Mr. Grove's statement at p. 314 (that he has seen three repetitions of the spectrum within the primary) of a splendid rainbow, which I saw at the Falls of the Handeck, near Meyringen, last summer.

The sun was very bright, about midday, and looking down at the Fall there appeared the most beautiful rainbow I ever saw. The colours were intense, probably from the spray being in fine drops; and I observed between the primary and secondary, *i.e.* between the two violets, a band of *a fine rich brown colour*.

I have often observed when rainbows are bright, that there is a dark band of a neutral tint between the two. This effect was shown very beautifully in a drawing by Mr. Alfred W. Hunt, exhibited at the Water Colour Society two or three years ago. He appears to have seen the same effect, but I had never seen the rich brown colour before. It was no effect of background, for when I varied my position the brown moved with the bows.

I have also often seen four or five, what may be called tertiary bows, inside the primary. They are grouped together as it were, and form a band of alternate red and green, becoming fainter as they recede from the primary. They appear to be a repetition of the primary in which the red and green are the most prominent colours.

Gateshead, January 23 R. S. NEWALL

Cuckows' Eggs

WILL you kindly allow me space to thank Prof. Newton for the trouble he has taken in replying to my inquiries, although I must confess I am still unconvinced?

My omission of the name of the eminent oölogist in my last letter was entirely accidental, for I had no purpose in concealing it, but rather the reverse. My quotation was from a letter of Mr. Hewitson's, in the *Field* of March 17, 1868.

Mr. Newton mentions the eggs of the Black Cap Warbler and the Tree Pipit, as some indication of the existence of a condition which I doubted in my sixth question. I have not found the eggs of the Black Cap vary more than this, that in some the ground colour was of a warmer tone than in others. The eggs of the Tree Pipit, I freely admit, do vary greatly, but their variations are all confined to different shades of nearly the *same colour*—viz., purple; ranging from purplish red on the one side, to bluish purple on the other, but these variations have, nevertheless, so much similitude that there is no difficulty in at once recognising them.

Mr. Newton says: "If the eggs in question were not cuckows', what birds laid them?" My reply is, simply, that they were laid by the birds in whose nests they were found. It seems to me far more likely that an egg laid by a certain bird should vary slightly from the rest of her eggs in the same nest, than that another species should lay eggs varying to the extent mentioned by Dr. Baldamus—viz., from vinous red to greenish blue, olive green, plain brown, &c., or even pure white, or light blue green, mentioned by Degland and Gerbe, as quoted by Mr. Newton.

Mr. Newton will excuse me for saying that I did not refer to the German authors mentioned by him in the footnote to his letter, excepting where quoted by Dr. Baldamus, for unfortunately I do not possess a knowledge of the German language, and am therefore unacquainted with their writings.

The doubts I have expressed, and still feel, have nothing personal in them, but only apply to the theory and the evidence on which it is supported. It does seem to me singular that these extreme variations of colour in the eggs of the cuckoo should only have been remarked in Germany. They do not appear to have been observed in Britain. Mr. Newton does not say he has found them himself, and admits that the evidence on which these German eggs are pronounced cuckoos' might have been more satisfactory. Mr. Hewitson says "few eggs differ less," and Mr. Dawson Rowley has remarked, in a letter to the *Field*, "I believe few men have taken with their own hands so many eggs of *cuculus canorus* as myself;" and yet his experience does not confirm the theory, but the contrary.

I cannot help feeling that we still want more positive information on this point. Were all the varied eggs alleged to be cuckoos' really laid by that bird? I can easily conceive an enthusiastic naturalist, with a favourite theory to maintain, imagine when he takes out of the nest of the hedge-sparrow, or tree pipit, an egg rather larger than the rest, but marked and coloured in a similar manner, that it is that of the cuckoo. I hold, however, that nothing less than *positive proof* that it was deposited by a cuckoo will suffice. I admit this may be difficult to obtain, but it is not the less necessary. A dogma like the one in question must be based on evidence that is not only unimpeachable, but above suspicion, and this I think the advocates of the theory have not yet furnished.

May I ask you to be good enough to allow my orthography of the word "cuckoo" to remain? With all deference to so high an authority as Prof. Newton, I prefer and always use the common mode of spelling the word to the one adopted by him, as better representing the call-note, from which the name is derived.

W. J. STERLAND

January 17

Dr. Livingstone's Discoveries

IN the conclusion of a letter which has lately appeared in your journal on the subject of Dr. Livingstone's recent letters, Dr. Beke gives the opinion that the river and lake chain which forms the main part of the great traveller's latest discoveries, is the head stream of the Nile. Though I am unwilling to differ from such an authority as Dr. Beke, yet there appear to me to be considerable difficulties in the way of his conclusions.

Will you allow me to show how it seems equally, if not more probable, that Dr. Livingstone, whilst he has ascertained the sources of the Nile, has also the merit of being the discoverer of the head streams of one of the great rivers which flow to the Atlantic, perhaps of the Congo. The Chambeze, the head stream of the lake chain in question, has its rise somewhere in the eastern part of the great plateau or ridge which skirts the whole side of Africa, next the Indian Ocean. Dr. Livingstone crossed it in lat. 10° 34′ south; from this it flows first westwards to Lake Bangweolo, then north to Lake Moero. The position of Lake Moero can only be determined as yet by reference to that of Lunda, the capital of the kingdom of the Cazembe, twelve miles beyond which town the lake is said to begin. Portuguese travellers are the only Europeans who are known to have previously visited this town, and the two routes from which we can assign it a position on the map, are those of Dr. Lacerda in 1798, and of Major Monteiro in 1831. These two travellers, with their escorts, have passed over almost the same route from Tete on the Zambesi to the Cazembe. From the former traveller there remain two astronomically fixed positions in the middle of this route, and the latter has published a volume which contains the distances and directions of his journey, but no astronomical positions. The route of Monteiro then, justified by the now ascertained position of Tete at the beginning, and by the positions formerly determined by Lacerda for its middle course, gives the place of the Cazembe town of Lunda, at its termination, in lat. 8° 40′ S., lon. 28° 20′ E.

Dr. Livingstone describes Lake Moero as beginning twelve miles below this position and extending for fifty miles to northward. Since he proceeded north from Cazembe town along the eastern shore of Lake Moero, in his attempt to reach Ujiji in the end of 1867, the great bulk of this lake must lie to westward of the meridian of Lunda. The centre of Moero would then be in the latitude of the south end of Tanganyika, and at about 120 miles to westward of its longitude. Dr. Livingstone has seen the river at its outflow from this lake and also at the point where it emerged from the "crack in the mountains of Rua," when, according to his own observation, the river turned to *north-north-west* to form Ulenge, a third lake or marsh in the country west of Tanganyika.

This north-north-westerly direction would carry this river quite out of the line of Tanganyika or of the Albert Nyanza; besides, both of these lakes appear to be closed in on the western side by high mountains.

The levels of the river also appear to present a great obstacle to its joining the Nile lakes.

Leaving the Valley of the Loangwa, Dr. Livingstone tells us that he ascended to a great plateau which extends for 350 miles square, southward of Tanganyika. This table-land is at an elevation of from 3,000 to 6,000 feet above the sea. The valley of the Chambeze crosses this plateau from east to west, and the river descends from it into the great valley of the Lakes Bangweolo and Moero, not far west from the point where it was crossed by Dr. Livingstone. The valley of the Chambeze is no doubt one of the greatest hollows in this plateau, and so the bed of the river here may be taken to be at the lowest general height of the plateau given by Dr. Livingstone—that is, 3,000 feet, or 200 feet above the Tanganyika. From the point at which the Chambeze was crossed, its course is for perhaps 200 miles westward to Lake Bangweolo, and in this part of its flow from the plateau to the valley the fall of the river must be considerable. Between Bangweolo and Moero the course of perhaps 120 miles to northward seems to be through a more level part of the valley. Still, here there must be another descent to Lake Moero. According to the Portuguese traveller, Monteiro, the kingdom of the Cazembe extends on the east and north-east to the land of the Auembas, apparently the same as the Luwemba of Burton and Speke on the south-east of Tanganyika. His country is described as low and flat, and this would seem to be confirmed by the absence of current in the marshy rivers visited by the Portuguese to the east of Cazembe's town, and also by the Lake Liemba of Dr. Livingstone, which he has found to be the termination of a long river-like arm of Tanganyika, stretching south-south-east to the north edge of the before-mentioned plateau. Lake Moero, then, cannot be above the level of Tanganyika, else its outflow would surely be over this level country, and not through the mountains to northward. From Lake Moero the river flows on through a "rent in the Mountains of Rua." In passing through this gorge, it appears certain that the river must have a further and rapid descent, lowering its bed still more beneath the level of Tanganyika.

In his letter of 30th May, 1869, from Ujiji, which has the brevity of a telegram, Dr. Livingstone says: "Tanganyika, Nyige Chowambe (Baker's) are one water, and the head of it is 300 miles to south of this. The western and central lines of drainage converge into an unvisited lake west or south-west of this." If the expression "one water" here means that these two lakes are united by an extension of one into the other, and not by a river, then it is evident that the river and lake chain under consideration can never flow up to join either of them after having passed down through the rent in the Mountains of Rua; if it means that these lakes are joined together by a river, still the small difference in height between that computed for Lake Tanganyika by Mr. Finlay, of 2,800 feet (afterwards so curiously confirmed by Livingstone's height of Lake Liemba), and that found for the Albert Nyanza by Baker, would not give a sufficient lowness to the latter lake to allow this river to flow down to it through the five degrees of latitude which separate its outfall from the Mountains of Rua, from the southern end of the Albert Lake. Dr. Livingstone's statement in his letter above quoted from Ujiji, that the head waters of the Tanganyika and Albert Lakes are 300 miles south of that place, is not at all opposed to the view that the Chambeze River and its lake chain may join the Congo, for the streams which flow into his Lake Liemba may rise at this distance from Ujiji. In this case the sources of the Nile would be side by side with those of the Congo; and the man who has the claim to be called the greatest explorer that the world has ever known, has the double honour of having solved the two greatest of African problems.

74, Strand, W.C. KEITH JOHNSTON, Jun.

