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

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THE SIZE OF ATOMS

THE idea of an atom has been so constantly associated with incredible assumptions of infinite strength, absolute rigidity, mystical actions at a distance, and indivisibility, that chemists and many other reasonable naturalists of modern times, losing all patience with it, have dismissed it to the realms of metaphysics, and made it smaller than "anything we can conceive." But if atoms are inconceivably small, why are not all chemical actions infinitely swift? Chemistry is powerless to deal with this question, and many others of paramount importance, if barred, by the hardness of its fundamental assumptions, from contemplating the atom as a real portion of matter occupying a finite space, and forming a not immeasurably small constituent of any palpable body.

More than thirty years ago naturalists were scared by a wild proposition of Cauchy's, that the familiar prismatic colours proved the "sphere of sensible molecular action" in transparent liquids and solids to be comparable with the wave-length of light. The thirty years which have intervened have only confirmed that proposition. They have produced a large number of capable judges; and it is only incapacity to judge in dynamical questions that can admit a doubt of the substantial correctness of Cauchy's conclusion. But the "sphere of molecular action" conveys no very clear idea to the non-mathematical mind. The idea which it conveys to the mathematical mind is, in my opinion, irredeemably false. For I have no faith whatever in attractions and repulsions acting at a distance between centres of force according to various laws. What Cauchy's mathematics really proves is this: that in palpably homogeneous bodies such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the wave-length. Thus in water, contiguous cubes, each of one one-thousandth of a centimetre breadth, are sensibly similar. But contiguous cubes of one ten-millionth of a centimetre must be very sensibly different. So in a solid mass of brickwork, two adjacent lengths of 20,000 centimetres each, may contain, one of them nine hundred and ninety-nine bricks and two half bricks, and the other one thousand bricks: thus two contiguous cubes of 20,000 centimetres breadth may be considered as sensibly similar. But two adjacent lengths of forty centimetres each might contain, one of them one brick and two half bricks, and the other two whole bricks; and contiguous cubes of forty centimetres would be very sensibly dissimilar. In short, optical dynamics leaves no alternative but to admit that the diameter of a molecule, or the distance from the centre of a molecule to the centre of a contiguous molecule in glass, water, or any other of our transparent liquids and solids, exceeds a ten-thousandth of the wave-length, or a two-hundred-millionth of a centimetre.

By experiments on the contact electricity of metals made eight or ten years ago, and described in a letter to Dr. Joule, which was published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that plates of zinc and copper connected with one another by a fine wire attract one another, as would similar pieces of one metal connected with the two plates of a galvanic element, having about three-quarters of the electro-motive force of a Daniel's element.

Measurements published in the Proceedings of the Royal Society for 1860 showed that the attraction between parallel plates of one metal held at a distance apart small in comparison with their diameters, and kept connected with such a galvanic element, would experience an attraction amounting to two ten-thousand-millionths of a gramme weight per area of the opposed surfaces equal to the square of the distance between them. Let a plate of zinc and a plate of copper, each a centimetre square and a hundred-thousandth of a centimetre thick, be placed with a corner of each touching a metal globe of a hundred-thousandth of a centimetre diameter. Let the plates, kept thus in metallic communication with one another be at first wide apart, except at the corners touching the little globe, and let them then be gradually turned round till they are parallel and at a distance of a hundred-thousandth of a centimetre asunder. In this position they will attract one another with a force equal in all to two grammes weight. By abstract dynamics and the theory of energy, it is readily proved that the work done by the changing force of attraction during the motion by which we have supposed this position to be reached, is equal to that of a constant force of two grammes weight acting through a space of a hundred-thousandth of a centimetre; that is to say, to two hundred-thousandths of a centimetre-gramme. Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space one hundred-thousandth of a centimetre thick. The whole work done by electric attraction in the formation of this pile is two centimetre-grammes.

The whole mass of metal is eight grammes. Hence the amount of work is a quarter of a centimetre-gramme per gramme of metal. Now 4,030 centimetre-grammes of work, according to Joule's dynamical equivalent of heat is the amount required to warm a gramme of zinc or copper by one degree centigrade. Hence the work done by the electric attraction could warm the substance by only $\frac{1}{16120}$ of a degree. But now let the thickness of each piece of metal and of each intervening space be a hundred-millionth of a centimetre instead of a hundred-thousandth. The work would be increased a millionfold unless a hundred-millionth of a centimetre approaches the smallness of a molecule. The heat equivalent would therefore be enough to raise the temperature of the material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be four-hundred-millionth of a centimetre. The work and its heat equivalent will be increased sixteen-fold. It would therefore be 990 times as much as that required to warm the mass by 10 cent., which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout ; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of a three-hundred-millionth of a centimetre thick, placed close together alternately, form a near approximation to a chemical combination, if indeed such thin plates could be made without splitting atoms.

The theory of capillary attraction shows that when a bubble—a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observation of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about sixteen milligrammes weight per millimetre of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimetre-milligrammes, is equal to sixteen times the number of square millimetres by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal effect of drawing out a Film of Liquid," published in the Proceedings of the Royal Society for April 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by twenty-four milligramme-millimetres for every square millimetre added to its area.

Suppose then a film to be given with a thickness of a millimetre, and suppose its area to be augmented ten thousand and one fold: the work done per square millimetre of the original film, that is to say per milligramme of the mass, would be 240,000 millimetre-milligrammes. The heat equivalent of this is more than half a degree centigrade of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately a ten-thousandth of a millimetre. The commonest observation on the soap-bubble (which in contractile force differs no doubt very little from pure water) shows that there is no sensible diminution of contractile force by reduction of the thickness to the ten-thousandth of a millimetre; inasmuch as the thickness which gives the first maximum brightness round the black spot seen where the bubble is thinnest, is only about an eight-thousandth of a millimetre.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be further stretched, until its thickness is reduced to a twenty-millionth of a millimetre. The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 1,130 times the quantity required to raise the temperature of the liquid by one degree centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. A smaller amount of work spent on the liquid would convert it into vapour at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of a twenty-millionth of a millimetre. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of a twenty-millionth of a millimetre of water.

The kinetic theory of gases suggested a hundred years ago by Daniel Bernouilli has, during the last quarter of a century, been worked out by Herapath, Joule, Clausius, and Maxwell, to so great perfection that we now find in it satisfactory explanations of all non-chemical properties of gases. However difficult it may be even to imagine what kind of thing the molecule is, we may regard it as an established truth of science that a gas consists of moving molecules disturbed from rectilineal paths and constant velocities by collisions or mutual influences, so rare that the mean length of proximately rectilineal portions of the path of each molecule is many times greater than the average distance from the centre of each molecule to the centre of the molecule nearest it at any time. If, for a moment, we suppose the molecules to be hard elastic globes all of one size, influencing one another only through actual contact, we have for each molecule simply a zigzag path composed of rectilineal portions, with abrupt changes of direction. On this supposition Clausius proves, by a simple application of the calculus of probabilities, that the average length of the free path of a particle from collision to collision bears to the diameter of each globe, the ratio of the whole space in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes in unit volume is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path. But we cannot believe that the individual molecules of gases in general, or even of any one gas, are hard elastic globes. Any two of the moving particles or molecules must act upon one another somehow, so that when they pass very near one another they shall produce considerable deflexion of the path and change in the velocity of each. This mutual action (called force) is different at different distances, and must vary, according to variations of the distance, so as to fulfil some definite law. If the particles were hard elastic globes acting upon one another only by contact, the law of force would be—zero force when the distance from centre to centre exceeds the sum of the radii, and infinite repulsion for any distance less than the sum of the radii. This hypothesis, with its "hard and fast" demarcation between no force and infinite force, seems to require mitigation. Without entering on the theory of vortex atoms at present, I may at least say that soft elastic solids, not necessarily globular, are more promising than infinitely hard elastic globes. And, happily, we are not left merely to our fancy as to what we are to accept for probable in respect to the law of force. If the particles were hard elastic globes, the average time from collision to collision would be inversely as the average velocity of the particles. But Maxwell's experiments on the variation of the viscosities of gases with change of temperature prove that the mean time from collision to collision is independent of the velocity, if we give the name collision to those mutual actions only which produce something more than a certain specified degree of deflection of the line of motion. This law could be fulfilled by soft elastic particles (globular or not globular); but, as we have seen, not by hard elastic globes. Such details, however, are beyond the scope of our present argument. What we want now is rough approximations to absolute values, whether of time or space or mass—not delicate differential results. By Joule, Maxwell, and Clausius we know that the average velocity of the molecules of oxygen or nitrogen or common air, at ordinary atmospheric temperature and pressure, is about 50,000 centimetres per second, and the average time from collision to collision a five-thousand-millionth of a second. Hence the average length of path of each molecule beween collisions is about $\frac{1}{100000}$ of a centimetre. Now, having left the idea of hard globes, according to which the dimensions of a molecule and the distinction between collision and no collision are perfectly sharp, something of apparent circumlocution must take the place of these simple terms.

First, it is to be remarked that two molecules in collision will exercise a mutual repulsion in virtue of which the distance between their centres, after being diminished to a minimum, will begin to increase as the molecules leave one another. This minimum distance would be equal to the sum of the radii, if the molecules were infinitely hard elastic spheres; but in reality we must suppose it to be very different in different collisions. Considering only the case of equal molecules, we might, then, define the radius of a molecule as half the average shortest distance reached in a vast number of collisions. The definition I adopt for the present is not precisely this, but is chosen so as to make as simple as possible the statement I have to make of a combination of the results of Clausius and Maxwell. Having defined the radius of a gaseous molecule, I call the double of the radius the diameter; and the volume of a globe of the same radius or diameter I call the volume of the molecule.

The experiments of Cagniard de la Tour, Faraday, Regnault, and Andrews, on the condensation of gases do not allow us to believe that any of the ordinary gases could be made forty thousand times denser than at ordinary atmospheric pressure and temperature, without reducing the whole volume to something less than the sum of the volume of the gaseous molecules, as now defined. Hence, according to the grand theorem of Clausius quoted above, the average length of path from collision to collision cannot be more than five thousand times the diameter of the gaseous molecule; and the number of molecules in unit of volume cannot exceed 25,000,000 divided by the volume of a globe whose radius is that average length of path. Taking now the preceding estimate, $\frac{1}{100000}$ of a centimetre, for the average length of path from collision to collision, we conclude that the diameter of the gaseous molecule cannot be less than $\frac{1}{500000000}$ of a centimetre; nor the number of molecules in a cubic centimetre of the gas (at ordinary density) greater than 6×10^{21} (or six thousand million million million).

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air at ordinary pressure and temperature; and, therefore, the number of molecules in a cubic centimetre may be from 3×10^{24} to 10^{26} (that is, from three million million million million to a hundred million million million million). From this (if we assume for a moment a cubic arrangement of molecules), the distance from centre to nearest centre in solids and liquids may be estimated at from $\frac{1}{140000000}$ to $\frac{1}{460000000}$ of a centimetre.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish with what we cannot but regard as a very high degree of probability the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the hundred-millionth, and greater than the two thousand-millionth of a centimetre.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a rain drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket-balls.

W. T.

FRESENIUS'S ANALYSIS

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Seventh Edition. Edited by Arthur Vacher. 8vo., pp. viii. and 264. (London: Churchill, 1869.)

Quantitative Chemical Analysis. By Dr. C. Remigius Fresenius. Fifth edition. Edited by Arthur Vacher. 8vo., pp. viii. and 377. (London: Churchill, 1870.)

Anleitung zur qualitativen chemischen Analyse. By Dr. C. Remigius Fresenius. 8vo., pp. xii. and 240, with 43 woodcuts; price 4*s.* (Brunswick, 1869. London: Williams and Norgate.)

IN no branch of chemistry, perhaps, has more useful progress been made of late years than in analysis. The other departments of the science, technical and organic chemistry, for instance, have been cultivated with assiduity and even ostentation; while the study of analysis, invaluable and necessary as it is, has been comparatively neglected as humble and unadorned. Much of this apathy has no doubt arisen from the mechanical nature of the task of analytical discovery, which commonly requires a greater share of industry than intellectual effort. The gradual introduction of refined physical methods has, however, commenced, and will no doubt complete an entire change in the aspect of this subject.