Physical Meteorology

ASSUMING with your correspondent that there is an ascending current in the heart of a cyclone, no doubt latent heat will play its part. I presume, however, your correspondent does not imagine that air is ever actually heated by such means to 370° F.

Suppose, for instance, that two cubic feet of saturated air, both at thirty inches pressure, but one at the temperature 32° F. and the other at the temperature 90° F., become mixed. The cubic foot at 32° F. will contain 2·37 grains of vapour, that at 90° F. 14·50 grains. Hence, after mixture the average weight of vapour in unit of volume will be 8·43 grains. This would saturate a temperature = 71·7° F. But this is greater than the mean between the two temperatures or 61° F. There will, therefore, be hardly enough heat to keep the mixture at 71°·7 F and prevent deposition.

On the other hand, we cannot imagine the temperature of the mixture to fall as low as 61° F.

The temperature of the mixture will therefore, I presume, be greater than 61° and less than 71·7°.

B. STEWART

Veined Structure in Ice

FEW men have had better opportunities of examining glacial phenomena than Mr. Whymper, and his explanation of the veined structure is certainly an ingenious one. I venture, however, to doubt whether it can be regarded as generally satisfactory, although, possibly, it might explain some isolated cases.

The following, which, so far as my experience goes, are common facts in glaciers, appear to me difficult to reconcile with his explanation.

(1.) One common case in which the veined structure becomes conspicuous is after the glacier has been pressed into a narrower channel than has been occupied by its *névé*. The structure planes are then roughly parallel to the *sides* of the channel. Dr. Tyndall has pointed this out in his "Glaciers of the Alps," p. 387, and I have frequently observed the same thing myself. Three instances occur to me at this moment: one on the Gorner Glacier, under the Gorner Grat; another in the middle part of the Glacier de la Pilatte (Dauphine); a third on the upper part of the Mer de Glace. Did I search through my note-book I have no doubt I could find plenty more. If now, say in the second example, the veined structure was due to the crevasses in the ice fall below the Col du Sélé, surely its planes would hardly be twisted through a right angle in the comparatively short distance intervening between the ice fall and the rocky spur from the Crête des Bœufs Rouges which causes the "nip." Moreover, if the planes have been turned by the unequal motion of the centre and sides of the ice stream, ought we to find them so uniform in direction as they now are, often extending with a very general parallelism over the greater part of the glacier?

(2.) If the veined structure is the result of healed crevasses, how are we to explain the great number of these plates of different coloured ice on glaciers which are not remarkable for very numerous crevasses. For example, on the Roseg Glacier, near Pontresina, these plates of blue and white ice alternate with each other for at least several hundred yards as you walk up the glacier, and are commonly only an inch or so thick. I have in my notebook a diagram of a piece to exhibit the weathering of the two kinds of ice, in which are shown five plates, three blue and two white. One of the former is about an inch thick, and all the rest are thinner. Each of the white is about half an inch, and I remember that this was a fair sample of most of the ice near. If, then, the crevasses, from whose healing this platy structure has resulted, were formed simultaneously or in close succession, how are we to explain the thinness of the white portion, its layers being, if anything, thinner than the blue? Crevasses are not usually so near together as this, and if they were not thus formed is it probable that the plates would be so produced as to be, for about as far as one could trace them, parallel one to another, so accurately that my diagram looks like a bit from a cliff of midland lias?

Cambridge, Jan. 7 T. G. BONNEY

Personal Equation of Astronomical Observers

IN the number for November 18, 1869, of NATURE, "J." asks if an experiment has hitherto been tried to ascertain the value of the personal equation of astronomical observers. The fact is, that it has been tried in different manners, as by Mr. Wolf in Paris, and Mr. Hirsch in Neuchatel, but first of all by Dr. F. Kaiser, Astronomical Professor, and Director of the Observatory of Leiden. The apparatus of Prof. Kaiser was first constructed in 1851, but was afterwards highly improved, so that it is fitted equally well for observations with or without the chronograph.

A description of the method and apparatus of M. Kaiser is to be found in the "Archives Néerlandaises des Sciences exactes et Naturelles," vol. i. p. 194, and of the improved one in the reports and communications of the Royal Academy of Science of the Netherlands (Verslagen en mededeelingen der Koninklijke Academie van Wetenschappen), Second Series, vol. ii.; the former is written in French, and titled: "Sur la détermination absolue de l'erreur personelle dans les observations astronomiques;" the latter, in German: "Uebereinen neuen Apparat zur absoluten Bestimmung von persönlichen Fehlern bei astronomischen Beobachtungen." H. VON DE STADT, Ph.D.

Arnheim, Netherlands, January 3

Anatomical Lectures to Female Medical Students

I HAVE great pleasure in hearing that the Professors of Anatomy in Scotland have not all forgotten that women ought to be treated with some degree of chivalry.

Professor Struthers, of Aberdeen, and Professor Bell, of St. Andrews, hearing that the five ladies who are studying at the Edinburgh University are excluded from the opportunity of studying anatomy there, have severally offered their services as instructors. Many a lady will rejoice that the numbers of those willing and ready to help in the good cause of fuller knowledge for women are increased by two professors, who have bravely come forward with much moral courage and chivalrous feeling.

Edinburgh, Jan. 22. A NON-MEDICAL WOMAN

NOTES

THE Physical Section of the Academy of Sciences at their last meeting recommended Professor Kirchhoff, of Heidelberg, to fill the place of correspondent of the section, vacant by the death of Principal Forbes. The other candidates were MM. Ångström, Billet, Dove, Grove, Henry (of Philadelphia), Jacobi, Joule, Lloyd (of Dublin), Riess, Stokes, Tyndall, Volpicelli, and Sir William Thomson.

IN our statement last week that "the Senate of London University has proposed to establish a Faculty of Science," "London University" should have been "University College, London;" the fact being that the Senate of the University of London—in advance of every other university of the kingdom—established a Faculty of Science *ten years ago;* constituting, at the suggestion, and with the advantage of the advice, of the ablest men of science in this country, a scheme for graduation in science, which has continued in efficient operation from that time to the present. And we may add that in the new building of the University the four Faculties—Arts, Sciences, Law, and Medicine—are typified by the four sitting statues over the portico, representing Milton, Newton, Bentham, and Harvey.

WE are happy to be able to announce that the council of the Chemical Society has decided to have a report of their proceedings and an abstract of the papers read before the society drawn up immediately after its meetings, and to offer copies of this report to the editors of journals who may be likely to wish to publish it. The days when the newest results of science were regarded as something secret—or, at all events, of no concern to the ordinary man of education—are gone by, we trust, for ever. It may now be confidently expected that the example of the Chemical Society in thus seeking a wide publicity for the reports of their proceedings will be followed by those societies—such as the Linnean and the Astronomical—which have not offered hitherto such facilities to thosewho endeavour to inform the general scientific public and the world of letters of the latest researches in science.

WE learn from the fifth annual report of the Sanitary Commissioner with the Government of India, just received, that the first scientific report on the inquiry into epidemic cholera in India, the instructions for which were prepared by the Army Sanitary Commission, has been presented. The reporters state that they have been making "careful and systematic examinations of cholera excreta, and the changes taking place in them during decomposition as compared with healthy excreta, and the changes occurring in them as well as in other fluids and solids during the same process. These changes have been studied as occurring under various circumstances, associated with various substrata and media. In addition to the above experiments, others on the effects of cholera excreta on growing rice plants have been entered upon. Careful daily observations have been made, and notes and *camera lucida* drawings of all the changes observed to occur have been accumulated. As far as the observations have as yet gone, they have not been confirmatory of those of Hallier. For, though fungi have frequently appeared on choleraic materials, yet—(1) several species have appeared; (2) the same species have occurred in abundance on other substrata in like circumstances; (3) the species observed have not belonged to the cholera series of Hallier. As yet, however, it would be premature to draw any definite conclusions in the matter, as any series of observations on such points is beset with innumerable difficulties and fallacies, necessitating careful and frequent repetition of each experiment before coming to a final decision as to the value of its results." Observations are being conducted at various stations to ascertain whether Pettenkofer's theory of the relation of cholera to subsoil water level is borne out in India.

THE Boston Society of Natural History, at its last meeting, passed the following vote:—

"That the net proceeds of the celebration of the centennial anniversary of the birth of Humboldt, together with the money received from the sale of Prof. Agassiz's Address, previous to Jan. 1, 1870, and the money subscribed at the solicitation of the society's committee, be given to the trustees of the Museum of Comparative Zoölogy at Harvard College, in trust, for the establishment of an endowment, under the title of the Humboldt Scholarship, the income of which is to be solely applied, under the direction of the Faculty, toward the maintenance of one or more young and needy persons engaged in study at said museum."

THE subject of the Ravizza prize of a thousand lire for 1870 is, the effect which emigration to foreign countries and removal to cities produce on the population of agricultural districts. Manuscripts are to be marked with a motto, and accompanied by a sealed letter containing the author's name. They must be written in Italian, addressed *Presidenza del Regio Liceo, Cesare, Beccaria, Milan*, and delivered not later than the last day of December next.

A MONTHLY Journal, devoted to social and sanitary economy, is advertised to appear on the 1st proximo, under the title of the *Food Journal*. Judging by the names included in the published list of contributors, we may confidently expect that the important subjects to be dealt with in this periodical will be treated of with ability, and in accordance with the most recent results of scientific research.

THAT interesting and useful periodical the *American Naturalist*, which is devoted to the popularisation of Natural History, commences a new volume in March next. The first article in the volume will be an illustrated paper by Mr. E. G. Squier, the eminent archæologist, on the Ancient Megalithic Monuments of Peru compared with those in other parts of the world. The second article will be on Sponges, by Prof. Leidy, of Philadelphia.

WE have received from the Mannheim Association for Natural Science the annual report published in February of last year, and giving an account of the society's operations during the year 1868-9. The usefulness of the Association appears to have been somewhat limited by the want of funds. To the same cause must doubtless be ascribed the fact that the only papers published with the report before us are on the meteorology of Mannheim. We are glad, however, to learn from the Secretary that the volume for 1870 will shortly appear, and that it is to contain several interesting astronomical, meteorological, and botanical communications.

M. DUMERIL, Member of the Institute and Professor at the Jardin des Plantes, commenced on the 15th inst. at the Museum of Natural History, a course of lectures on the general history of reptiles, batrachians, and fish.

MR. DYER, of Cirencester College, has been appointed by Earl De Grey to the Professorship of Botany in the Royal College of Science, Dublin.

IT was some time since announced that the prize offered by Lieut.-Colonel Scott, R.E., the secretary of the Royal Horticultural Society, for an essay on the Principles of Floral Criticism, would be awarded on January 19th. It is now stated that the award will not be made till Wednesday, May 4th, 1870.

WE note the appearance of a new edition of the very handy geological map of Germany, France, England, and the neighbouring countries, originally drawn up by Von Dechen in 1839. It will be found very useful to any continental tourists who have some geological knowledge, and who care to take an intelligent survey of the countries they travel through. Copies may be obtained of Messrs. Nutt, 270, Strand. The scale is 1:2,500,000.

A COMMITTEE has been formed at Liverpool for the purpose of establishing a Zoölogical Society. It is hoped that the corporation will grant a site for the society's garden in one of the public parks.

MELBOURNE has recently acquired a fresh utility and ornament in the shape of a turret-clock, the first manufactured in the colony, which for perfection of work and peculiarities of construction challenges the admiration of all horologists. The dials, six feet in diameter, consisting of frames of cast-iron—the rings, figures, and minute marks (eight inches long and one broad) all formed in one casting—are eighty feet from the level of Bourke Street. The weight is about 120 pounds, suspended on a barrel seventeen inches long and ten inches in diameter, revolving twenty-nine times a week, giving a downfall of about seventy feet. The pendulum swings once in two seconds, and consists of a dry and varnished pine rod fifteen feet six inches long, with a cylinder of lead weighing 320 pounds. As it was thought desirable to make the hands move by easily seen impulses every half-minute, so as to ascertain the time to a second, a special arrangement, called a spring *remontoire*, has been added, by means of which the wheels of the clock are detained for thirty seconds and escape at the half-minute, allowing the weight to move the hands over half a minute's space, and wind up a small clock-spring in the spindle, which revolves in two minutes. This keeps the pendulum and the escapement going while the rest of the works are held back. The following is a technical description of this *remontoire*:—"The wheel in the spindle, which revolves once in five minutes, drives a second pinion with eight teeth, the spindle of which projects through the back of the clock frame, and carries a little cylinder with two notches at the end, one broad and shallow, the other narrow and deep. At the end of the *remontoire* arms are two steel projections, one of which passes through the broad notch and the other through the narrow one. As the two-minute spindle revolves, with the notched cylinder, it brings a notch in a right position to let one of the arm projections through every half-minute, allowing the train to move the hands, and wind up the small spring. To effect this the pinion with sixteen leaves is loose on the two-minute spindle, and is only attached to one end of the spring. The clock is also supplied with Denison's double three-legged gravity escapement.

THE Mount Washington Railway, which ascends the White Mountains, New Hampshire, U.S., is about three miles in length, the average gradient being a little more than 1 in 4, which is increased in some places to the extraordinary extent of 1 in 3. The engine draws itself up the line by means of a "pinion," which works into a strong "rack" fixed between the rails, and the ascent of three miles is completed in about one hour.

M. MOHN, Director of the Observatory of Christiania, has recently surveyed the *névé* field of Fostedalsbrœn, which occupies 750 square kilometres. He finds that it feeds twenty-two glaciers of the first order and more than 200 smaller ones. The *névé* is seventy kilometres from the sea.

MESSRS. BELL AND DALDY have just issued the first part of a work bearing the title "Natural Phenomena and Chronology of the Seasons," and containing a chronological register of the remarkable frosts, droughts, thunderstorms, gales, floods, earthquakes, &c. which have occurred in the British Isles since A.D. 220. The author, Mr. E. J. Lowe, F.R.S., the well-known meteorologist, wishes it to be understood that his chief object in publishing what is confessedly a very imperfect record is to call attention to the subject, and elicit further information for a more extensive work, embracing the more remarkable natural phenomena of foreign countries. The compilation of a catalogue of this nature, if it is to be of any real benefit to science, involves an enormous amount of labour and much critical skill. The original authorities for the phenomena recorded should, in each case, be referred to so precisely that the quotations may be readily verified. It is hardly satisfactory to see the *Preston Herald* quoted in support of the assertion that 1,500 houses were unroofed and destroyed in London, in the year 944.

MR. JOHN H. MARTIN, secretary to the Maidstone and Mid-Kent Natural History Society, has just brought out the first number of a new publication, "Microscopic Objects figured and described," containing 16 wood-cuts and short descriptions of vegetable objects, from the yeast-plant to the spiral-vascular tissue from garden rhubarb. The work is intended, when complete, to contain about 200 figures, all of them original, to be issued in monthly numbers. It is proposed to commence with the primary forms of vegetable life, and to proceed onwards through the tissues to the woody structures of the Exogens and Endogens, next descending to the Acrogens, and so passing to the extreme limits of vegetable life, as the Desmidiæ, &c., thence to the lower forms of animal life, the Infusoria, and on through the Radiata to the Insects, which will be drawn and described in their various orders, and the minute organs figured separately.

THE *Architect* states that Lieutenant Cole, R.A., and three sappers, sent out by the Secretary of State for India to take casts of the Sanchi Tope, have arrived in Calcutta. For the benefit of those of our readers who have not had the privilege of seeing Mr. Fergusson's magnificent work "On Tree and Serpent Worship," we may mention that the Sanchi Tope, a monument of very high antiquity, is surrounded by walls and gateways covered with elaborate sculptured decorations of the greatest interest to the student of the early history of the human race.

THE *Field* of Saturday last contains some interesting notes on special agricultural training-schools in France, Germany, and Switzerland. At Rütte in the Canton of Berne, and at Santhoven in Bavaria, particular attention is given to the theory and practice of dairy operations, and the general treatment of cow stock. The school of Lézardeau, on the estate of Count Conédic, in the Department of Finisterre, offers special facilities for the study of draining and irrigation. In this school there is a technical library, a museum, a collection of meteorological instruments, a laboratory, and tools of all descriptions. The general course of study includes elementary mechanics, agricultural chemistry and botany, the pruning and grafting of fruit-trees, the making of roads, and other practical knowledge. At Görtz, in Austria, is a special institution for silkworm culture, supporting a journal exclusively devoted to that branch of industry. The *Athenæum* states that the Ottoman Government is giving its support to a project of M. Netter, of Constantinople, to found an agricultural school for Jews in Palestine.

We have been requested to notify that the following premiums have been placed at the disposal of the Council of the Society for the Encouragement of Arts, Manufactures, and Commerce, for the term of seven years, by M. Septimus Piesse:—1. A premium of £5, for one pound of Otto of Bergamot, of the value of 16*s*. or more in the London market, being the produce of plants (*Citrus bergamia*) grown in Australia, New Zealand, Natal, any of the British West India Islands, or any other British Colony or Dependency. 2. A premium of £5, for one ounce of Otto of Roses, of the value of 20*s*. or more in the London market, being the produce of any variety of roses grown together in one plantation in Australia, New Zealand, Natal, any of the British West India Islands, or any other British Colony or Dependency. 3. A premium of £10, for a canister of Enflowered Butter or Fat, so scented with any kind or sort of flower, either by infusion or enfleurage, or by means of these processes jointly, of the weight of 3 lbs. or more, and of the value of 6*s*. per lb. in London. The said butter or fat to be enflowered or infused with flowers grown for the purpose in Australia, New Zealand, Natal, any of the British West India Islands, or any other British Colony or Dependency.

ON HAZE AND DUST

SOLAR light in passing through a dark room reveals its track by illuminating the dust floating in the air. "The sun," says Daniel Culverwell, "discovers atomes, though they be invisible by candle-light, and makes them dance naked in his beams."

In my researches on the decomposition of vapours by light, I was compelled to remove these "atomes" and this dust. It was essential that the space containing the vapours should embrace no visible thing; that no substance capable of scattering the light in the slightest sensible degree should, at the outset of an experiment, be found in the "experimental tube" traversed by the luminous beam.

For a long time I was troubled by the appearance there of floating dust, which, though invisible in diffuse daylight, was at once revealed by a powerfully condensed beam. Two tubes were placed in succession in the path of the dust: the one containing fragments of glass wetted with concentrated sulphuric acid; the other, fragments of marble wetted with a strong solution of caustic potash. To my astonishment it passed through both. The air of the Royal Institution, sent through these tubes at a rate sufficiently slow to dry it and to remove its carbonic acid, carried into the experimental tube a considerable amount of mechanically-suspended matter, which was illuminated when the beam passed through the tube. The effect was substantially the same when the air was permitted to bubble through the liquid acid and through the solution of potash.

Thus, on the 5th of October, 1868, successive charges of air were admitted through the potash and sulphuric acid into the exhausted experimental tube. Prior to the admission of the air the tube was *optically empty;* it contained nothing competent to scatter the light. After the air had entered the tube, the conical track of the electric beam was in all cases clearly revealed. This indeed was a daily observation at the time to which I now refer.

I tried to intercept this floating matter in various ways; and on the day just mentioned, prior to sending the air through the drying apparatus, I carefully permitted it to pass over the tip of a spirit-lamp flame. The floating matter no longer appeared, having been burnt up by the flame. It was therefore *organic matter*. When the air was sent too rapidly through the flame, a fine blue cloud was found in the experimental tube. This was the *smoke* of the organic particles. I was by no means prepared for this result; for I had thought, with the rest of the world, that the dust of our air was, in great part, inorganic and non-combustible.

Mr. Valentin had the kindness to procure for me a small gas-furnace, containing a platinum tube, which could be heated to vivid redness. The tube also contained a roll of platinum gauze, which, while it permitted the air to pass through it, ensured the practical contact of the dust with the incandescent metal. The air of the laboratory was permitted to enter the experimental tube, sometimes through the cold, and sometimes through the heated tube of platinum. The rapidity of admission was also varied. In the first column of the following table the quantity of air operated on is expressed by the number of inches which the mercury gauge of the air-pump sank when the air entered. In the second column the condition of the platinum tube is mentioned, and in the third the state of the air which entered the experimental tube.