Chemists will not need to be reminded of the obligations they owe to Fresenius, whose analytical manuals are deservedly known, and in common use in almost every laboratory. The enormous amount of special results they contain is hardly conceivable to an outsider, who will not readily appreciate the respect paid to them by grateful practitioners. The thirteenth edition (of which the first instalment has just reached us) has been retouched and improved, and is in every way worthy of the confidence of chemists. Its appearance has, however, suggested to us some reflections which might, not improbably, be carried out with advantage. Few readers will fail to observe that the dimensions of the book and the subject have both increased, and at a rate that will necessitate a remedy in the course of a few years. Possibly it may be agreed that, in addressing a single book to four kinds of students, the author has performed a somewhat superfluous task for three of them. Those who are engaged in pharmacy, the arts, manufactures, and agriculture, will all find here much more than they respectively require. The inconvenience obviously admits of easy removal; each class ought to have a handbook suited to its own peculiar demands, and to those alone. The mere economy of time thus effected would be an additional recommendation to such a step. Moreover, it would allow of altering, for the sake of the purely chemical student, the entire system of nomenclature and notation. These admittedly belong to a period that has passed away; and their presence in the book universally interferes much with its proper utility in the laboratory. The apology in the preface will hardly be accepted by teachers; neither can we suppose that the author, if writing specially, would say of chemistry that it is "the science which acquaints us with the substances of which our earth consists, their composition and decomposition, and mutual deportment generally." It is time that this barren and lifeless definition ceased to be adopted by every instructor. There is, indeed, a quaint mediæval tone about the whole of the book.

The historical reader will notice under the section devoted to "Operations," titles and processes and explanations that abound as we trace our steps backwards from Thénard. The practical investigator might regret the omission of Bunsen's method of filtration, of argentic sulphate as a common impurity in the nitrate, of the formation of Claudet's body as a test for cobalt. We see a condenser figured which could not easily be used with a continuous stream of water, and has been long abandoned in laboratories. Again, hydric nitrate is said to be purified by distilling at the temperature of ebullition; which does not, however, occur unless under that point. We may, nevertheless, assure the experimenter that he will not find in Dr. Fresenius's work more than a few errors of the kind which affect him. There is evidence throughout it of accumulated care and forethought, and the writer's clear exposition and systematic procedure are a proof both of unsparing labour and severe economy.

The author's two German treatises have hitherto been presented to the English student in the form of translations, which portrayed the characteristic features of the originals with satisfactory fidelity. But, for the reasons we have already intimated, there has been a general desire for such changes as would make each of these volumes a handy laboratory book. Mr. Vacher has boldly taken upon himself this task of dual reform, having "specially striven to meet the present wants of English students." He has no doubt succeeded in making two thoroughly English books, which will be most valuable to students; but they can no longer be legitimately termed editions of Fresenius, which they resemble only as a pen-and-ink sketch resembles a perfect picture. That the editor himself is to a certain extent conscious of this is tolerably apparent from the following passages, which are too important to be left unnoticed:—"The present edition has been entirely re-written and much condensed. . . . The metals have been grouped anew, and the old grouping of the acids has been abandoned. . . . The processes have been made as far as possible complete in themselves, so as to obviate constant reference to different pages of the book. Several new figures have been added, and the number of tables has been increased. I have endeavoured to render the arrangement as simple as possible" (preface to "Quantitative Analysis"). And again, "The language has been condensed, the notation and nomenclature have been modernised, the arrangement has been simplified. The consideration of rare inorganic bodies and of organic has been deferred to the latter part of the volume, where a section has been devoted to each. . . . The cause of analysis has been simplified in description, and the preliminary examination has been curtailed. . . . All the facts and, except in one or two trivial instances, all the processes given in this edition have the authority of Fresenius, but the principal changes in the arrangement have been made on my own responsibility" (preface to the "Qualitative Analysis"). Editions for which the editor is thus far responsible, while the reputed author is responsible for a part only of the *facts* (not the whole of them, as Mr. Vacher states), ought not to bear Fresenius's name in any other than a subordinate manner, all these works are, in reality, outlines of analytical chemistry on the basis of Fresenius.

Mr. Vacher has, on the whole, however, carried out successfully the various changes he had in view. The adoption of the prevailing notation is in itself an improvement, teachers having hitherto been compelled to use one interpretation for symbols in the lecture-room, and another in the laboratory. He has not, however, quite so well fulfilled his promise as to the nomenclature, which is still unsystematic; and therefore misleading. For example, at p. 3 ("Quantitative Analysis") we find *calcium chloride*, at p. 27 *carbonate of calcium*; at p. 40 *ferrous sulphide*, at p. 42 *sulphide of manganese*, at p. 40 *ferric oxide*, at p. 44 *alumina*. At p. 54 the word "acid" undergoes a very sudden transformation; "Ignited titanic *acid* does not dissolve in hydrochloric acid." These instances of haste, of which there are not a few, ought to be removed from the next edition.

It seems hardly probable that these large analytical treatises will be perfectly available for the use of students until they are divided into separate works. Subjects such as water analysis, manure analysis, volumetric analysis, assaying, &c., require, as we have said, a separate treatment. Mere condensation, however ably performed, will not accomplish a widely useful end. Mr. Vacher, nevertheless, deserves much credit for a hazardous experiment, which, though not altogether unimpeachable, is a real service to chemistry.

Our general impression of analysis is, that, both in point of scientific form and literary expression, it needs a strong stimulus from other quarters.

OUR BOOK SHELF

The Literature and Curiosities of Dreams. A commonplace Book of Speculations, concerning the mystery of Dreams and Visions, records of Curious and well-authenticated Dreams, and Notes on the various modes of Interpretation adopted in Ancient and Modern Times. By Frank Seafield, M.A. 2nd. Ed. Revised; pp. 518. (London: Lockwood and Co. 1869.)

IN this book the author has gathered together almost every kind of information on the subject of which it treats. The compilation of it has doubtless been a labour of love, and the author's great object has been to select, from all sources, whatever is most characteristic of his opinions which have been held on the subject of Dreams, and also all the best examples upon which these opinions have been founded. He tells us that no amount of research has been considered irksome or irrelevant, so that, in his opinion, "there is nothing extant in the way of dream-speculation or anecdote which is not fairly and impartially represented." The book is, in fact, a rich and methodically arranged storehouse of dreams and of opinions thereon, which will be valued by many who are merely curious, as well as by those who are more seriously interested about their causes and phenomena. It is likely to serve as a book of reference, or as one which may be had recourse to in spare half-hours, rather than as a work which will be taken up to be at once read and mastered.

Untersuchungen über Psychologie. Anmerkungen zu Robert Zimmermann's "Philosophische Propädeutik." Mit Rücksicht auf Herbart, J. H. v. Fichte, Ulrici, Fechner, Lindner, Drbal, Flüget, Nahlowsky, Lange, Darwin, C. Vogt, L. Büchner, Moleschott, Lotze, Hoppe, u.s.w. Von Dr. F. A. v. Hartsen. (Leipzig, 1869.)

Untersuchungen über Logik. Mit Rücksicht auf Apelt, Bolzaus, Drbal, Gratry, Kuno Fischer, Hegel, Herbart, Kant, Maudsley, J. Stuart Mill, Strümpell, W. Schuppe, Trendenburgh, Ueberweg, R. Zimmermann, u.s.w. Von Dr. F. A. v. Hartsen. (Leipzig, 1869.)

THESE publications by Dr. Hartsen are somewhat discursive criticisms of disputed matters in psychology and logic, and of the opinions of the different authorities whose names are mentioned on the title-pages. Neither of them lays claim to give a systematic account of the subject with which it deals, and both of them have the character of critical articles suited to a review, rather than of treatises. The "Psychological Inquiries" consist entirely of discussions upon Zimmermann's "Empirical Psychology," paragraph by paragraph, references being made to the numbered paragraphs in the original work. It will be obvious that this plan of procedure is rather trying to the reader who is obliged to guess from the critical observations at the nature of the opinions that are in question, and whose interest is not easily kept up in so desultory a disquisition. Nevertheless, if he is content to persevere, he will meet with much that is suggestive and instructive. The "Logical Inquiries," though not systematic and complete, will in like manner repay perusal; they are contributions to the foundation of a system of scientific logic.

Kurzes Lehrbuch der Physiologie des Menschen. Von Dr. E. Larisch. (Marburg: Oscar Ehrhardt. London: Williams and Norgate.)

THIS is very similar to Budge's little work, but smaller, and written in more of a narrative style. It therefore contains less matter, and this is especially the case in the part relating to the nervous system. Dr. Larisch has not the scientific eminence of Prof. Budge, and therefore speaks with less authority; on the other hand, he is free from the temptation of attaching too much importance to his own researches and views.

LETTERS TO THE EDITOR

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

Formation of Ground Ice

I THINK the enclosed letter contains as good a description of the formation of ground ice as I have hitherto seen; I therefore send it to you, thinking it of sufficient interest to appear in NATURE. JOHN TYNDALL

Royal Institution, March 27

To PROFESSOR TYNDALL

I have been engaged since September last in making a survey for the Intercolonial railway, up the valley of the Matapediac, a stream about two hundred feet in width and four or five feet in depth, which discharges into the Restijinche river, about twenty miles above the head of the Baie des Chaleures.

The Matapediac, which is fed by large fresh water springs, runs over a rocky bottom covered with loose stones, ranging in size from coarse gravel to boulders as large as a hogshead, and the average current is about four miles an hour.

Early in November last the temperature went down in one night to 12°F., and on going out of camp the following morning I noticed large quantities of what appeared to be snow saturated with water floating down the stream, but not a particle of snow had fallen near us or for many miles round, as far as I could see by the mountain tops, nor had any ice formed on the surface of the river.

The water opposite where I stood was about six feet deep, and perfectly clear, so that I could see every stone on the bottom, and with the exception of the floating slush, the river was as it had been the previous day when the temperature was about 50° F.; I got into a canoe and paddled with the current for half a mile or so, and in shooting some small rapids, where the water in places was not more than two or three feet deep, I noticed on the bottom, masses of the slush clustered round and between the boulders, and a slight touch with the paddle was sufficient to free these clusters, when they rose to the surface, and were carried away by the current. I continued down the stream for three or four miles, and noticed the same thing in every rapid, where the water was shallow and *ruffled* by stones at the bottom.

The buoyancy of this slush was such that when detached from the bottom it rose so rapidly as to force itself well out of the water, and then floated off about half submerged.

I watched this forming of slush for many days, and in several cases found small stones imbedded in the floating slush, having been torn from the bottom when the buoyancy of the slush, aided by the running water, caused it to rise.

The temperature continued getting lower daily, and the slush in the rapids formed more rapidly than it was carried away, so much so that a bar or dam was formed across the river at each rapid, backing up the water in some cases five and six feet, when it generally found an outlet over the adjoining land, and into its natural bed again, or the head of water became sufficient to tear away the obstruction, which by this time had become a solid frozen mass.

All this time, no properly crystallised ice had formed on the surface of the river, the current being too rapid, but the slush or "anchor ice" as the trappers call it, was forming in deeper water than it had formed in before, indeed all over the river bottom, and was rising and floating away as I have already described, Eventually the temperature got down to two and three degrees below zero, when the river *surface* began to freeze in the eddies and along the edges, and the open water space became narrower every day, and was filled with floating "anchor ice" and detached masses of solid ice, which here and there became jammed and frozen together, so as to form ice-bridges on which we could cross.

These ice-bridges served as booms to stop much of the floating ice, which froze solid the moment it came to rest; and in this manner the river at last became completely frozen over for about forty miles of its length, but not until after we had experienced five weeks of steady cold, with the thermometer never above + 12° F., and frequently down to − 16° F.

It is just possible that what I have endeavoured to describe may suggest something to you, or it may be an old story; if so, please pardon the intrusion. W. G. THOMPSON

Dalhousie, N.B., February 18

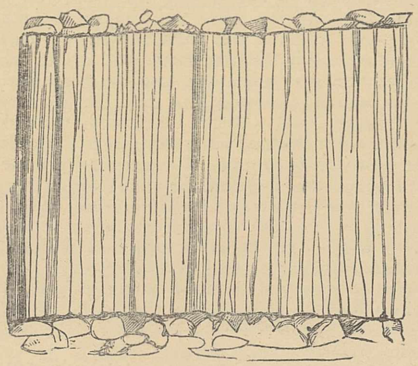
Prismatic Ice—Sandstone Boulder in Granite

WALKING over the Rough Tors of Dartmoor a few days since in company with a friend, Mr. Walter Morrison, M.P., we observed two phenomena which it appears to us should be recorded, since if they have been met with before, we believe that the occurrence of either is infrequent.