Quantity of Air.	State of Platinum Tube.	State of Experimental Tube.
15 inches	Cold	Full of particles.
15 ,,	Red-hot	Optically empty.
15 ,,	Cold	Full of particles.
15 ,,	Red-hot	Optically empty.
15 ,,	Cold	Full of particles.
15 ,,	Red-hot	Optically empty.

The phrase "optically empty" shows that when the conditions of perfect combustion were present, the floating matter totally disappeared. It was wholly burnt up, leaving not a trace of residue. From spectrum analysis, however, we know that soda floats in the air; these organic dust particles are, I believe, the *rafts* that support it, and when they are removed it sinks and vanishes.

When the passage of the air was so rapid as to render imperfect the combustion of the floating matter, instead of optical emptiness a fine blue cloud made its appearance in the experimental tube. The following series of results illustrate this point:—

Quantity.	Platinum Tube.	Experimental Tube.
15 inches, slow	Cold .	Full of particles.
15 ,, ,,	Red-hot	Optically empty.
15 ,, quick	,,	A blue cloud.
15 ,, ,,	Intensely hot	A fine blue cloud.

The optical character of these clouds was totally different from that of the dust which produced them. At right angles to the illuminating beam they discharged perfectly polarised light. The cloud could be utterly quenched by a transparent Nicol's prism, and the tube containing it reduced to optical emptiness.

The particles floating in the air of London being thus proved to be organic, I sought to burn them up at the focus of a concave reflector. One of the powerfully convergent mirrors employed in my experiments on combustion by dark rays was here made use of, but I failed in the attempt. Doubtless the floating particles are in part transparent to radiant heat, and are so far incombustible by such heat. Their rapid motion through the focus also aids their escape. They do not linger there sufficiently long to be consumed. A flame it was evident would burn them up, but I thought the presence of the flame would mask its own action among the particles.

In a cylindrical beam, which powerfully illuminated the dust of the laboratory, was placed an ignited spirit-lamp. Mingling with the flame, and round its rim, were seen wreaths of darkness resembling an intensely black smoke. On lowering the flame below the beam the same dark masses stormed upwards. They were at times blacker than the blackest smoke that I have ever seen issuing from the funnel of a steamer, and their resemblance to smoke was so perfect as to lead the most practised observer to conclude that the apparently pure flame of the alcohol lamp required but a beam of sufficient intensity to reveal its clouds of liberated carbon.

But is the blackness smoke? The question presented itself in a moment. A red-hot poker was placed underneath the beam, and from it the black wreaths also ascended. A large hydrogen flame was next employed, and it produced those whirling masses of darkness far more copiously than either the spirit-flame or poker. Smoke was therefore out of the question.

What, then, was the blackness? It was simply that of stellar space; that is to say, blackness resulting from the absence from the track of the beam of all matter competent to scatter its light. When the flame was placed below the beam the floating matter was destroyed *in situ;* and the air, freed from this matter, rose into the beam, jostled aside the illuminated particles, and substituted for their light the darkness due to its own perfect transparency. Nothing could more forcibly illustrate the invisibility of the agent which renders all things visible. The beam crossed, unseen, the black chasm formed by the transparent air, while at both sides of the gap the thick-strewn particles shone out like a luminous solid under the powerful illumination.

But here a difficulty meets us. It is not necessary to burn the particles to produce a stream of darkness. Without actual combustion, currents may be generated which shall exclude the floating matter, and therefore appear dark amid the surrounding brightness. I noticed this effect first on placing a red-hot copper ball below the beam, and permitting it to remain there until its temperature had fallen below that of boiling water. The dark currents, though much enfeebled, were still produced. They may also be produced by a flask filled with hot water.

To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a voltaic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current the temperature of the wire was gradually augmented, but before it reached the heat of ignition, a flat stream of air rose from it, which when looked at edgeways appeared darker and sharper than one of the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air. What is the explanation? Simply this. The hot wire rarefied the air in contact with it, but it did not equally lighten the floating matter. The convection current of pure air therefore passed upwards *among the particles*, dragging them after it right and left, but forming between them an impassable black partition. In this way we render an account of the dark currents produced by bodies at a temperature below that of combustion.

Oxygen, hydrogen, nitrogen, carbonic acid, so prepared as to exclude all floating particles, produce the darkness when poured or blown into the beam. Coal-gas does the same. An ordinary glass shade placed in the air with its mouth downwards permits the track of the beam to be seen crossing it. Let coal-gas or hydrogen enter the shade by a tube reaching to its top, the gas gradually fills the shade from the top downwards. As soon as it occupies the space crossed by the beam, the luminous track is instantly abolished. Lifting the shade so as to bring the common boundary of gas and air above the beam, the track flashes forth. After the shade is full, if it be inverted, the gas passes upwards like a black smoke among the illuminated particles.

The air of our London rooms is loaded with this organic dust, nor is the country air free from its pollution. However ordinary daylight may permit it to disguise itself, a sufficiently powerful beam causes the air in which the dust is suspended to appear as a semi-solid rather than as a gas. Nobody could, in the first instance, without repugnance place the mouth at the illuminated focus of the electric beam and inhale the dirt revealed there. Nor is the disgust abolished by the reflection that, although we do not see the nastiness, we are churning it in our lungs every hour and minute of our lives. There is no respite to this contact with dirt; and the wonder is, not that we should from time to time suffer from its presence, but that so small a portion of it would appear to be deadly to man.

And what is this portion? It was some time ago the current belief that epidemic diseases generally were propagated by a kind of malaria, which consisted of organic matter in a state of *motor-decay;* that when such matter was taken into the body through the lungs or skin, it had the power of spreading there the destroying process which had attacked itself. Such a spreading power was visibly exerted in the case of yeast. A little leaven was seen to leaven the whole lump, a mere speck of matter in this supposed state of decomposition being apparently competent to propagate indefinitely its own decay. Why should not a bit of rotten malaria work in a similar manner within the human frame? In 1836 a very wonderful reply was given to this question. In that year Cagniard de la Tour discovered the *yeast plant*, a living organism, which when placed in a proper medium, feeds, grows, and reproduces itself, and in this way carries on the process which we name fermentation. Fermentation was thus proved to be a product of life instead of a process of decay.

Schwann, of Berlin, discovered the yeast plant independently, and in February 1837 he also announced the important result, that when a decoction of meat is effectually screened from ordinary air, and supplied solely with air which has been raised to a high temperature, putrefaction never sets in. Putrefaction, therefore, he affirmed to be caused by something derived from the air, which something could be destroyed by a sufficiently high temperature. The experiments of Schwann were repeated and confirmed by Helmholtz and Ure. But as regards fermentation, the minds of chemists, influenced probably by the great authority of Gay-Lussac, who ascribed putrefaction to the action of oxygen, fell back upon the old notion of matter in a state of decay. It was not the living yeast plant, but the dead or dying parts of it, which, assailed by oxygen, produced the fermentation. This notion was finally exploded by Pasteur. He proved that the so-called "ferments" are not such; that the true ferments are organised beings which find in the reputed ferments their necessary food.

Side by side with these researches and discoveries, and fortified by them and others, has run the *germ theory* of epidemic disease. The notion was expressed by Kircher, and favoured by Linnæus, that epidemic diseases are due to germs which float in the atmosphere, enter the body, and produce disturbance by the development within the body of parasitic life. While it was still struggling against great odds, this theory found an expounder and a defender in the President of this Institution. At a time when most of his medical brethren considered it a wild dream, Sir Henry Holland contended that some form of the germ theory was probably true. The strength of this theory consists in the perfect parallelism of the phenomena of contagious disease with those of life. As a planted acorn gives birth to an oak competent to produce a whole crop of acorns, each gifted with the power of reproducing its parent tree, and as thus from a single seedling a whole forest may spring, so these epidemic diseases literally plant their seeds, grow, and shake abroad new germs, which, meeting in the human body their proper food and temperature, finally take possession of whole populations. Thus Asiatic cholera, beginning in a small way in the Delta of the Ganges, contrived in seventeen years to spread itself over nearly the whole habitable world. The development from an infinitesimal speck of the virus of small-pox of a crop of pustules, each charged with the original poison, is another illustration. The reappearance of the scourge, as in the case of the *Dreadnought* at Greenwich, reported on so ably by Dr. Budd and Mr. Busk, receives a satisfactory explanation from the theory which ascribes it to the lingering of germs about the infected place.

Surgeons have long known the danger of permitting air to enter an open abscess. To prevent its entrance they employ a tube called a cannula, to which is attached a sharp steel point called a trocar. They puncture with the steel point, and by gentle pressure they force the pus through the cannula. It is necessary to be very careful in cleansing the instrument; and it is difficult to see how it can be cleansed by ordinary methods in air loaded with organic impurities, as we have proved our air to be. The instrument ought, in fact, to be made as hot as its temper will bear. But this is not done, and hence, notwithstanding all the surgeon's care, inflammation often sets in after the first operation, rendering necessary a second and a third. Rapid putrefaction is found to accompany this new inflammation. The pus, moreover, which was sweet at first, and showed no trace of animal life, is now fetid, and swarming with active little organisms called vibrios. Prof. Lister, from whose recent lecture this fact is derived, contends, with every show of reason, that this rapid putrefaction and this astounding development of animal life are due to the entry of germs into the abscess during the first operation, and their subsequent nurture and development under favourable conditions of food and temperature. The celebrated physiologist and physicist Helmholtz is attacked annually by hay-fever. From the 20th of May to the end of June he suffers from a catarrh of the upper air-passages; and he has found during this period, and at no other, that his nasal secretions are peopled by these vibrios. They appear to nestle by preference in the cavities and recesses of the nose, for a strong sneeze is necessary to dislodge them.

These statements sound uncomfortable; but by disclosing our enemy they enable us to fight him. When he clearly eyes his quarry the eagle's strength is doubled, and his swoop is rendered sure. If the germ theory be proved true, it will give a definiteness to our efforts to stamp out disease which they could not previously possess. And it is only by definite effort under its guidance that its truth or falsehood can be established. It is difficult for an outsider like myself to read without sympathetic emotion such papers as those of Dr. Budd, of Bristol, on cholera, scarlet-fever, and small-pox. He is a man of strong imagination, and may occasionally take a flight beyond his facts; but without this dynamic heat of heart, the stolid inertia of the free-born Briton cannot be overcome. And as long as the heat is employed to warm up the truth without singeing it over-much; as long as this enthusiasm can overmatch its mistakes by unequivocal examples of success, so long am I disposed to give it a fair field to work in, and to wish it God speed.