In a small roadside quarry, most probably excavated for the purpose of obtaining stones for metalling the road, we observed some granite undergoing the process of decomposition. The stratification of the superincumbent mass struck us as being peculiar, and we entered the quarry to examine it.

The near examination exhibited to us that the surface of the granite had been elevated to the extent of an inch and a half from the rock by means of ice. The whole of the ice, which formed a very extensive bed, was made up of a series of small needle-like prisms, which stood upon their end like minute basaltic columns. These prisms were all perpendicular to the surface of the rock on which they stood; that is, those that were situated on the side of the quarry projected horizontally, while those at our feet stood in a position vertical to the floor. These latter, instead of being on a rock, were upon the surface of granitic gravel, the débris of the surrounding rocks. And in the same manner as the ice mass had forced out the surface of rock at the sides of the quarry, it had raised up the entire surface of the gravel; and this so completely, that we only observed it in consequence of examining it for comparison, since the ice was strong enough to bear our weight when not increased by irregular motion.

It was to us new and interesting to find the ice assuming this basaltic form, and occupying the position in which we found it, in relation to the rock and gravel over which



a. Débris on the surface of the *b* Crystalline ice. *c*. Surface of rock.

it lay. Water in freezing will put on almost any description of crystalline appearance; there is no reason then why upright needle-like prisms may not be as frequent as any other, but that it should put on this form in this position can only be explained when we know how the water that formed the ice got to that position. In trying to obtain this information, we observed that the lines of the stratification in the softer parts corresponded above and below the ice, and that those on the surface of the ice agreed with those at the base. This would demonstrate that the water did not arrive in that position as water flows over the surface of the rock. Had that been the case, the outer crust would, if it had not fallen off, have been moved a degree lower, which would have coincided with the spot at the base from which it was removed. The only way in which, therefore, the formation of the ice appears possible, consistently with existing facts, is by the water having percolated through the decomposing granite rock. There, oozing gradually, it only froze when it came to the surface, while the weight of water behind had power to lift off the external crust of rock. This again was forced outward by the water still pressing behind, the whole freezing only when it passed out through the rock. One and, I must admit, a strong objection to this idea lies in the circumstance that the gravel at the bottom of the quarry was elevated in the same manner; there cannot have been at the bottom of the quarry a weight of water from behind pressing the basaltic prisms outward. Whatever may be the cause of the phenomenon, there can be no doubt but that it must be an immensely powerful agent in the disintegration of rocks.*

The second feature that appears to be worthy of being recorded is that of a water-worn sandstone pebble imbedded in a mass of granite. On the summit of the hill near Warren Tor, we came upon the remains of a cairn, most of the stones of which had been carted away, apparently for the purpose of being built into a house that has recently been erected near it. The stones generally consisted, like those common to all the cairns of the moor, of angular pieces of granite of irregular shape and size, but mostly of such proportions as could be conveniently conveyed by hand. Lying near what appeared to have been the centre of the cairn were two other slabs of granite of much larger proportions. These were about four feet long by two and a half broad, and from eight to twelve inches thick, and appeared to us to have been the remains of the kistvaen, over which the stones were heaped. In one of these granite slabs my companion pointed out to me the presence of a large water-worn boulder pebble of sandstone. The pebble was about ten inches long by five wide at its longest and broadest diameters. It projected from the surface of the granite some two inches, or thereabout, but the form of the stone was suggestive of the larger part being still imbedded in the granite. The stone narrows somewhat towards one extremity, and at that extremity it is cracked across. The pebble is of blue sandstone, smooth but not polished on the surface, and at its contact with the granite exhibits no alteration of character.

The granite is of the grey colour that is so common on Dartmoor. It is of a coarse quality, containing numerous large crystals of felspar, locally known by the name of horses' teeth. The granite generally appears as if it had been deposited round the pebble, the crystalline character taking a bend somewhat conformable to the curves of the pebble.

The original rock from which the pebble was derived it is difficult to determine. In its general appearance the pebble bears a resemblance to those found on the pebble ridge of Northam Burrows, in North Devon. Unfortunately the granite slab itself is not in its natural bed. But since it forms part of an ancient monumental erection, and was placed in its present position long before the period when the transition of heavy weights could be easily accomplished, it is more than probable that the slab was procured from the neighbouring Tor.

The surface of the granite slab is very clean, being free from lichen and dendrital growths. But that it was weather-worn before it was placed in its present position, I think that there can be little doubt. Had it been artificially split, supposing that our ancient people had the knowledge and skill to have split the rock, they could not have done so without breaking the pebble as well.

Mr. Sorby demonstrated some five or six years since the presence of water in the hollows existing in crystals in granite, and thus came to the conclusion that granite must have been formed under a heat of less than 500 degrees. This heat is still, it appears to me, sufficient to convert all the water upon the surface of the earth into steam. But the rounded and smoothed character of the pebble of which we are speaking is evidence that water was present in its liquid state when it was rubbed into its present rounded form. It also demonstrates that the pebble was in existence as a hard rock before the granite was formed. Thus it would seem clear that when this granite was formed, the temperature of the earth must have cooled down to below the boiling point of water.

Plymouth, March. C. SPENCE BATE

* Since this was written I have seen Mr. Bonney's letter on Prismatic Ice. The wind during our walk was N.E. The quarry lay in a sheltered position. The temperature was probably as Mr. Bonney states.

Apparent Size of the Moon

THE public have what are called "views" on everything in the world and out of it. De Morgan well says that, on the question "whether there be volcanoes on the unseen side of the moon larger than those on our side," the odds are, that the first comer has a pretty stiff opinion in three seconds. Does anyone believe that the moon is made of green cheese? One woman hailing from this enlightened neighbourhood told my wife that she thought the moon was *made of blood!* She was not a Latter-day Saint, either.

If opinions differ as to the moon's constituents, they are not very divergent as to her apparent size. Mr. Thomas K. Abbott, of Trinity College, Dublin, in his singularly learned and able work called "Sight and Touch," 1864, p. 57, collates the opinions of Heraclitus, Aristotle, and Cicero, who all assign to the moon's apparent diameter the length of a foot. He quotes, too, from Arthur Collier's *Clavis Universalis* the expression, "the moon which I see is a little figure of light, no bigger than a trencher." The old-fashioned trencher was about a foot in diameter. The late Sir William Rowan Hamilton pointed out to me this passage in Descartes' "Dioptricæ," cap. VI. § xx.:—"Abque hoc patet ex eo quod Luna et Sol, qui sunt e numero corporum remotissimorum, quæ contueamur, ... *pedales ut plurimum, vel ad summum bipedales* nobis videantur," &c. Bishop Berkeley, as Philonous, asks Hylas, "Since, according to you, men judge of the reality of things by their senses, how can a man be mistaken in thinking the moon a plain, lucid surface, about a foot in diameter?" Many more such cases could be cited from ancients and moderns, all concurring in assigning a foot, or something between one foot and two feet, to the apparent diameter of the moon. Let me now cite two recent cases. A law-clerk, whose lay notions certainly owed very little to books, told me that the moon always appeared to him of the size of a door-handle. This would give, at most, a diameter of three inches. An eminent astronomer tried his daughter with the question. She replied that the moon looked to her about *half a degree*. He said, "Come, you learned that from astronomy; but answer as a girl of common sense." She now replied, "A small saucer." That would be some four or five inches in diameter.

I suppose I was too early spoiled by trigonometry to enter into the merits of this style of estimate. Look at the moon as I may, I cannot compare her to anything definite, as a door-handle, a saucer, or a trencher. Judging by the distance at which we ordinarily see such things in the use of them, they all seem to me to be enormously too large. Looking at the moon through my window, sitting three or four yards from it, I should guess that a wafer stuck on it would eclipse her. My own conviction is that in the ordinary estimate (or rather comparison), there is no reference, the most covert or subconscious, to any standard of distance. True, thirty-six or thirty-seven yards would be the distance at which Arthur Collier's trencher would subtend the same angle as the moon. But who thinks of this in connection with a trencher, which is usually under a man's nose or on the kitchen rack or shelf, at a distance not exceeding four or five yards? In another letter I will, if you will allow me, call attention to some of the *veræ causæ* which are probably concomitant in these popular estimates; and this I shall do in respect to the apparently augmented size of the moon's disc on the horizon. Meanwhile, let me ask as a preliminary to that inquiry, is it a matter of fact that *to the naked eye* the moon does subtend the same angle at the horizon as at positions near the zenith? I am unable to perform the measurement myself, not merely for want of a proper instrument, but by reason of the fact that I always see in the moon a multitude of discs partially overlapping each other, five of which I can distinctly count. It would be awkward to find that one was attempting to solve an imaginary problem, like the Royal Society over King Charles's fish.

Ilford, March 24 C. M. INGLEBY

THE "lurking idea" of Mr. G. C. Thompson, that the moon looks about the size of a fourpenny piece, seems to me to show that those views of it have made most impression on him which he has taken when standing a few feet from the window, when it would cover some such space: while others, with the one foot or two feet idea, have been more wrought upon by unconscious measurement of it against trees in the garden, or house chimneys along the street. I do not think we can get beyond this, in regard to a "personal equation." As to the apparent difference between the moon near the horizon and the moon in mid-sky, your correspondents have not yet referred to the theory that the felt degree of convergence of the eyes is one help toward measuring distance; which, however, soon ceases as the object is more remote, and the convergence insensible: and that, in looking at the moon along the earth's surface, we feel that she lies beyond this limit by comparison with the objects which intervene, while in looking up through free air there is no such gradation to guide us; that, therefore, we assign, unconsciously, a greater distance to her, *i.e.*, a greater "lurking idea" of estimated magnitude for the same apparent surface, in the former case than in the latter. I write from a dim recollection of one of Sir Sidney Smith's lectures on Moral Philosophy, but I suppose the notion is trite to experts. Is there anything in it? J. R.

Concomitant Sounds and Colours.

THE investigation of the points of resemblance between two sciences, has its value and assists the development of both. Music gains by being thus raised from a mere sentimental recreation to the dignity of a science, but the science of colours may perhaps gain even more than music by the comparison, and this because the ear, in most persons, can distinguish with more precision a discord in sound, than the eye can in colour.

In the most ancient times it was well known that concomitant sounds produce a resultant whose vibrations are generated by the interference of the sonorous waves of the primaries. This physical fact was not only known but employed in the construction of Gregorian *Cantilenas*, whose succession of intervals shows a deep penetration of this truth.

The law of combination of the vibrations of concomitant sounds may be stated thus:—The resultant of two sounds has, as its number of vibrations, the difference between those of its primaries. Also any number of sounds combined two and two together, the 1st with the 2nd, the 2nd with the 3rd and so on, will form a series of resultants, which similarly combined two and two together form a second series of resultants; so that (continuing this process) we finally arrive at a single resultant which is that of the original combination. This law has been tested experimentally by Hallström and Scheibler. I considered that it might be useful to express this law by a general formula, so I will give it in this place.

If we have n sounds whose vibrations are $x_1, x_2, x_3, \ldots x_n$, all in ascending order as to *pitch*, then the resultant will be

$$R = (x - 1)_{n-2} (x_2 - x),$$

where the suffixes must be treated as indices, and $(x - 1)_{n-2}$ expanded according to the binomial theorem.

R is not, however, a resultant in the strict sense requiring the vanishing of the primary sounds; it might, perhaps, be better called the *Residuant* of the combination. It is thus the measure of the imperfection of the combination which is, more or less, a discord according as R is less or more nearly related to the primaries.

If we apply this formula we shall easily see that the tonic and subdominant generate a note two octaves below the subdominant: for example, C and F generate $\frac{F}{4}$. Also CEG, CGC^2, and CDE and all similar combinations in which the vibrations stand in arithmetical progression generate no residuant, hence, combinations of this class are perfectly consonant and are called by Boethius *equisonal concords*. (The combination CDE is discordant enough on a modern instrument, but I mean CDE tuned perfectly without temperament.)

Supposing then that an impression is made upon the retina by two or more colours in juxtaposition, analogous to that produced on the auditory nerve by two or more simultaneous sounds. We shall perceive that two complementary colours placed side by side ought to increase in intensity that one whose vibrations are the most rapid. Red and green, for instance, should give intensity to the green, since D and G generate $\frac{G}{4}$. Moreover, the colours corresponding to the *equisonal concords* ought to give us the most harmonious combinations; these are they:—

Violet	placed between two		yellows,
Red	,,	,,	greens,
Orange	,,	,,	blues,
Yellow	,,	,,	indigo-blues,
Green	,,	,,	violets,
Blue	,,	,,	reds.

Yellow and indigo-violet ought always to be discordant, as they correspond with the discord F B or *tritone*. Again—

Violet, orange, green Yellow, blue, violet Indigo-blue, red, yellow Violet, red, orange Yellow, green, blue Indigo-blue, violet, red.	All correspond to equisonal concords and should form perfectly harmonious combinations.

It will be noticed that these tints must be precisely of the same shade as those in Newton's image; the slightest variation would destroy the harmony of colour. I have no doubt if pigments were made of tints identical with the ring-colours, the beauty of these combinations would be appreciated by all who used them.

It will be noticed also that D, F, A generate $\frac{B\flat}{16}$, or red, yellow, blue generate indigo-blue. A, C, E generate $\frac{F}{16}$, or blue, violet, orange generate yellow. I need not increase the length of my paper by more examples, but leave the field open to all who choose to test the above formula as regards its application to combinations of colour.

In conclusion, I wish to make the following suggestion.

Should it be admitted that the musical scale, in its perfect division into intervals under the law of harmonical progression, finds its counterpart in Newton's rings rather than in the prismatic spectrum, would not a spectroscope, constructed so as to give the image of these rings, be a more perfect instrument for the comparison of colours than that in present use? We might also have a double spectroscope, capable of giving the images of the secondary rings produced by the refraction of homogeneous light, the cube roots of whose diameters give the series which corresponds to that of the musical scale.

We should in this way be able to know the melody which corresponds to the light of any particular star, provided that the light be strong enough to produce the images of the secondary rings. W. S. OKELY

Rome, February 18

Analogy of Colour and Music

AT the close of my short article on the Analogy of Colour and Music, published in your journal of January 13, I ventured to ask for the opinion of physicists on the subject. Accordingly I, for one, am much indebted to the many able contributors who thereupon addressed you. The correspondence having apparently ceased, I will now ask your permission to say a few words.

Although I do not attach too much importance to the closely approximate ratios, given in my paper, between the wave-lengths of colour and the notes of the diatonic scale, yet I think nothing said by your correspondents seriously affects my main argument.

The most important objection is that urged by Mr. Monro (NATURE, No. 14), who regards the correspondence of the two ratios as a mere coincidence, depending on the mode by which Prof. Listing obtained his scale of wave-lengths of the colours. By an ingenious calculation, Mr. Monro shows that Listing most probably "divided his spectrum into seven equal parts upon some scale which varies inversely as the wave-lengths; . . . so that it nearly corresponds with the ratios of the musical scale because these approximately form a harmonic progression." When I wrote my article I had not read Listing's paper, but, as stated, quoted his numbers from a recent memoir by Thalen. The perusal of his original paper shows me that Listing obtained his numbers in the following manner, which I think Mr. Monro will see confirms his calculation, but overrides his criticism:—

Employing pure spectra, and using every precaution, Listing experimentally determines the transition places and the central region of each colour, Fraunhofer's lines being used as landmarks. The observations are repeated upon the normal spectrum obtained by diffraction, and are checked by the independent observations of others, and by repetitions at different times. In this way the remarkable fact is disclosed that the numbers of vibrations at the transition spots form an *arithmetical progression* throughout the entire series of colours. For reasons given he adopts the following scale of colours—brown, red, orange, yellow, green, cyanogen, indigo, and lavender, and states as a law that this series can be physically expressed by an arithmetical progression of eight numbers, in which the last is the double of the first. He then proceeds to discover the constant factor by which this series can be turned into absolute values. After considerable care, and upon grounds fully detailed, he selects $48\frac{1}{2}$ billions as the number of vibrations per second expressing the range of each colour. The possible error he shows to be $\pm$ 0·038—taking billions as unity—and this, though apparently a large error, is actually less than $\frac{1}{7}$th of the interval between the two D lines.

The number of vibrations corresponding to the extreme limit of colour at the red end, he fixes, upon Helmholtz's and Angstrom's authority, at 363·9 billions per second, or a wave-length of 819·8 millionths of a millimetre. By adding to the former number half the colour interval—namely, $24\frac{1}{4}$ billions—the normal centre of the first colour is obtained; $48\frac{1}{2}$ billions added to that gives the centre of the next colour, and so on. These, and also the limits of each colour, are tabulated along with the corresponding wave-lengths.

Listing closes his paper with the statement of a general law, that while the successive vibrations of the series of colours in the spectrum form an arithmetical progression, the same is also true of the logarithms of the vibrations corresponding to each musical note in the so-called chromatic scale. Hence he concludes that although physiologically and pyschologically there may be differences, yet there is an indisputable *physical* basis for the analogy between tones and colours. From this very imperfect outline it will be seen that the entire memoir is a remarkable one, and I am surprised no translation of it has appeared. It is certainly the most important contribution to the analogy that I have met with, and renders my little paper on the subject quite unnecessary.

Mr. Okely, writing to the next number of NATURE, gives some additional evidence in favour of the analogy, but thinks my process of taking the mean of the limiting wave-lengths of each colour, in order to obtain the average wave-length, is "very rough." Mr. Okely does not tell us what he would do in such a case, but turns aside to become the champion of the widths of Newton's rings, charging me with having treated too summarily this old and famous ally of the analogy. But to this, my next critic, Mr. Sedley Taylor, replies, although other considerations also influenced me in neglecting this analogy.

Mr. Taylor, however, believes that he has deprived my comparison of any serious importance, for the following reason:—In the musical scale, he observes, "a very slight departure from accurate pitch in any concord provokes a harsh dissonance;" but "any part of any one colour-division produces an equally harmonious effect on the eye," because "in the spectrum there is very little, if any, change of tint except close to the extremities of any one colour." Whilst it would be certainly unwise to push the analogy too far, I think Mr. Taylor is here mistaken. There *is* a very material difference of tint in different parts of any one colour in the spectrum. Regarded alone, any region of the spectrum, like any single musical note, is, of course, equally agreeable; but it is not the case that an equally harmonious effect on the eye is produced by the combination of *any* part of any one colour-division with some other colour.

Mr. Stuart, in an interesting letter, points out a close relationship, discovered by Prof. Mossotti, between the intensities of the light in different parts of a normal spectrum and the notes of the diatonic scale. Finally, Dr. Chaumont, in an early part of this discussion, showed, what indeed had been noticed elsewhere, that if the ratios I give be accepted, then the once-called primary colours, red, yellow, and blue, correspond to the notes of the common chord; whilst the modern triad, red, green, and blue, correspond to the tonic, sub-dominant and dominant, that is to say, to the three notes which in music constitute the fundamental base of the scale.

In addition to what has been brought forward in this correspondence, there are some valuable remarks on the analogy in one of Dr. Thomas Young's memoirs, "Philosophical Transactions, 1802"; in Chevreul's work on the "Principles of Harmony of Colour"; in a recent brochure by Dr. Macdonald on "Sound and Colour"; and, lastly and chiefly, in §19 of Helmholtz's "Physiological Optics." In this last a list is given of authorities who have written on the subject since the time of Newton.

Reviewing what has been done in this matter, there are therefore, I believe, many good grounds for asserting the existence of a physical basis for the analogy between colour and tone. Opposed, it is true, are many mental differences: such, for example, as that of the judgment, which is far more prompt and correct in determining a colour than a note; then also colour primarily involves only the conception of *space*, music the conception of *time*. Nevertheless against all this we may place the facts that the source of harmony in colour, as in music, is purely a question of *relative* impressions; and a painting and a melody evoke a succession of ideas that have a remarkable similarity.

Woodlands, Isleworth, March 13 W. F. BARRETT

THE METROPOLITAN MAIN DRAINAGE

THE magnitude of the underground works of London is scarcely understood by the public in general. They occasionally hear of this or that sewer or pumping station being completed, but as the greater portion of them is hidden beneath the surface of the ground, nothing but a personal inspection during the process of construction can give any adequate idea of the vastness and intricacy of our drainage system. The following facts, collected from the engineer's papers on the subject and reports to the Metropolitan Board of Works, may present to the mind some notion of these great works. There are about 1,400 miles of sewers in London, 82 miles of which are intercepting sewers of "The Main Drainage." The area drained by the intercepting sewers is about 100 square miles. The total amount of sewage and rainfall which they will carry off is 63 million cubic feet per day, or equal to a lake as large as Hyde Park four feet in depth. There have been consumed in their construction about 340 million bricks, and upwards of 900,000 cubic yards of concrete. The total engine-power employed is 2,380 nominal horse-power, but this will soon be increased by about 400 horse-power on the construction of the Pimlico pumping station. The cost of these works when completed will be a little over 4,000,000*l*.

The accompanying map shows the position of the main intercepting sewers, pumping stations, outfalls, &c., and it will be seen that they run east and west, the reason for this being that the main sewers in existence before the construction of the main drainage, emptied themselves into the Thames, running more or less at right angles to it, and consequently, by constructing the intercepting sewers parallel to it, they would cross all the main outlets and cut off their sewage. In carrying out this scheme the great object to be kept in view was to discharge as much sewage as possible by gravitation, in order to avoid the great expense of pumping. To this end three lines of sewers are constructed on each side of the Thames, called respectively the High, Middle, and Low Level sewers, together with their branches. On the north side the High and Middle Level discharge their sewage by gravitation; that of the Low Level, which joins them at Abbey Mills, being pumped up a height of 36 feet into the Upper Level, when all three flow through the Northern Outfall Sewer to the reservoirs at Barking, and discharge their contents into the river by gravitation. On the south side the Low Level sewer meets the High and Middle Level at Deptford, where its contents are pumped a height of 18 feet into the Outfall Sewer, which carries the three streams to Crossness, where they can discharge by gravitation; but as this can only be done at low water, they are generally pumped a height of 20 feet into the reservoirs. Such is the general outline of the Main Drainage scheme, as being carried out by Mr. Bazalgette, the able engineer of the Metropolitan Board of Works.

Before commencing these works a large number of experiments and observations had to be made in order to decide the numerous knotty points that suggested themselves: such as the determination of the position of the outfalls, the shape and inclination of the sewers, the amount of sewage and rainfall to be provided for, the amount to be pumped, &c., &c., each requiring a great amount of labour and study. The results arrived at, and which were acted upon, may be summed up as follows:—That it was necessary to take the sewage down the river as far as Barking Creek; that its lowest mean velocity should be $1\frac{1}{2}$ miles per hour; the quantity of sewage to be intercepted was, in populous districts, 750,000 gallons per square mile per day; the amount of rainfall to be provided for was a quarter of an inch per day; the form of the sewers was to be circular, and the sewage was to be discharged at or about the time of high water. The above are a few of the principles upon which the Main Drainage of London is based.

THE WORKS.—The High Lever Sewer North commences near Hampstead Hill, where it intercepts the Fleet Sewer. It varies in size from 4ft. in diameter to 9ft. 6in. by 12ft., is about seven miles long, and drains an area of about ten square miles in its course from Hampstead to its junction with the Middle Level at Old Ford. It passes under three railways, the New River, and Sir George Duckett's Canal, in the last case the top of the sewer is within 2 ft. of the waters in the canal.

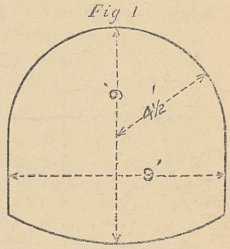
The Middle Level Sewer commences near Kensal Green and flows to its junction with the High Level at Old Ford. The total length of the main line, together with the Piccadilly branch, is about $11\frac{1}{2}$ miles; the area drained is $17\frac{1}{2}$ square miles; in form it varies from 4ft. 6in. by 3ft. to 9ft. 6in. by 12ft. Six miles of this sewer were constructed in tunnel. In its course it passes under two canals and one railway, and over the Metropolitan line, across which it was carried without interfering with the traffic, the bottom of the sewer being only a few inches above the chimneys of the engines.

At the junction of the High and Middle Level sewers there is a large "penstock" and "weir" chamber, for the purpose (should occasion demand) of letting storm waters or all the sewage flow into the River Lea. It is 150ft. in length, 40ft. wide, and 36ft. high, and fitted with five large penstocks or valves, which are raised or lowered by machinery.