But let us return to our dust. It is needless to remark that it cannot be blown away by an ordinary bellows; or, more correctly, the place of the particles blown away is in this case supplied by others ejected from the bellows, so that the track of the beam remains unimpaired. But if the nozzle of a good bellows be filled with cotton wool not too tightly packed, the air urged through the wool is filtered of its floating matter, and it then forms a clean band of darkness in the illuminated dust. This was the filter used by Schroëder in his experiments on spontaneous generation, and turned subsequently to account in the excellent researches of Pasteur. Since 1868 I have constantly employed it myself.

But by far the most interesting and important illustration of this filtering process is furnished by the human breath. I fill my lungs with ordinary air and breathe through a glass tube across the electric beam. The condensation of the aqueous vapour of the breath is shown by the formation of a luminous white cloud of delicate texture. It is necessary to abolish this cloud, and this may be done by drying the breath previous to its entering into the beam; or still more simply, by warming the glass tube. When this is done the luminous track of the beam is for a time uninterrupted. The breath impresses upon the floating matter a transverse motion, but the dust from the lungs makes good the particles displaced. But after some time an obscure disc appears upon the beam, the darkness of which increases, until finally, towards the end of the expiration, the beam is, as it were, pierced by an intensely black hole, in which no particles whatever can be discerned. The air, in fact, has so lodged its dirt within the lungs as to render the last portions of the expired breath absolutely free from suspended matter. This experiment may be repeated any number of times with the same result. It renders the distribution of the dirt within the lungs as manifest as if the chest were transparent.

I now empty my lungs as perfectly as possible, and placing a handful of cotton wool against my mouth and nostrils, inhale through it. There is no difficulty in thus filling the lungs with air. On expiring this air through the glass tube, its freedom from floating matter is at once manifest. From the very beginning of the act of expiration the beam is pierced by a black aperture. The first puff from the lungs abolishes the illuminated dust and puts a patch of darkness in its place, and the darkness continues throughout the entire course of the expiration. When the tube is placed below the beam and moved to and fro, the same smoke-like appearance as that obtained with a flame is observed. In short, the cotton wool, when used in sufficient quantity, completely intercepts the floating matter on its way to the lungs.

And here we have revealed to us the true philosophy of a practice followed by medical men, more from instinct than from actual knowledge. In a contagious atmosphere the physician places a handkerchief to his mouth and inhales through it. In doing so he unconsciously holds back the dirt and germs of the air. If the poison were a gas it would not be thus intercepted. On showing this experiment with the cotton wool to Dr. Bence Jones, he immediately repeated it with a silk handkerchief. The result was substantially the same, though, as might be expected, the wool is by far the surest filter. The application of these experiments is obvious. If a physician wishes to hold back from the lungs of his patient, or from his own, the germs by which contagious disease is said to be propagated, he will employ a cotton-wool respirator. After the revelations of this evening such respirators must, I think, come into general use as a defence against contagion. In the crowded dwellings of the London poor, where the isolation of the sick is difficult, if not impossible, the noxious air around the patient may, by this simple means, be restored to practical purity. Thus filtered, attendants may breathe the air unharmed. In all probability the protection of the lungs will be protection of the entire system. For it is exceedingly probable that the germs which lodge in the air-passages, and which, at their leisure, can work their way across the mucous membrane, are those which sow in the body epidemic disease. If this be so, then disease can certainly be warded off by filters of cotton wool. I should be most willing to test their efficacy in my own person. And time will decide whether in lung diseases also the woollen respirator cannot abate irritation, if not arrest decay. By its means, so far as the germs are concerned, the air of the highest Alps may be brought into the chamber of the invalid.

JOHN TYNDALL

SCIENTIFIC SERIALS

THE *Zeitschrift für Chemie* (No. 1) contains an account of some unfinished experiments by Muck on manganous sulphide, and a note by Dr. Baumhauer, of Bonn, on the action of aqueous hydric chloride on nitrobenzol. In the latter of these the author points out the interesting fact that dichloraniline is a principal product of the reaction. Robert Otto communicates several papers containing the results of experiments which he has performed, for the most part, with the co-operation of Eugen Dreher. The subjects of the papers are "Mercuric Diphenyl," under which title a tolerably exhaustive account of this body is given; "Mercuric Ditolyl," which was not so extensively examined; "On the deportment of Dibenzyl at a high temperature" (it splits into Toluol and Toluylene); "On the transformation of hydro-phenylic sulphide into phenylic sulphide" (the mercuric derivative decomposes thus at 180°:—$(C_6H_5)_2HgS_2 = (C_6H_5)_2, S_2 + Hg$); "On mercuric dinaphtyl," from which it appears that the presence of ethylic acetate is very advantageous in the usual mixture whereby the body is prepared; "On mono-ethylic and mono-methylic mercuric acetate;" and "On the preparation of organic sulphur-compounds by means of sodic hyposulphite."—A. Geuther contributes a short article "On the volatile acids of croton oil." He finds that the oil contains no crotonic acid, which name is consequently a misnomer. Of the two metamers, $C_4H_6O_2$, he consequently designates the solid modification *tetranylic*, and the liquid variety *quartenylic* acid. Croton oil contains a metamer of angelic acid, for which the author proposes the name *tiglinic* acid.—Markownikoff finds that the butylic (fermentation) alcohol, when transformed into iodide, and then, by alcoholic potash, into olefiant, furnishes with hydric iodide the tertiary pseudo-butylic iodide.—Petrieff describes *solid* azoxytoluide (fusing at 57°).

THE *Annales de Chimie et de Physique* for December last contains a short note by M. Soliel on an ocular micrometer, the principle of which was discovered independently by Prof. Govi and himself. The rest of the number is wholly occupied by abstracts of foreign scientific papers.

THE *Annals and Magazine of Natural History*, fourth series, No. 25. The January number of this journal contains the conclusion of Mr. Wollaston's descriptions of the Coleoptera of St. Helena, to which we shall refer elsewhere. Mr. D. Sharp also publishes a paper on the species of *Philhydrus* found in the Atlantic Islands, which may be regarded as supplementary to and rectificatory of Mr. Wollaston's works on the Coleoptera of those islands. Dr. Lycett describes a byssiferous fossil *Trigonia* (*T. carniata* Ag.) Messrs. A. Hancock and R. Howse describe in considerable detail the remains of a fossil fish (*Janassa bituminosa* Schloth.) from the Permian marl-slate of Midderidge in the county of Durham. They identify with the genus *Janassa* the coal-measure form described by Messrs. Hancock and Atthey under the name of *Climaxodus linguæformis*, and regard the genus as belonging to the Rays, and probably allied to *Myliobates*. The known specimens consist chiefly of the teeth, which were originally described by Schlotheim as Trilobites, under the name of *T. bituminosus*; different examples have been described as fish-remains under various names, and the authors refer to their species the *Janassa angulata*, *Humboldti*, *bituminosa*, and *dictea*, *Dictea striata* and *Byzenos latipinnatus* of Count Münster, and the *Acrodus larva* of Professor Agassiz. This paper is illustrated with two excellent plates. Dr. Carl Semper describes the *Helix inæqualis* (Pfeiff.) from Australia as forming a new genus of Testacellidæ, to which he gives the name of *Rhytida*. Professor E. Perceval Wright describes and figures a new parasitic Crustacean, *Pennella orthagorisci*, obtained from a sun-fish in Cork Harbour; and Mr. John Gould describes a new Pigeon, *Otidiphaps nobilis*, forming the type of a new genus. —The only botanical paper in the number is the thirty-first instalment of the Rev. W. A. Leighton's "Notulæ Lichenologicæ," containing an analytical examination of certain new characters in the species of the genera *Nephroma* and *Nephromium*.—Besides the translations and abstracts of foreign papers which appear among the miscellaneous contributions, this number contains the first part of a translation of Professor Häckel's memoir on the organisation of sponges, and their relationship to corals, in which the author maintains that the corals (*Anthozoa*) are very nearly related to the Sponges, that the latter belong to the great group of the Cœlenterata, and that "the sole morphological character which sharply and decidedly separates" them from the rest of the Cœlenterata is to be found in the "deficiency of the urticating organs in all sponges."

THE *Moniteur Scientifique* for January 1st has much of its space occupied by an account of the legal inquiry resulting from the remarkable explosion of potassic picrate which occurred in the Place de la Sorbonne on the 16th June last. The evidence and the speeches of counsel are given at considerable length. M. Dubrunfant contributes an article on the Saline Analysis of Sugars, and on Melassimetry. There is also the usual account of the sittings of the Academy of Sciences; a Photographic Review, and a review of foreign journals—both very carefully written. A new feature in this serial, introduced this year, is a price-current of the principal products referred to in the papers it contains, and for the general use of subscribers. We cannot, however, help thinking that our able contemporary, in seeking to oblige its readers, has undertaken a task which, from the smallness of available space, it cannot adequately fulfil. A full price-current ought to have a periodical to itself.

Revue des Cours Scientifiques, January 22.—The first paper consists of extracts from an eloquent obituary notice of Trousseau, read by M. J. Béchard, the Secretary of the French Academy of Medicine, at the recent annual general meeting. "In this *éloge* of the man who is one of the most distinguished personifications of the old empirical medicine, M. Béchard has skilfully contrived to afford us a glimpse of the advent—possibly close at hand—of scientific medicine." A lecture by M. Lortet on the physiological effects of mountain climbing is the second paper in the present number of the *Revue*. We hope shortly to lay before our readers a full account of M. Lortet's observations. The other papers are one by Prof. Mayer, of Heilbronn, the recently elected Corresponding Member of the Institute of France, on the necessary consequences and inconsequences of the mechanical theory of heat, and a communication, lately presented to the Academy of Sciences, by M. St. Claire Deville, on the nascent state of bodies.