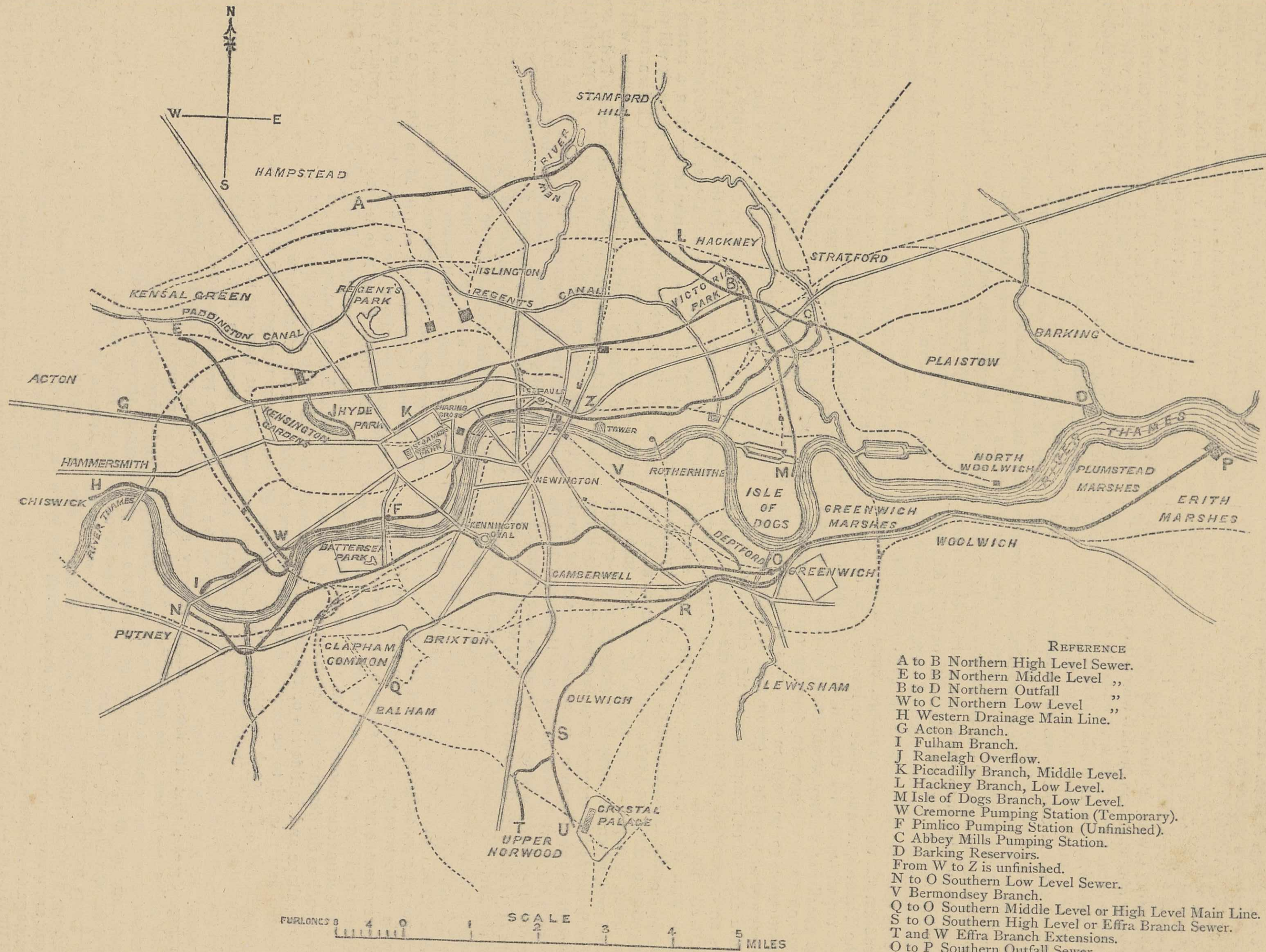
The Low Level North will commence by a junction with the Western Drainage near Cremorne, and flow along the intended Chelsea Embankment to Pimlico, where its contents will be pumped up a height of $17\frac{1}{2}$ feet into the remaining length, which will flow *viâ* the Thames Embankment to Abbey Mills. Its length, including branches, is about $12\frac{1}{4}$ miles, and it varies in size from 6ft. 9in. to 10ft. 3in. diameter. The area drained by it, including the Western Drainage, the sewage of which it receives, is $24\frac{1}{2}$ square miles. This sewer is by far the most interesting on the north side, in consequence of the large works in connection with it. In the first place, it receives the drainage of the Isle of Dogs, in consequence of which, what was formerly little better than a marsh is now perfectly habitable; in the second place, it will pass along the two new embankments, viz., Chelsea and the Northern Thames; and in the third, its contents will be twice lifted by steam power, the lift at Pimlico being over 17ft., and that at Abbey Mills 36ft. At Blackfriars Bridge, where it intercepts the Fleet, there will be a large penstock-chamber fitted with four penstocks and as many tide flaps, to prevent the return of the tide at high water. A large portion of this sewer, namely, that extending from Blackfriars to Abbey Mills, will be executed for the most part in tunnel.

The Northern Outfall Sewer, which receives the whole of the sewage on the north side of the Thames, is totally unlike any other portion of the Main Drainage system, being entirely above the ordinary level of the ground. The first length, from B to C (see plan), consists of a double line of sewers, each being 9ft. by 9ft. of the section shown in Fig. 1 running parallel to each other; at C it receives the contents of the Low Level, and from this point to the reservoirs at Barking it is of the section shown in Fig. 2, which is three parallel culverts 9ft. by 9ft. built upon a concrete foundation and covered by an earthen embankment, the top of which would serve as a road or railway. In its course to Barking it crosses the River Lea and six streams, four railways, and ten roads. Fig. 3 gives a general idea of the method adopted for carrying the sewers over the streams, railways, &c. It consists of three cast-iron culverts carried between four wrought-iron girders, the top being covered with cast-iron plates, which support the ballasting, roadway, &c.; the parapets are ornamented with cast-iron mouldings.

Fig 1



The contents of this sewer are received by the reservoirs at Barking. These are about $9\frac{1}{2}$ acres in extent, and $16\frac{3}{4}$ft.



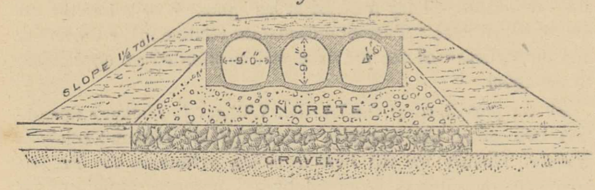
THE MAIN DRAINAGE OF LONDON

REFERENCE

A to B Northern High Level Sewer.
E to B Northern Middle Level ,,
B to D Northern Outfall ,,
W to C Northern Low Level ,,
H Western Drainage Main Line.
G Acton Branch.
I Fulham Branch.
J Ranelagh Overflow.
K Piccadilly Branch, Middle Level.
L Hackney Branch, Low Level.
M Isle of Dogs Branch, Low Level.
W Cremorne Pumping Station (Temporary).
F Pimlico Pumping Station (Unfinished).
C Abbey Mills Pumping Station.
D Barking Reservoirs.
From W to Z is unfinished.
N to O Southern Low Level Sewer.
V Bermondsey Branch.
Q to O Southern Middle Level or High Level Main Line.
S to O Southern High Level or Effra Branch Sewer.
T and W Effra Branch Extensions.
O to P Southern Outfall Sewer.
O Deptford Pumping Station.
P Crossness Pumping Station and Reservoirs.

deep, covered in by brick arches, the floor being paved with stone. Attached to each is an outlet fitted with penstocks, &c. The Abbey Mills pumping station is a work of such magnitude and importance that but a scanty idea can be given of it here: suffice it to say, that there are eight engines employed, of a gross power of 1,140 horse-power, which work sixteen double-acting pumps of 3ft. 10½in. diameter and 4½ft. stroke. The engines are supplied by sixteen boilers, each being 8ft. diameter and 30ft. long. The engine and boilers, coal stores, &c., are enclosed in a fine block of buildings beautifully decorated.

Fig. 2.

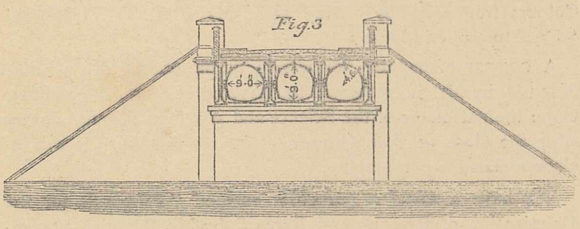


The Southern High Level, or Effra Branch Sewer, commences at Dulwich, and flows in a south-easterly direction to a junction with the Middle Level or High Level main line at New Cross. It is 4½ miles in length, ¼ mile of which is in tunnel. It varies in size from 7ft. diameter to a form similar to one of the culverts in Fig. 2, only the dimensions are 10ft. 6in. by 10ft. 6in.

The Southern Middle Level, or High Level main line, commences at Clapham, and flows under Deptford Creek to the Outfall Sewer. It, together with the High Level, drains an area of about 20 square miles. It is carried under Deptford Creek by four 3ft. 6in. diameter iron pipes. Overflows for storm waters are provided, which discharge into the Creek, if necessary.

The Low Level Sewer South commences at Putney, and flows to Deptford pumping station, where its contents are pumped up a height of 18 feet, by four engines 125 horse-power each, into the Outfall Sewer. Its length is about 10 miles, and it drains an area of about 20 square miles, the greater portion of which is considerably below high-water mark. It varies in size from one culvert 4 ft. in diameter, to two, each 7ft. by 7ft. The soil in which it was executed was exceedingly treacherous in places, being greatly charged with water: in one case sufficient engine-power had to be employed to pump ten million gallons per day, in order to keep the works clear of water. The Bermondsey branch of this sewer is two miles in length,

Fig. 3



and from 5ft. to 5ft. 6in. diameter. It joins the Low Level at High Street, Deptford.

The Southern Outfall Sewer, which receives the contents of the High, Middle, and Low Level sewers, flows from Deptford to the reservoirs at Crossness. Unlike the Northern Outfall, it is underground for its entire length. It is 11ft. 6in. diameter, and 7¾ miles in length. One mile of it was constructed in tunnel under the town of Woolwich. The outfall of this sewer is of such a level as to allow of the sewage being discharged at low water, but it will be pumped into the reservoirs, and there stored till high water.

The Crossness pumping station reservoirs are situated on the northern side of Erith Marshes at the point marked P on the map. They extend over 6½ acres. The reservoirs are similar to those at Barking. There are four beam engines employed of 125 horse-power each, which drive eight pumps, each pump having four compound plungers. The engines are supplied by twelve Cornish boilers 30ft. long and 8ft. diameter. The ordinary amount of sewage to be lifted is about 60,000 gallons per minute, the lift varying from 10ft. to 30ft., which necessitated a peculiar construction in the pumps. The sewage is delivered from the pumps into the reservoirs till high water, when it is discharged into the river. The foundations for the reservoirs, &c., had to be sunk 25ft. below the surface, as the ground consists of peat and sand. On the top of the reservoirs are built the manager's, schoolmaster's, and labourers' cottages, coal stores, school, and workshops, the centre space being laid out as ornamental gardens, the whole forming quite a village of some hundred inhabitants.

NOTES

THE last number of the Proceedings of the Royal Society contains the names of the 53 candidates, from among whom, 15 will be selected by the council for election into the society. The list is a varied one, and there is as usual a large number of medical candidates—21; Art and Literature being also represented. Here is the list:—William Baker, C.E., E. M. Barry, R.A., Rev. Francis Bashforth, B.D., B. E. Brodhurst, F.R.C.S., Samuel Brown, P.I.A., James Brunlees, C.E., F. T. Buckland, M.R.C.S., G. W. Callender, F.R.C.S., Commander W. Chimmo, R.N., F. Le G. Clark, F.R.C.S., Henry Dircks, Alex. Fleming, M.D., P. Le Neve Foster, Sir Charle Fox, C.E., William Froude, T. M. Goodeve, E. H. Greenhow, M.D., E. T. Higgins, M.R.C.S., Rev. Thomas Hincks, Charles Horne, Rev. A. Hume, LL.D., James Jago, M.D., W. S. Jevons, George Johnson, M.D., M. K. King, M.D., J. A. Langridge, C.E., N. S. Maskelyne, M. T. Masters, M.D., Major F. G. Montgomerie, R.E., Alfred Newton, Andrew Noble, Thomas Nunnely, F.R.C.S., E. L. Ormerod, M.D., Captain Sherard Osborn, R.N., Rev. Stephen Parkinson, B.D., Captain R. M. Parsons, R.E., W. O. Priestly, M.D., C.B. Radcliffe, M.D., W. H. Ransom, M.D., E. J. Reed, C.B., W. J. Russell, Ph.D., R. H. Scott, John Shortt, M.D., Edward Thomas, C. F. Varley, C.E., G. F. Verdon, C.B., Augustus Voelcker, Ph.D., Viscount Walden, P.Z.S., G. C. Wallich, M.D., A. T. H. Waters, M.D., Samuel Wilks, M.D., Captain C. W. Wilson, R.E., and John Wood, F.R.C.S.

THE *Pall Mall Gazette* has very properly called public attention to Lord Kinnaird's imputation of dishonesty brought against the late Master of the Mint, the lamented Graham. The good old rule, *de mortuis nil nisi bonum*, is one with which Lord Kinnaird does not seem to be acquainted; it is charitable, indeed, to suppose that he sinned in ignorance. He probably also does not know that Graham was a far greater man than he, and that Graham's name will live long after Lord Kinnaird's has been forgotten.

ON Saturday the members of Working Men's Clubs visited the British Museum, under the guidance of Professor Owen and Mr. Henry Woodward. Professor Owen explained the nature of the extinct animals. The next visit will be made to the National Gallery, under the guidance of Mr. Francis Turner Palgrave.

THE examinations for the gold and bronze medals offered as prizes for proficiency in Physical and Political Geography, by the Royal Geographical Society, were held on Monday last, 28th inst. Forty-one schools had been invited to compete, out of which number nineteen accepted, sending a total of fifty-nine candidates: thirty-four in Physical, and twenty-five in Political Geography. The names of the successful candidates will be announced at the ensuing anniversary meeting.

IN the introductory lecture to his course of Comparative Anatomy, delivered at the Royal College of Surgeons, Prof. Flower discusses the objection to the theory of the origin of species by the process of natural selection, founded on the existence of corresponding types of structure in the Monodelphous and Didelphous sections of mammalia. He considers the probabilities, instead of being against the independent origin of such similar structures, are exceedingly in their favour. The lecturer lays down as a valuable guiding principle in morphological studies, that when we wish to discover the distinguishing characters between different organisms, it is necessary to examine them in their most fully developed condition; if, on the other hand, our object is to trace their resemblance, their intimate relationships, we must study them in their early embryonic stages.

THE *Revue des Cours Scientifiques* reports that the Sars subscription now approaches 9,000 francs. Subscriptions have been received from Germany, Hungary, and America. In France nearly all the professors of higher instruction in science have subscribed to it.

WE have received from the Royal Society a report of Prof. Duncan's important paper on the Madrepores of the *Porcupine* Expedition. We shall return to this subject after completing Dr. Carpenter's report of the more general results of the expedition.

PROF. TYNDALL will have much to answer for in the results that may be expected from the spread of his "dust and disease" theory. It is stated by the *Athenæum* that a new idea has been broached in a recent lecture by Mr. Bloxam, the lecturer on chemistry to the department of artillery studies. He suggests that the committee on explosives, abandoning gun cotton, should collect the germs of small-pox and similar malignant diseases, in cotton or other dust-collecting substances, and load shells with them. We should then hear of an enemy dislodged from his position by a volley of typhus, or a few rounds of Asiatic cholera. We shall expect to receive the particulars of a new "Sale of Poisons" Act, imposing the strictest regulations on the sale by chemists of packets of "cholera germs" or "small-pox seed." Probably none will be allowed to be sold without bearing the stamp of the Royal Institution, certifying that they have been examined by the microscope and are warranted to be the genuine article.

WE have received from Professor H. A. Newton, of Yale College, a report on the meteor-display of November last, from which it appears that the cloudy weather prevented continuous observations in most parts of the United States. In the few stations, however, where the skies were clear, the observers furnish ample testimony to the appearance of unusual numbers of meteors on the morning of Nov. 14, the display continuing for several hours. The most successful observations were made at Pensacola, Florida, where they were observed in extraordinary numbers from 1·15 till dawn, most numerously between 3 and 4 a.m.; at Santa Barbara, California, where 556 meteors were observed between 1·18 and 3·43 a.m.; and at Fredericton, New Brunswick. Prof. Newton remarks that if the whole number coming during the twelve or more hours of the display had been condensed into an hour or two, as in 1866, we should have had a like though not equally brilliant shower.

THE Council of the University of Otago, New Zealand, announces that it is now prepared to consider applications from candidates for two of the professorial chairs to be instituted for classics, including Greek, Latin, the English language and literature; and for Mathematics and Natural Philosophy. The salary attached to each chair will be 600*l.* per annum, which will commence to run from the date of embarkation, besides the class fees, which have been fixed at 3*l.* 3*s.* for attendance on each professor, per term of six months, commencing in the beginning of May of each year. An adequate allowance will be made for passage money and outfit. All candidates must be graduates of some established and recognised university. No religious test will, however, be required from any person to entitle him to hold office in the university, or to graduate or to hold any advantage or privilege thereof. Applications from candidates must be addressed to John Auld, Esq., W.S., Edinburgh, agent of the Province of Otago in Britain, and must be accompanied with testimonials and certificates. They must be in his hands on or before the 1st day of April next. Further information relative to the university and the statistics of the province will be afforded on application to the agent.

BY the provisions of the late Dr. William J. Walker's foundation two prizes are annually offered by the Boston Society of Natural History for the best memoirs, written in the English language, on subjects proposed by a Committee appointed by the Council. For the best memoir presented, a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased, at the discretion of the Committee, to one hundred dollars. For the memoir next in value a sum not exceeding fifty dollars may be given; but neither of these prizes is to be awarded unless the papers under consideration are deemed of adequate merit. Memoirs offered in competition for these prizes must be forwarded on or before April 1st, of the years specified below, prepaid and addressed "Boston Society of Natural History, for the Committee on the Walker Prizes, Boston, Mass." Each memoir must be accompanied by a sealed envelope enclosing the author's name, and superscribed by a motto corresponding to one borne by the manuscript. Subject of the Annual Prize for 1870: "The reproduction and migration of *Trichina spiralis.*" Subject of the Annual Prize for 1871: "On the mode of the natural distribution of plants over the earth."

THE following are the Afternoon Scientific Lectures of the Royal Dublin Society, to be delivered during April and May, at four o'clock on Saturdays, in the Lecture Theatre. April 2nd, Dr. J. Emerson Reynolds, "On Ozone, Nature's bleaching agent and disinfectant." April 9th, Dr. H. Minchin, "On some interesting phenomena of sound. April 16th, Prof. E. Hull, "On the extension of the Coal-fields of England under the newer formations." April 30th, Prof. Wyville Thomson, "On the Cruise of the *Porcupine.*" May 7th, Mr. H. N. Draper, "On Colours from coal-tar." May 14th, Mr. C. R. C. Tichborne, "On Atmospheric Dust."

A COURSE of lectures for women on the science and practice of music, by Mr. Sullivan, will be delivered at South Kensington, under the patronage of the Science and Art Department, shortly after the close of Prof. Oliver's course on botany. It will include a class for part-singing.

WE have received a pamphlet entitled "Proposals for the Illumination of Beacons and Buoys," by Mr. Thomas Stevenson, F.R.S.E. The author discusses the different sources of illuminations for beacons and buoys, and the different applications of sound for warnings during fogs. The subject is a highly important one, and we purpose to return to it at an early period.

ACCORDING to the *British Medical Journal*, Sir W. Fergusson is about to resign the chair of systematic surgery at the Medical School of King's College, and Mr. Partridge his appointment as surgeon to the hospital. Sir William will, however, be appointed professor of clinical surgery, while Mr. Partridge still retains the chair of anatomy. Mr. Wood is expected to succeed Sir W. Fergusson as professor of systematic surgery;

he will also be appointed to the rank of full surgeon to the hospital. Two vacancies will therefore occur, that of assistant-surgeon to the hospital, and demonstrator of anatomy.

WE learn from *Van Nostrand's Eclectic Engineering Magazine* (New York) that the Darien Canal project is reviving. The United States steamer *Nipsic*, attached to the South Atlantic squadron, is under orders to proceed to the Isthmus of Darien to make surveys and explorations, with a view to determine the best location for an inter-oceanic canal. A similar survey on the Pacific shore of the Isthmus will be made at a future day.

M. FAVRE has recently detected evidences of the glacial period in the Caucasus, and M. Ed. Collomb finds traces, in the form of moraines and erratic blocks, of its having existed with great severity in the central plateau of France. This plateau forms an almost circular geological island 300 kilometres in diameter ; its altitude increases progressively from north to south, and it is terminated on the south and west side by a barrier, the highest points of which, the Mézenc, the Plomb du Cantal, and the Mont d'Or rise to a height of from 1,750 to 1,900 metres (5,700 to 6,200 feet), above the level of the sea.

THE sense of taste has rarely been submitted to scientific examination, or at all events has attracted far less attention than its sister senses of sight and hearing, perhaps on account of the impossibility of treating it mathematically. That it differs to a remarkable extent in different individuals is, however, as every culinary artist would acknowledge, a matter of fact ; and it is also well known that it is capable of extraordinary cultivation in some men, as shown by wine- and tea-tasters obtaining lucrative posts from the delicacy of their discrimination. Recently Dr. Keppler has published a paper in Pflüger's "Archives of Physiology," in which he gives the details of a number of experiments he performed with a view of determining the limits of gustatory discrimination for sapid substances in various degrees of concentration. In these experiments he first made a standard solution, and then successively employed weaker or stronger solutions, which were tasted with due precautions, sometimes before and sometimes after the standard solution, until no perception of flavour was distinguished. The substances he selected were common salt, quinine, phosphoric acid, and glycerine, all of them, be it observed, destitute of odour, which plays so important a part, often overlooked, in our ideas of the flavour of particular objects. In one series of experiments the solutions were taken freely into the mouth, rolled over all parts of the membrane lining it, and then discharged. In a second series the solutions, were more carefully applied to the surface of the tongue alone by means of a camel's hair brush. It was found in both cases that when a difference of 2·5 per cent. existed between the standard solution and the experimental one, the observers were able to form a correct judgment on the point that there *was* a difference in 53 per cent. of the trials, but when there was a difference between the two solutions amounting to 10 per cent., the answers were rightly given in 80 per cent. of all the trials. A more correct judgment was given when the standard solution was tasted before than after the experimental one with common salt and quinine, and the acuteness in the perception of a difference was greater when the trial solution was stronger than when weaker, but the opposite held good for the other substances.

WE learn from the *Gardener's Chronicle* that the Royal Horticultural Society has decided to retain a portion of the old Chiswick garden, comprising the ground occupied by the glass-houses, and extending sufficiently eastwards and southwards to include the large vinery and the fruit-room.

M. DUCLAUX has lately been experimenting on the effect of certain gases in retarding the incubation of silkworms' eggs. He has also been trying the effect of cold upon the same organisms, and finds that instead of retarding the period of incubation, it accelerated it : in fact, that eggs laid in autumn and left to themselves would only incubate in spring ; but if subjected to the action of a freezing mixture for forty days, they would hatch into larvæ immediately afterwards, on being submitted to the action of a gentle heat. If these experiments are confirmed, M. Duclaux will have undoubtedly discovered an entirely new principle in physiology : that cold has a vivifying influence. Hitherto physiologists have always believed that its action was diametrically opposite.

THE journal of the Proceedings of the Asiatic Society of Bengal for January has an interesting article by Dr. F. Skoliczka on the Kjökenmöddings of the Andaman Islands.

THE *Journal of the Scottish Meteorological Society* has some interesting papers on the cold of last summer in Ireland, and upon the thunderstorms of Scotland. The part also contains a report on the Meteorology of Scotland and a minute of the meeting of the Council.

THE *American Gas Light Journal* reports that at a recent meeting of the Lyceum of Natural History of New York, Mr. Loew stated that ozone is produced copiously by blowing a strong current of air into the flame of a Bunsen's burner. He also communicated that he had observed the decomposition of sulphurous acid with production of sulphuric acid and deposition of sulphur, when an aqueous solution of the gas was exposed for two months to sunlight.

THE hardness of metals may now be ascertained by the aid of an instrument invented by a French engineer. It consists of a drill turned by a machine of a certain and uniform strength. The instrument indicates the number of revolutions made by the drill. From this, compared with the length of the bore-hole produced, the hardness of the metal is estimated. It is said that a great proportion of the rails now employed in France are tested by this instrument.

ON THE TEMPERATURE AND ANIMAL LIFE OF THE DEEP SEA*

III.