THE *American Naturalist*.—In the number for the present month there is an original article on the microscopic examination of shavings, and two others on the birds of Massachusetts; likewise a continuation of a review of Professor Huxley's Classification of Animals, in which the opinion is expressed that the publication of that book will not endanger the Cuvierian system. A reprint is also given from the *Popular Science Review* of the temperate and well-written article in which Prof. Cleland, of Glasgow, once more lays the ghost of phrenology, and gives, in a popular way, much solid information.

BOTANY

British Museum Herbarium

THE national Herbarium at the British Museum, though not equal in extent to that at Kew, is one of very great value to botanists from the numbers of "type-specimens" it contains; that is, specimens named by the original discoverer or describer, thus serving as a standard for reference. According to the official report lately issued by the Curator, Mr. J. J. Bennett, the herbarium has received large and important additions during the past year, by purchase and donation, from all parts of the world, including flowering plants, ferns, lichens, mosses, sea-weeds, the microscopic Diatoms, fossil plants, sections of wood, &c.; while collections previously received have been arranged and incorporated.

Wood for Gunpowder

ALTHOUGH the materials of which gunpowder is made have not varied since its first invention, there has been considerable variety in the kind of wood from which the charcoal has been obtained. Dense woods are always rejected and the lighter kinds chosen, especially those most free from silica, and capable of producing a friable porous charcoal which burns quickly and leaves the least possible quantity of ash; the kind now generally used by gunpowder manufacturers is known as "Dog-wood," and is usually described as being obtained from the small tree popularly known under that name, the *Cornus sanguinea.* Dr. Hooker has, however, recently discovered that this is a popular error, and that the wood is really almost universally obtained from the Buckthorn, or *Rhamnus frangula*; the former tree being now never used for this purpose, if indeed it ever was. Till a few years since, the bulk of the Buckthorn wood used in this manufacture was supplied from English plantations in Suffolk, Norfolk, Essex, and Kent, but the great increase recently in the demand for the finer descriptions of gunpowder has rendered this source insufficient; and it is now cultivated in immense districts of forest and marsh in North Germany, lying between Berlin and Frankfort, where it forms the natural undergrowth. From the high price obtained for the wood, 10*l.* to 15*l.* per ton, its cultivation would be exceedingly lucrative in this country, as it will grow in almost any soil.

Action of Ether on Plants

THE action of ether as an anæsthetic on the animal frame has induced Dr. Maxwell Masters to experiment on its effects on plants. He states that if a drop is placed gently on the leaf of the Sensitive plant, it produces a paralysing effect, rendering it insensible to subsequent contact. If, however, the ether impinges on the leaf with force, or is allowed to drop from a considerable height, contraction of the leaf immediately takes place, the impact of the falling drop counteracting any paralysing power. It is well known that in the contraction of the leaves of the Sensitive plant a certain amount of vital force is expended, and that if often repeated the plant becomes exhausted, and a time of rest is required before the phenomena are repeated.

Viridescence of Leaves

M. PRILLIEUX has established, as the result of a large number of observations on the leaves of barley, that viridescence is more rapid in diffused light than in the direct light of the sun, in contrast to the production of oxygen, which is more abundant the stronger the light. He introduced into a dark chamber a pencil of solar rays, and, by means of a lens, produced a diverging cone, in which he placed the barley at different distances from the lens, consequently under different intensities of light determinable with precision. He found that near the lens, that is, placed in a very intense light, the etiolated leaves scarcely became green, while at a greater distance the viridescence took place more rapidly, and attained its maximum at a distance of three or four metres, beyond which the activity decreased; so that in a too feeble light the effect was the same as in too strong a light. [L'Institut].

MÖLLER has prepared a beautiful microscopic slide, containing 300 distinct species of Diatoms, showing an extraordinary variety of form, and arranged with marvellous regularity. It forms one of the most interesting objects for the microscope we have seen.

THE "Prodromus Systematis naturalis Regni vegetabilis," the work of three generations of De Candolles, is now approaching completion, as it is not intended to continue it beyond the Exogens. The first section of the sixteenth volume, just published, includes two important monographs, the *Urticaceæ* by Weddell, and the *Piperaceæ* by Casimir De Candolle.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 20.—The following papers were read:—

"On the mechanical performance of logical inference," by W. Stanley Jevons, M.A. Lond., Professor of Logic, &c., in Owens College; communicated by Professor E. Roscoe, F.R.S. The author first referred to the general use of mechanical contrivances for the purpose of mathematical computation, and then contrasted this fact with the utter absence of machines for aiding logical operations. This absence he attributed to the incompleteness of the old logical doctrines. The problem of logical science in its complete generality was first solved by Boole. His logical views, when simplified and corrected, give us a method of indirect deduction of extreme generality and power, founded directly upon the fundamental laws of thought. A proof of the truthfulness and power of this system is to be found in the fact that it can be embodied in a machine just as the calculus of differences is embodied in Mr. Babbage's calculating machine. To explain the nature of the logical machine alluded to, it may be pointed out that the third of the fundamental laws of thought allow us to affirm of any object one or the other of two contradictory attributes, and that we are thus enabled to develope a series of alternatives which must contain the description of a given class or object. Thus, if we are considering the propositions,

Iron is metal,
Metal is element,

we can at once affirm of iron that it is included among the four alternatives:—

Metal, element,
Metal, not element,
Not metal, element,
Not metal, not element.

But according to the second law of thought, nothing can combine contradictory attributes, and this law prevents us from supposing that *iron* can be *not-metal*, while the first premise affirms that it is *metal*. The second premise again prevents our supposing that the combination *metal*, *not-element*, can exist. Hence the only combination of properties which the premises allow us to affirm of *iron* is *metal*, *element*. In a similar manner a complete solution of any logical problem may be effected by forming the complete list of combination in which the terms of the problem can manifest themselves, and then striking out such of the combinations as cannot exist in consistency with the conditions of the problem. The logical machine actually constructed represents the combination, 16 in number, of four positive terms, denoted by A, B, C, D, and their corresponding negatives, *a*, *b*, *c*, *d*. The instrument is provided with eight keys, representing these terms when appearing in the subject of a proposition, with eight keys, placed to the right hand of the former, representing the terms when occurring in the predicate of a proposition, and with the certain *operation* keys denoting the *copular* of the proposition, the *full stop* at the end of it, and the conjunction *or*, according as it occurs in the subject or predicate. There is also a key denoting the *finis* or end of an argument, which has the effect of obliterating any previous impressions, and making the machine a *tabula rasa.* If now each of the letter terms, A, B, C, D, be taken to represent some logical term or noun, and propositions concerning them be, as it were, played upon the machine, as upon a telegraphic instrument, the machine effects thereby such a classification and selection of certain rods representing the 16 possible combinations of the terms, that only those combinations consistent with the propositions remain indicated by the machine at the end of the operations. When once a series of propositions is thus impressed upon the machine, it is capable of exhibiting an answer to any question which may be put to it concerning the possible combinations which form any class. The machine thus embodies almost all the powers of Boole's logical system up to problems involving four distinct terms, and to represent problems of any complexity involving any number of terms only requires the multiplication of the parts of the machine. The construction involves no mechanical difficulties, and depends upon a peculiar arrangement of pins and levers, which it would not be easy to explain without drawings. In this arrangement of the parts the conditions of correct thinking are observed; the representative rods are just as numerous as the laws of thought require, and no rod represents inconsistent attributes. The representative rods are classified, selected, or rejected by the reading of a proposition in a manner exactly answering to that in which a reasoning mind should treat its ideas, and at every step in the progress of a problem the machine indicates the proper condition of a mind exempt from mistake. It is believed that this logical machine may be usefully employed in the logical class-room to exhibit the complete analysis of any argument or logical problem; and it is considered by the author superior for this purpose to a more rudimentary contrivance, the logical abacus, constructed by him for the same purpose. But by far the chief importance of the machine is in a theoretical point of view as demonstrating in the simplest and most evident manner the character and powers of a universal system of logical deduction, of which the first, although obscure solution, was given by Dr. Boole.

"On Jacobi's theorem respecting the relative equilibrium of a revolving ellipsoid of fluid; and on Ivory's discussion of the theorem," by I. Todhunter, F.R.S. Jacobi discovered the theorem that a fluid ellipsoid revolving with uniform angular velocity round its least axis might be in equilibrium. Ivory discussed the theorem, and made several statements regarding the limitations of the proportions of the axis. Ivory's statements contain various errors, and truths based on erroneous reasoning. The object of the present memoir is to correct Ivory's errors, to supply his imperfections, and to add something to what is already known respecting the theorem.

Geological Society, January 12.—Professor Huxley, LL.D., F.R.S., president, in the chair. Messrs. J. Aitken, J.P., president of the Manchester Geological Society; E. Allen, C. Cadle, A. W. Edgell, C. F. Leaf, F.L.S., and S. J. Smith, were elected Fellows. Prof. Otto Torell, of Lund, was elected a foreign correspondent. The following communications were read:—1. "On the geological position and geological distribution of the reptilian or dolomitic conglomerate of the Bristol area." By R. Etheridge, Esq., F.G.S., Palæontologist to the Geological Survey of Great Britain. The author noticed the history of our knowledge of the dolomitic conglomerates of the Bristol area from which the remains of dinosaurian reptiles have been obtained, and then described their mode of occurrence and distribution over the district near Bristol. He regarded these deposits as due to the action of the sea-waves of the later or middle Triassic periods upon the rocks of older Triassic (Bunter) or Permian age during the gradual elevation of the land, and as the probable representatives in point of time of the Muschelkalk, otherwise deficient in Britain. The author then noticed the influence of the conglomerate upon the production of certain minerals, such as calamine and hæmatitic iron-ores, and discussed at some length the probable course of the phenomena of denudation which furnished the materials for the formation of the conglomerate at different levels, in which he recognised two great periods of oscillation, the first witnessing a downward movement of the palæozoic lands and lasting throughout the deposition of the New Red marl and sands, and the second, during which the accumulations of the former were again, at least partly, denuded. With regard to the time at which the remains of thecodont reptiles were imbedded in the conglomerate, the author inferred from the evidence that this took place late in the period of the Keuper. The President inquired on what ground the author considered these reptiles to belong to a late period in the Keuper, and was informed that the author spoke especially with relation to the Keuper of the Bristol area, of which the beds containing them occupied the highest position. Prof. Ramsay considered these conglomerates not merely as of marine origin, but as breccias which had covered the old land surface, which had been worked up by the water of the New Red period. He objected to the term Sea having been introduced into the paper; as, though the tracts may have been islands and promontories, and though the water which surrounded them was salt, there was no open sea, but merely a large inland salt-lake, in which the New Red Marl was formed. The marl was less connected with the New Red Sandstone than with the Lias. The Muschelkalk being absent, it was constantly the case that the marl rested immediately on the palæozoic rocks without the intervention of the Bunter Sandstone. He thought that there were good grounds for connecting the Rhætic beds with the New Red Marl below and the Lias above. The probability was that the change in character was due to a gradual influx of the sea into the inland lakes. He thought that the Thecodont Saurians might also eventually be found even in beds of Liassic age. Prof. T. Rupert Jones remarked that Mr. Tawney and Dr. Duncan had already intimated the St. Cassian aspect and character of the Sutton beds. The freshwater character of some of the Keuper beds was, he remarked, indicated by the presence of *Estheriæ*, and he alluded to the fact of the Bristol palæosaurians having been erroneously used as Permian characteristics in Russia and Carolina. Mr. W. Boyd Dawkins had found at Cheddar that the Dolomitic conglomerate formed two great tongues running up ravines in the older rocks, which had probably been due to subaërial action. Prof. Morris alluded to some sections which seemed to corroborate the views of Mr. Etheridge, and pointed out the relation of the conglomerate beds to the overlying strata at those points. He also mentioned certain peculiarities in the structure of the conglomerate itself. Mr. Etheridge stated in reply that the marls in the Bristol area were the exception, the greater part of the New Red beds being sandstone.