AN enormous addition has been made to the list of British *Echinodermata* by the discovery in our own seas of a number of species which had been previously known only as Norwegian or Arctic ; and these often occurred in extraordinary abundance. One of the most interesting of these was the large and beautiful feather-star, the *Antedon* (*Comatula*) *Eschrichtii*, hitherto known only as inhabiting the shores of Greenland and Iceland, but now found over all parts of our cold area. On the other hand, the influence of temperature was marked not only by the absence of many of the characteristically southern types of this group, but by the dwarfing of others to such an extent that the dwarfed specimens might be regarded as specifically distinct, if it were not for their precise conformity in structure to those of the ordinary type. Thus the *Solaster papposa* was reduced from a diameter of six inches to two, and had never more than ten rays, instead of from twelve to fifteen ; and *Asterocanthion violaceus* and *Cribella oculata* were reduced in like proportion. But, in addition, several echinoderms have been obtained which are altogether new to science, most of them of very considerable interest. The discovery, at the depth of 2,435 fathoms, of a living crinoid of the Apiocrinite type, closely allied to the little rhizocrinus (the discovery of which by the Norwegian naturalists was the starting-point of our own deep-sea explorations), but generically differing from it, cannot but be accounted a phenomenon of the greatest interest alike to the zoologist and the palæontologist. Another remarkable representative of a type supposed to have become extinct, occurred at depths of 440 and 550 fathoms in the warm area; being a large *echinidan* of the *diadema* kind, the "test" of which is composed of plates separated from one another by membrane, instead of being connected by suture, so as to resemble an armour of flexible chain-mail, instead of the inflexible cuirass with which the

* A Lecture delivered at the Royal Institution (continued from p. 540).

ordinary echinida are invested. This type bears a strong resemblance to the very singular fossil from the white chalk, described by the late Dr. S. P. Woodward, under the name of *Echinothuria floris*. Specimens were also obtained, both in the first and third cruises, of a most interesting *clypeastroid*, which is closely allied to the *infulaster*—specially characteristic of the later chalk.* These constitute only a sample of the interesting novelties belonging to this group, which our explorations brought to light.

Besides further additions to the remarkable group of vitreous sponges, which were made in the area over which the Globigerina-mud extends, a peculiar and novel form of sponge was found to be one of the most generally diffused inhabitants of the cold area. This sponge is distinguished by the possession of a firm branching axis, of a pale sea-green colour, rising from a spreading root, and extending itself like a shrub or a large branching gorgonia. The axis is loaded with siliceous spicules; and spicules of the same form are contained in the soft flesh which clothes it.

The foraminifera collected in the *Porcupine* expedition present features of no less interest, though their scale is so much smaller. The enormous mass of Globigerina-mud (sometimes almost pure, sometimes mixed with sand) that everywhere covers the deep-sea bottom in the region explored, save where its temperature is reduced nearly to the freezing-point, may be judged of from the fact that in one instance the dredge brought up half-a-ton of it from a depth of 767 fathoms. The resemblance of this deposit to chalk is greatly strengthened by the recognition of several characteristically cretaceous types among the foraminifera scattered through the mass of *Globigerinæ* of which it is principally composed; as also of the *Xanthidia*, frequently preserved in flints. Not many absolute novelties presented themselves among the foraminifera that form true calcareous shells; the chief point of interest being the occurrence of certain types of high organisation at great depths, and their attainment of a size that is only paralleled in much warmer latitudes, or in the Tertiary or yet older formations. This is especially the case with the Cristellarian group, which has a long geological range; and also with the Milioline, of which specimens of unprecedented size presented themselves. The most interesting novelty was a beautiful Orbitolite, which, when complete, must have had the diameter of a sixpence, but which, from its extreme tenuity, always broke in the process of collection. Of Arenaceous Foraminifera, however, which construct tests by cementing together sand-grains, instead of producing shells, the number of new types is such as seriously to task our power of inventing appropriate generic names. Many of these types have a remarkable resemblance to forms previously known in the chalk, the nature of which had not been recognsied. Some of them throw an important light on the structure of two gigantic Arenaceous types from the upper greensand, recently described by the speaker and Mr. H. B. Brady, an account of which will appear in the forthcoming part of the "Philosophical Transactions;" and there is one which can be certainly identified with a form lately discovered by Mr. H. B. Brady in a clay-bed of the carboniferous limestone.

The question now arises, whether—as there must have been deep seas in all geological periods, and as the changes which modified the climate and depth of the sea-bottom were for the most part very gradual—we may not carry back the continuity of the accumulation of Globigerina-mud on some part or other of the ocean bed into geological epochs still more remote; and whether it has not had the same large share in the production of the earlier calcareous deposits, that it has undoubtedly had in that of the later. The foraminiferal origin of certain beds of the carboniferous limestone, for example, appears to be indicated by the presence of *Globigerinæ*, long since observed by Professor Phillips in sections of them, as well as by the fact just stated. The sub-crystalline character of these rocks cannot be regarded as in any way antagonistic to such an idea of their origin, since it is perfectly well known that all traces of the organic origin of calcareous rocks may be completely removed by subsequent metamorphism, —as in the chalk of the Antrim coast.

What is the source of nutriment for the vast mass of animal life covering the abyssal sea-bed, is a question of the greatest biological interest. That animals have no power of themselves generating the organic compounds which serve as the materials of their bodies—and that the production of these materials from the carbonic acid, water, and ammonia of the inorganic world, under the influence of light, is the special attribute of vegetation —is a doctrine so generally accepted, that to call it in question would be esteemed a physiological heresy. There is no difficulty in accounting for the alimentation of the higher animal types, with such an unlimited supply of food as is afforded by the *Globigerinæ* and the sponges in the midst of which they live, and on which many of them are known to feed. Given the Protozoa, everything else is explicable. But the question returns,—on what do these Protozoa live?

The hypothesis has been advanced that the food of the abyssal Protozoa is derived from diatoms and other forms of minute plants, which, ordinarily living at or near the surface, may, by subsiding to the depths, carry down to the animals of the sea-bed the supplies they require. Our examination of the surface-waters, however, has afforded no evidence of the existence of such mycrophytic vegetation in quantity at all sufficient to supply the vast demand; and the most careful search in the Globigerina-mud has failed to bring to light more than a very small number of specimens of these siliceous envelopes of Diatoms, which would most assuredly have revealed themselves in abundance, had these Protophytes served as a principal component of the food of the Protozoa that have their dwelling-place on the sea-bed. Another hypothesis has been suggested, that these Protozoa which are so near the border of the vegetable kingdom, may be able, like plants, to generate organic compounds for themselves, manufacturing their own food, so to speak, from inorganic materials. But it is scarcely conceivable that they could do this without the agency of light; and as it is obviously the want of that agency which excludes the possibility of vegetation in the abysses of the ocean, the same deficiency would prevent animals from carrying on the like process.

A possible solution of this difficulty, offered by Professor Wyville Thomson in a lecture delivered last spring, has received so remarkable a confirmation from the researches made in the *Porcupine* expedition, that it may now be put forth with considerable confidence. It is, he remarked, the distinctive character of the Protozoa, that "they have no special organs of nutrition, but that they absorb water through the whole surface of their jelly-like bodies. Most of these animals secrete exquisitely-formed skeletons, sometimes of lime, sometimes of silica. There is no doubt that they extract both of these substances from the sea-water, although silica often exists there in quantity so small as to elude detection by chemical tests. All sea-water contains a certain amount of organic matter in solution. Its sources are obvious. All rivers contain a large quantity; every shore is surrounded by a fringe, which averages about a mile in width, of olive and red sea-weeds; in the middle of the Atlantic there is a marine meadow, the Sargosso Sea, extending over 3,000,000 of square miles; the sea is full of animals which are constantly dying and decaying; and the water of the Gulf Stream, especially, courses round coasts where the supply of organic matter is enormous. It is, therefore, quite intelligible that a world of animals should live in these dark abysses: but it is a necessary condition that they should chiefly belong to a class capable of being supported by absorption through the surface, of matter in solution; developing but little heat, and incurring a very small amount of waste by any manifestation of vital activity. According to this view, it seems highly probable that at all periods of the earth's history some form of the Protozoa—rhizopods, sponges, or both—predominated over all other forms of animal life in the depths of the sea; whether spreading, compact, and reef-like, as in the Laurentian and Palæozoic *Eozoon*; or in the form of myriads of separate organisms, as in the *Globigerinæ* and *Ventriculites* of the chalk."*

During each cruise of the *Porcupine*, samples of sea-water obtained from various depths, as well as from the surface, at stations far removed from land, were submitted to the Permanganate test, after the method of Prof. W. A. Miller, with an addition suggested by Dr. Angus Smith for the purpose of distinguishing the organic matter in a state of decomposition from that which is only decomposable; with the result of showing the uniform presence of an appreciable quantity of matter of the latter kind, which, not having passed into a state of decomposition, may be assimilable as food by animals, being, in fact, protoplasm in a state of extreme dilution. And the careful analyses of larger quantities collected during the third cruise, which have been since made by

* This was believed at the time to be an entirely new discovery; but since the return of the *Porcupine* we have learned that a type generically, if not specifically, the same, had been obtained by Count Pourtales during his most recent dredgings in the Gulf of Mexico, and had been described by Mr. Alex. Agassiz under the name *Pourtalesia miranda*.

* "The Depths of the Sea," a lecture delivered in the theatre of the Royal Dublin Society, April 10, 1869,

Dr. Frankland, have fully confirmed these results, by demonstrating the highly azotized character of this organic matter, which presents itself in samples of sea-water taken up at from 500 to 750 fathoms' depth, in such a proportion that its universal diffusion through the oceanic waters may be safely predicted.

Until, therefore, any other more probable hypothesis shall have been proposed, the sustenance of animal life on the ocean-bottom at any depth may be fairly accounted for on the supposition of Prof. Wyville Thomson, that the protozoic portion of that fauna is nourished by direct absorption from the dilute protoplasm diffused through the whole mass of oceanic waters, just as it draws from the same mass the mineral ingredients of the skeletons it forms. This diffused protoplasm, however, must be continually undergoing decomposition, and must be as continually renewed; and the source of that renewal must lie in the surface-life of plants and animals, by which (as pointed out by Prof. Wyville Thomson) fresh supplies of organic matter must be continually imparted to the oceanic waters, being carried down even to their greatest depths by that liquid diffusion which was so admirably investigated by the late Professor Graham.

Not only, however, has the nutrition of the abyssal fauna to be explained; its respiration also has to be accounted for; and on this process also the results of the analyses of the gases of the sea-water made during the *Porcupine* expedition throw very important light. Samples were collected not only at the surface, under a great variety of circumstances, but also from great depths; and the gases expelled by boiling were subjected to analysis according to the method of Prof. W. A. Miller—the adaptation of his apparatus to the exigencies of ship-board having been successfully accomplished during the first cruise bv Mr. W. L. Carpenter. The general average of thirty analyses of surface-water gives the following as the percentage proportions:—25·1 oxygen, 54·2 nitrogen, 20·7 carbonic acid. This proportion, however, was subject to great variations, as will be presently shown. As a general rule, the proportion of oxygen was found to diminish, and that of carbonic acid to increase, with the depth; the results of analyses of intermediate waters giving a percentage of 20·0 oxygen, 52·8 nitrogen, and 26·2 carbonic acid; whilst the results of analyses of bottom-waters gave 19·5 oxygen, 52·6 nitrogen, and 27·9 carbonic acid. But bottom-water at a comparatively small depth often contained as much carbonic acid and as little oxygen as intermediate water at much greater depths; and the proportion of carbonic acid to oxygen in bottom-water was found to bear a much closer relation to the abundance of animal life (especially of the more elevated types), as shown by the dredge, than to its depth. This was very strikingly shown in an instance in which analyses were made of the gases contained in samples of water collected at every fifty fathoms, from 400 fathoms to the bottom at 862 fathoms, the percentage results being as follows:—

	750 fath.	800 fath.	Bottom 862 fath.
Oxygen	18·8	17·8	17·2
Nitrogen	49·3	48·5	34·5
Carbonic Acid	31·9	33·7	48·3

The extraordinarily augmented percentage of carbonic acid in the stratum of water here immediately overlying the sea-bed was accompanied by a great abundance of animal life. On the other hand, the lowest percentage of carbonic acid found in bottom-water, viz. 7·9, was accompanied by a "very bad haul." In several cases in which the depths were nearly the same, the analyst ventured a prediction as to the abundance, or otherwise, of animal life, from the proportion of carbonic acid in the bottom-water; and his prediction proved in every instance correct.