2. "On the superficial deposits of portions of the Avon and Severn Valleys and adjoining districts." By Mr. T. G. B. Lloyd, C.E., F.G.S. The author, after describing the general characters of what he termed the Drifts of the Upper and Lower series, and the freshwater gravels of the Lower Avon, comprised within the district of the Avon Valley between Tewkesbury and Rugby, and of the Severn Valley above and below the town of Worcester, endeavoured to show that there was a balance of evidence in favour of the existence of an upper and lower platform of drift in the main valley of the Lower Avon, the upper one being of marine origin, and probably belonging to the same epoch as the stratified beds of gravel in the neighbourhood of Worcester, which contain marine shells and mammalian remains, whilst the lower one, of freshwater origin, had been derived from the former by fluviatile action, as supposed by the late Prof. Strickland. Further, that there was no evidence to warrant the supposition of the existence of high and low level river-gravels in those portions of the Severn and Avon Valleys under review, and that the apparent absence of any freshwater shells in the gravels of the Severn Valley between Bridgnorth and Tewkesbury led to the inference that the freshwater gravels of the Avon were not represented in the adjoining portions of the Severn Valley, although remains of some of the same species of mammalia occurred in both localities. After stating his opinion that the time had not yet arrived for indulging in theoretical speculations concerning the phenomena of the Drifts of the Upper and Lower series exhibited in so small an area as the one under consideration, the author concluded by expressing hopes that the facts which he had brought forward would contribute their share of help to the further elucidation of the question.

3. "On the surface-deposits in the neighbourhood of Rugby." By Mr. J. M. Wilson, F.G.S. The author commenced by noticing the general configuration of the surface of the district under review, which he stated to consist of an elevated plateau, bounded and rendered irregular in its outlines by valleys. The district consists chiefly of Lower Lias, with a few patches of Middle Lias. The surface-deposits on the plateau and on similar high lands in the neighbourhood consist of—1. Flinty or quartzose drift; 2. Sugary sand, with grains of chalk; 3. Clay, with pebbles, principally of chalk, and distinctly striated. The valleys bounding the plateau were described as belonging to two systems, those of the Avon and Leam. The bottom of each valley is generally a narrow strip of alluvial soil, bordered by sand in some places, and by drift in others. The author has bored down into the surface-deposits in the valley of Low Morton. In one boring, which reached a depth of 53 feet, he stopped in a greyish clay containing chalk-particles; in another, through similar clay to a depth of 57 feet, the rock was reached, and fragments of limestone were brought up.

Mr. Searles V. Wood, jun., had long been aware of the existence of the Middle Glacial Sand near Rugby. He pointed out the difference in the fauna of the sands of the Severn Valley below the glacial clay and those of similar deposits in the east of England, but notwithstanding thought they might be of the same age. Mr. Gwyn Jeffreys was doubtful as to the authenticity of some of the shells which had been brought to Mr. Maw. The fossil shells from the Severn Valley, Wolverhampton, Manchester, and Moel Tryfaen were nearly identical, and indicated raised beaches. He thought it possible that a definite line of such beaches might eventually be recognised through that part of England. Mr. W. Boyd Dawkins did not consider that there was any marked difference in the mammalian fauna of the Avon and Severn Valleys. He had failed to discover any traces of *Elephas antiquus* in either. Mr. Prestwich thought that the author had probably divided the superficial beds into too many separate deposits, though the facts brought forward were of great value. Mr. Evans mentioned that he had received information of the discovery many years ago of a flint implement in association with the bones of extinct mammals at Lawford. This implement had been exhibited at the time to the Geological Society, but had disappeared after the meeting. Mr. Lloyd and Mr. Wilson briefly replied.

Chemical Society, January 20.—Professor Williamson, President, in the chair. The following gentlemen were elected Fellows:—T. Bell, A. Bird, G. R. Hislop, E. Lapper, H. Seward. The first paper read was a note on the absorption of mixed vapours by charcoal, by John Hunter, M.A., Queen's College, Belfast. The author some time ago, published in the *Journal of the Chemical Society* (May 1868), the results obtained by absorbing the mixture of two vapours by means of cocoa-nut charcoal. He found that the absorption was increased when one of the vapours was at a temperature near to its point of condensation; and he explained the phenomenon by assuming that when a fragment of charcoal is introduced into a mixture of two vapours, the one which is nearest to its point of condensation is first absorbed, and this, in its condensed state, aids the absorption of the other. According to this view, a succession of condensations is going on. The theory is strikingly illustrated in experimenting with a mixture of water vapour and ammonia gas—obtained by heating an aqueous solution of ammonia of spec. grav. 0·88—when the mixture is much more largely absorbed than either the gas or the vapour separately. The mean of a set of experiments made at 100° C and 706·2 mm. pressure was 316·6 vols. of the mixture absorbed by one vol. of cocoa-nut charcoal. The President remarked that the results of the experiments were entirely in accordance with what was expected on theoretical grounds.—The next communication was "On the composition of iron rust," by Dr. Crace Calvert. The author had lately occasion to analyse rust obtained from two different places—from the outside of the Conway Bridge, and from Llangollen, North Wales—and he found both specimens to be composed as follows:—

Sesquioxide of iron	92·900
Protoxide of iron	6·177
Carbonate of iron	0·617
Carbonate of lime	0·295
Silica	0·121
Ammonia	traces
	100·000

This result induced the author to inquire to which of the constituents of the atmosphere the formation of rust is chiefly due. With the view of ascertaining this, carefully cleaned blades of steel and iron were put into tubes filled respectively with oxygen, oxygen and a little carbonic acid, oxygen and moisture, &c. The blades were introduced into gas-collecting cylinders, which were then filled above mercury with oxygen, &c. But this proved to be an unsatisfactory method, inasmuch as always some globules of mercury remained adhering to the iron, whereby a galvanic action was produced which of course induced a rapid oxidation. To avoid this the tubes were filled simply by displacement of atmospheric air. The blades were then left exposed to the action of the different agents for a period of four months. The results were as follows:—

Blades in dry oxygen	No oxidation.
,, moist ,,	Out of three experiments only in one a slight oxidation.
,, dry carb. acid	No oxidation.
,, moist ,,	Slight incrustation of a white colour. (Out of six experiments two did not give this result.)
,, dry carb. acid and oxygen	No oxidation.
,, moist carb. acid and oxygen	Most rapid oxidation.
,, dry oxygen and ammonia	No oxidation.
,, moist oxygen and ammonia	No oxidation.

These facts led the author to assume that it is the presence of carbonic acid in the atmosphere, and not oxygen or water vapour, which determines the oxidation of iron. The author next investigated the behaviour of iron in water in which successively oxygen, carbonic acid, a mixture of the two gases, &c., were conducted. He immersed only a part of the blade in the water. The results were analogous to those above mentioned, inasmuch as the most rapid oxidation took place when a mixture of oxygen with carbonic acid was introduced into the water. The action commenced immediately, and in a short time a dark precipitate covered the bottom of the vessel. Now the oxidation in this case was not due to a fixation of the oxygen dissolved in the water, but to oxygen liberated from the water by galvanic action; the occurrence of large quantities of hydrogen above the liquid in the bottle proved this sufficiently. A striking evidence in favour of the supposition that the iron is oxidised through the decomposition of the water, is to be found in the fact that when a bright blade was introduced into distilled water which had previously been deprived of all its absorbed gases by long-continued boiling, it became, in the course of a few days, covered here and there with rust. The spots upon which the rust appeared proved to be impurities in the iron. It is obvious they induced galvanic action, just as a mere trace of zinc placed on one end of the blade would establish a voltaic current.—Finally, Dr. Calvert investigated the state of iron in alkalies, and he discovered that not only the solution of caustic soda, but that of the carbonate of it as well, protects iron against any oxidising action.

Linnean Society, January 20.—Prof. Babington read a paper, being a revision of the Flora of Iceland. He gave an extempore sketch of the country, its climate and character, and then read the introductory part of his paper containing an historical account of what had been done towards ascertaining the vegetable products of the island. It appears there are about 450 species of phanerogamous plants (the exact number at present recorded is 467), of which only about 60 are not natives of Britain. None are peculiar to the island; all the remainder, with three exceptions, are to be found on the European continent, chiefly in Scandinavia; the three arctic plants not otherwise known as European are *Gentiana detonsa*, *Pleurogyne rotata*, and *Epilobium latifolium*. No woods are now to be found in the country, although some existed recently: they have been destroyed by the carelessness of the inhabitants. Now that more care is taken of their remains, it is expected that they will again spring up. The trees were all birch, nor is there any trace of the former existence of pine or other trees. Extensive woods of dwarf birch-trees are found in several places, and some fruticose willows exist, especially an abundance of *S. lanata*. No grain of any kind is grown on the island.

Zoological Society of London, January 13.—John Gould, F.R.S., V.P., in the chair. The secretary called attention to certain additions to the society's menagerie during November and December last, amongst which was particularly noticed a rare American monkey (*Pithecia ouakari*) from the Rio Negro, deposited by L. Joel, Esq., C.M.Z.S.—A letter was read from Lord Lilford, F.Z.S., relating to the exact locality of a specimen of *Otus capensis*, lately living in the society's gardens.—A letter was read from Dr. A. Ernst, of Caraccas, C.M.Z.S., containing some notes on animals recently obtained in the vicinity of that city.—The Rev. H. B. Tristram, F.R.S., exhibited a pair of tawny eagles (*Aquila nævioides*) obtained near Etawah, N.W. India, by Mr. W. G. Brooks, C.E., being the first authentic examples of this species received from that country.—Mr. Swinhoe exhibited and made remarks on some skins of tigers and leopards from various parts of China.—Mr. Gould exhibited a new and very remarkable pigeon, supposed to be from New Guinea, which he had recently described under the name *Otidiphaps nobilis*.—A communication was read from Mr. Henry Adams containing descriptions of a new genus, and of eighteen new species of land and marine shells from the Red Sea, Hainan, and other localities.—A communication was read from Dr. Cobbold containing the description of a new generic type of Entozoa, discovered in a specimen of the Aard-wolf (*Proteles cristatus*), which had recently died in the menagerie. To this were added remarks on the affinities of this Entozoön, especially in reference to the question of parthenogenesis.—A communication was read from Mr. Morton Allport, F.Z.S., containing a brief history of the introduction of the salmon (*Salmo salar*) and other *Salmonidæ* to the waters of Tasmania.—Dr. Murie read a paper containing additional memoranda on irregularity in the growth of salmon. Dr. Murie's observations were founded principally upon specimens hatched and reared in the society's fish-house.

The Institution of Civil Engineers, January 11.—Mr. C. B. Vignoles, F.R.S., president, in the chair. Five candidates were balloted for and declared to be duly elected, viz.: Mr. A. A. Langley, engineer and manager to the Hereford, Hay, and Brecon Railway; Mr. R. White, first-class engineer upon the Great Southern of India Railway; and Mr. E. Wragge, chief engineer on the Toronto, Grey, and Bruce, and the Toronto and Nipissing Railways in Canada, as members; and Mr. W. Rawlinson, engineer and manager of the Brazilian Street Railway Company, and Mr. C. Willman, Middlesbrough, as associates.—A report was brought up from the council, stating that, under the provisions of Sect. IV. of the Bye-laws, the following candidates had recently been admitted students of the Institution:—W. F. Alphonse Archibald, B.A., A. J. Hess, A. Innes Liddell, W. Allingham Magnus, and H. Goulton Sketchley.

Statistical Society, January 18.—William Newmarch, F.R.S., president, in the chair. The following gentlemen were elected Fellows:—Messrs. Iltuduo Thomas Prichard, Henry Hoare, David Maclagan, and Josiah Samuel Parker. Professor Levi read a paper on "the statistics of joint-stock companies from 1814 to the present time; and of companies with limited and unlimited liability formed since the year 1856."

DUBLIN

Royal Zoological Society of Ireland, January 11.—Dr. Banks in the chair. Rev. Dr. Haughton read the report for 1869, from which it appeared that the number of visitors to the Gardens was 9,000 more for 1869 than for 1868, and that the receipts for 1869 exceeded those of 1868 by 137*l*. It would appear that there are now in the Gardens 143 mammals, 219 birds, and 25 reptiles—specimens, not species, we presume—and that their health and condition are excellent. The fact is mentioned that since 1857 twenty lions and 31 lionesses have been bred in the Gardens. The Earl of Mayo was elected president for this year.

Royal Geological Society of Ireland, January 12.—Mr. W. Andrews in the chair. The secretary read a paper by Dr. L. Lindsay on further researches in the gold-fields of Scotland. Rev. Professor Haughton read a paper by Mr. J. D. Latouche on a spheroidal structure occurring in some Silurian Rocks of Wales. As supplementary to the views put forward in Mr. Latouche's paper, Dr. Haughton stated that this spheroidal structure shows on a small scale what cleavage does on a large one, and that he believed that the latent structure was brought out by the weathering, not caused by it; indeed, the cleavage stream of force might be compared to that of a great river—it might flow along for miles through a country in an even uninterrupted course, then some small obstacles came in its way, and as the result a series of eddies were formed. Spheroidal structures were representatives of these eddies of force, and the ordinary cleavage planes were representatives of the uninterrupted stream—the one was the other on an immense scale. Dr. Haughton also showed that it followed rigorously from the mathematical laws of cleavage, that the parallipipedal blocks formed by cleavage must have themselves an internal spheroidal structure, of a concentric kind. This was the latent structure brought out by weathering in the manner shown in the beautiful drawings of Mr. Latouche.—Dr. Macalister exhibited a portion of a skull which had been dug up recently, while some repairs were being made to the vaults of Trinity College Chapel. This fragment was found laid along with other bones, and had evidently been dug up when the foundations of the chapel were being laid, and then, with the other bones found on that occasion, again buried. The skull was of a low type. Rev. Dr. Haughton agreed with Dr. Macalister as to the low type of the skull. Mr. J. J. Lalor did not agree with Dr. Macalister that this skull was of a low type. He had made a series of accurate measurements of skulls in conjunction with Dr. Carpenter, of London, and therefore could speak on the subject. Absence of forehead was no evidence of absence of brain capacity; lowness of skull was considered a mark of beauty by some. He could not venture to say whether it was the skull of a man or a woman, but its brain capacity did not authorise one in saying that it was a low skull; it might have been the skull of a Provost, and certainly was one of more than ordinary capacity. Dr. Macalister in reply stated that he saw no reason to alter his view on the subject, as it had been based on careful measurements and on exact reasoning, neither of which he thought admitted of contradiction.

Institution of Civil Engineers of Ireland, January 12.—Mr. J. Ball Greene, C.E., in the chair. Mr. B. Stoney read a paper by Mr. C. P. Cotton on a novel means of transit for minerals in the county of Sligo. An extensive barytes quarry was worked on the side of a steep hill, the mineral had to be lowered a depth of over 1,000 feet, and this was effected by means of boxes swung on ropes, forming a wire rope railway. Mr. A McDonnell read a paper on workshop machinery driven by rapidly moving ropes.

Royal Dublin Society, January 18.—Mr. John Adair in the chair. Professor Macalister read a paper on "The Curves in the Spine considered from an æsthetical point of view." Dr. J. Emerson Reynolds read some notes on "the determination of the flashing point of petroleum oils, as settled by Act of Parliament." The author described in detail the apparatus directed to be employed, and pointed out the difficulties and sources of error to be guarded against in using the Government test. He suggested the adoption of an uniform mode of estimating the flashing point of mineral oils, which experience proved to be that most suited for affording reliable results; and further proposed that in all doubtful cases—a special method—which he indicated, should be employed in order to serve as a test of the accuracy of the parliamentary process.—A drawing of the Nebulæ in Argos, and Dr. Monckhoven's new light for photography were exhibited.

MANCHESTER

Literary and Philosophical Society, January 11.—Ordinary Meeting.—Mr. E. W. Binney, F.R.S., F.G.S., vice-president, in the chair. The chairman described the aurora borealis, as observed by him at Cheetham Hill on the evening of Monday, the 3rd inst., at 7-30 P.M. Dr. Joule, F.R.S., said he had noticed some remarkable disturbances of the magnetic dip on the 3rd inst., which no doubt were connected with the auroral display. He had also noticed similar disturbances of the dipping needle during the gale on Saturday, the 8th inst.—Letters were read from Mr. A. H. Green and Mr. E. Hull, defending the accuracy of the Geological Survey map in the matter of the red rock fault referred to in Mr. Binney's paper, read before this society on November 16th (see NATURE, No. 7).—The chairman, with all respect to Messrs. Green and Hull, again denied the correctness of their map and sections so far as the "red rock fault" was concerned. He stated that he was prepared to maintain his position on the ground where the sections were exposed between Stockport and Macclesfield.—Dr. Joule exhibited his current meter, and with it, in connection with a galvanometer, made an experiment to determine the horizontal intensity of the earth's magnetism in absolution measure; the result gave 3·83 as the value of this element in the hall of the society. The current employed was produced by a single cell of a Bunsen's battery.

Microscopical and Natural History Section, January 3.—R. D. Darbishire, B.A., F.G.S., in the chair.—Mr. J. Sidebotham read the following paper:—"Notes on the pupa and imago of *Acherontia atropos*." The peculiar cry or squeak of the death's-head moth is very well known. It has been by some observers thought that this sound is produced by the friction of the joints of the prothorax and mesothorax; this conclusion is, in the opinion of the author of the paper, much strengthened by the following circumstance. A few weeks ago, when he was replacing some damp moss on some pupæ, he heard the peculiar cry of the moth, but much weaker. On examining the pupæ he selected the one from which the cry proceeded, and placed it in the palm of his hand; when at rest there was no sound, but the pupa at once produced it on being touched or pressed gently; on taking hold of it between the finger and thumb, if the head alone were confined, there was no sound, but if the tail, the motion of the joints was more energetic and the sound louder. In five days afterwards a very fine female moth emerged from the pupa, apparently none the worse for his experiments. The fact of the pupa ever producing this cry, disproves all ideas as to its being produced by expelling air through cavities, against a membrane, since in the pupa state all the muscles are as it were bound up in a horny case, and only those able to move which work the joints of the thorax and body, and besides this the amount of air which could be taken through the spiracles of the pupa would be obviously insufficient to produce such a volume of sound.

PARIS

Academy of Sciences, January 17.—M. de Verneuil presented and made some remarks upon a geological map of the Ural, published by M. de Moeller, a Russian officer of Mines. He stated that M. de Moeller had referred the sandstones of Artinsk—regarded as Permian by MM. Murchison; Keyserling,