It would appear, therefore, that the increase in the proportion of carbonic acid and the diminution in that of the oxygen, in the abyssal waters of the ocean, is due to the respiratory process, which is no less a necessary condition of the existence of animal life on the sea-bed, than is the presence of food-material for its sustenance. And it is further obvious that the continued consumption of oxygen and liberation of carbonic acid would soon render the stratum of water immediately above the bottom completely irrespirable—in the absence of any antagonistic process of vegetation—were it not for the upward diffusion of the carbonic acid through the intermediate waters to the surface, and the downward diffusion of oxygen from the surface to the depths below. A continual interchange will take place at the surface between the gases of the sea-water and those of the atmosphere; and thus the respiration of the abyssal fauna is provided for by a process of diffusion, which may have to operate through three miles or more of intervening water.

The varying proportions of carbonic acid and oxygen in the surface-waters are doubtless to be accounted for in part by the differences in the amount and character of the animal life existing beneath; but a comparison of the results of the analyses made during the agitation of the surface by wind, with those made in calm weather, showed so decided a reduction in the proportion of carbonic acid, with an increase in that of oxygen, under the former condition, as almost unequivocally to indicate that superficial disturbance of the sea by atmospheric movement is absolutely necessary for its purification from the noxious effects of animal decomposition. Of this view a most unexpected and remarkable confirmation has been afforded by the following circumstance:—In one of the analyses of surface-water made during the second cruise, the percentage of carbonic acid fell as low as 3·3, while that of oxygen rose as high as 37·1; and in a like analysis made during the third cruise, the percentage of carbonic acid was 5·6, while that of oxygen was 45·3. As the results of every other analysis of surface-water were in marked contrast to these, it became a question whether they should not be thrown out as erroneous; until it was recollected that, whilst the samples of surface-water had been generally taken from the bow of the vessel, they had been drawn in these two instances from abaft the paddles, and had thus been subjected to such a violent agitation in contact with the atmosphere, as would pre-eminently favour their thorough aëration.

Hence, then, it may be affirmed that every disturbance of the ocean-surface by atmospheric movement, from the gentlest ripple to the most tremendous storm-wave, contributes, in proportion to its amount, to the maintenance of animal life in its abyssal depths —doing, in fact, for the aëration of the fluids of their inhabitants, just what is done by the heaving and falling of the walls of our own chest for the aëration of the blood which courses through our lungs. A perpetual calm would be as fatal to their continued existence as the forcible stoppage of all respiratory movement would be to our own. And thus universal stagnation would become universal death.

Thus it has been shown that the bed of the deep sea, even in the immediate neighbourhood of our own shores, is an area of which the conditions have until lately been as completely unknown us those of the ice-bound regions of the poles, or of the densest forests, the most arid deserts, the most inaccessible mountain-summits, that lie between the tropics; and further, that by the systematic employment of the sounding-apparatus, the thermometer, and the dredge, almost as complete a knowledge can be gained of those conditions, as if the explorer could himself visit the abyssal depths he desires to examine. Of the important discoveries in almost every department of science, but more particularly in what Mr. Kingsley has well termed Bio-Geology, which may be anticipated from the continuation and extension of an inquiry of which the mere commencement has yielded such an abundant harvest, the speaker felt it scarcely possible to form too high an expectation. And, in conclusion, he referred to the systematic and energetic prosecution of deep-sea explorations by the United States Coast Survey and by the Swedish Government —the results of which prove to be singularly accordant with those now briefly expounded—as showing that other maritime powers are strongly interested in the subject; and expressed the earnest hope that the liberal assistance of Her Majesty's Government, which has already enabled British naturalists to obtain the lead in this inquiry, would be so continued as to enable them to keep it in the future. In particular, he called attention to the suggestion lately thrown out by M. Alex. Agassiz, that an arrangement might be made by our own Admiralty with the naval authorities of the United States, by which a thorough survey, physical and biological, of the North Atlantic should be divided between the two countries; so that British and American explorers, prosecuting in a spirit of generous rivalry labours most important to the science of the future, might meet and shake hands on the Mid-Ocean.

W. B. CARPENTER

NOTE.—Tables I. and II. on the following page give the Temperature of the Sea at different Depths—(I.) in the Channel between the North of Scotland, the Shetland Isles, and the Faroe Islands (the Roman Numericals indicate the *Lightning* Temperature-Soundings, corrected for pressure); and (II.) near the Western margin of the North Atlantic Basin, as ascertained by *Serial* and by *Bottom* Soundings.

TABLE I.

Warm Area.						Cold Area.						
Series 87.						Series 64.		Ser. 52				
Depth.	Temperature.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.	Depth.	Temperature.	Temperature.	Station No.	Depth.	Surface Temperature.	Bottom Temperature.
f.h.	°		fths.	°	°	fths.	°	°		fths.	°	°
0	52·6					0	49·7	52·1				
50	48·1	73	84	52·7	48·8	50	45·5	48·5	70	66	53·4	45·2
		80	92	53·2	49·4				69	67	53·5	43·8
100	47·3					100	45·0	47·3	68	75	52·5	44·0
		71	103	53·0	48·6				61	114	50·4	45·0
		81	142	53·3	49·1				62	125	49·6	44·6
150	47·0	84	155	54·3	49·2	150	43·3	46·5	60	167	49·5	44·3
		85	190	53·9	48·7				IX.	170	52·0	41·0
200	46·8					200	39·6	45·6				
		74	203	52·5	47·7							
						250	34·3	38·4				
300	46·6					300	32·4	30·8				
									63	317	49·0	30·3
									65	345	52·0	29·9
									76	344	50·3	29·7
		50	355	52·6	46·2	350	31·4	..	54	363	52·5	31·4
		46	374	53·9	46·0	384	..	30·6				
400	46·1					400	31·0	..	86	445	53·6	30·1
		89	445	53·1	45·6	450	30·6	..				
		90	458	53·1	45·2				56	480	52·6	30·7
		49	475	53·6	45·4				53	490	52·1	30·0
500	45·1					500	30·1	..	X.	500	51·0	30·8
		VI.	530	52·5	44·8				58	540	51·5	30·8
		47	542	54·0	43·8				VIII.	550	53·0	29·8
		XV.	570	52·0	43·5	550	30·1	..	77	560	50·9	29·8
									59	580	52·7	29·7
600	43·0					600	29·9	..				
		XVII	620	52·0	43·5				55	605	52·6	29·8
		XIV.	650	53·0	42·5				57	632	52·0	30·5
						640	29·6	..				
700												
		88	705	53·5	42·7							
767	41·4											

TABLE II.

Serial Soundings								Bottom Soundings.			
Depth.	Temperature. Ser. 23.	Temperature. Ser. 42.	Temperature. Ser. 22.	Temperature. Ser. 19.	Temperature. Ser. 20.	Temperature. Ser. 21.	Temperature. Ser. 38.	Statn. No.	Depth.	Surface Temperature.	Bottom Temperature.
fths.	°	°	°	°	°	°	°		faths.	°	°
0	57·3	62·6	56·9	54·8	55·5	56·2	64·0				
50	..	53·2						27	54	55·6	48·3
								34	75	66·0	49·7
								6	90	54·0	50·0
								35	96	63·4	51·3
100	48·5	51·1						8	106	54·2	51·2
								24	109	57·7	46·5
150	..	50·9						7	159	53·2	50·4
								14	173	53·2	49·6
								18	183	53·2	49·4
200	48·0	50·5						13	208	53·6	49·6
250	..	50·2	48·5	48·0	48·5	48·3	50·5	4	251	53·5	49·5
300	47·8	49·6									
350	..	49·1						1	370	54·0	49·0
400	47·5	48·5									
450	..	47·6						15	422	52·2	47·0
								45	458	60·7	48·1
500	45·8	47·4	46·7	46·7	46·9	47·5	47·8				
								40	517	63·4	47·7
550	..	46·4									
600	44·5	45·5						41	584	63·4	46·5
630	43·4										
650	..	44·3									
700	..	43·6						12	670	52·2	42·6
								3	723	54·5	43·0
								36	725	63·9	43·9
750	..	42·5	42·0	41·2	41·6	42·4	41·3				
800	..	42·0									
								2	808	54·1	41·4
862	..	39·7						16	816	53·0	39·5
900								44	865	61·2	39·4
1000	..	..	38·8	38·5	38·8	38·5	38·3				
1200								43	1207	61·7	37·7
1263	..	..	37·3								
								28	1215	57·7	37·1
1250	..	..	..	..	37·7	37·9	37·7	17	1230	53·2	37·8
1300								29	1264	56·9	36·9
1360	..	..	..	37·4							
1400											
1443	..	..	..	..	37·0			32	1320	55·9	37·4
1450								30	1380	56·0	37·1
1476	..	..	..	..	..	36·9					
1500	..	..	..	..	..	..	37·2				
1750	..	..	..	..	..	..	36·7				
2090	..	..	..	..	..	..	36·3	37	2435	65·6	36·5

PHYSICS

Mechanical Theory of Heat

WE translate the following passages from a paper by Dr. Meyer, of Heilbronn :—

It has been inferred from the meteorite theory, which supposes the sun to derive its heat from the impact of planeto-kosmic masses, that the entire machine of creation must eventually come to a standstill. I gladly seize the opportunity which now offers itself, to state that I do not share this view. The doctrine of the development of heat by the collision of spatially separate masses, has but just arisen, has therefore advanced but little, and cannot yet serve as an appropriate foundation for so comprehensive a consequence. I will briefly state what may be said, from my own point of view, as to the stability of the universe. Its final cessation will occur, when all the ponderable matter it contains is combined in a single mass; whereupon, as we may readily perceive, the whole of its existing *vis viva* would be uniformly distributed in the form of heat throughout the mass, which would thus attain an eternal equilibrium.

But how could such a combination happen? Five years have passed since Brayley, of London (and Reuschle just recently in a number of the German quarterly journal), stated, that if masses of the magnitude of our sun, or only half as great, were to come into collision, so enormous would be the effect, that all cohesion would be at an end, and the molecules would fly off into infinite space. Now we have every reason to suppose that, in the ceaseless course of time, and in an unlimited expanse, this kind of destruction or partial ruin of worlds has taken place, and is actually in progress. We have a striking proof of it in the observation of meteorites with a hyperbolic path. On this point I would refer to the important memoir of Prof. Heis, of Münster. "The large fire-ball which was seen on the evening of March 4, 1863, in Holland, Germany, Belgium, and England (Halle, 1863)." The true heliocentric motion of this meteorite amounted to 9.145 geographical miles per second. A body lying between the earth's orbit and the sun, and owing its motion solely to the attraction of the latter, cannot have a greater rate than 5·8 geographical miles; so that the fire-ball above referred to must have entered the sphere of attraction of our sun with an initial velocity of 7 geographical miles per second. Now, whence could it have derived such a motion?

In order to throw light on this question, we might imagine a peculiar progressive movement of the whole solar system in space, or have recourse to a movement round a so-called central sun. But we cannot suppose any such accumulation exists sufficiently large to confer an appreciable velocity on our sun at the distance of the fixed stars. Moreover, if our earth possessed a distant motion towards space in addition to its centripetal motion towards the sun, the light which reaches it from the fixed stars would present phenomena of aberration different from those actually observed. Were this proved, meteors with a hyperbolic path would be so many fiery couriers, living witnesses of a conflict somewhere and sometime happening in strength sufficient to explode and scatter the molecules in every direction. If we also consider that the radiating power of the sun's body, as of all the fixed stars, is connected with the consumption of collided masses, yet that consumption has not therefore ceased, since throughout the disturbance, large masses of debris continually reach our world.

All the phenomena of terrestrial motion, except volcanic action and the ebb and flow of the tides, are eventually derived from the sun. One of these, which we are about to consider more particularly, is an electric current on the surface of the earth. That it actually exists is evident from the direction of the magnetic needle, as also from the immediate observations of Lamont. But as there can be no action without corresponding cause, it follows that this remarkable expenditure of electric effort must be attended with as large a compensation. We have, then, to consider our earth as being, in this respect, a huge and permanently efficient electric machine. I do not here refer to the local phenomenon of thunderstorms.

For a constant source of the constant disturbance of electrical equilibrium in the earth's body, we can only have recourse to the unceasing flow of air between the tropics, known under the name of the trade-winds. The lowest layer of the trade-wind assumes, by friction on the surface of the sea, an opposite electrical condition. This air, however, heated by the sun, and dislodged by the colder current setting beneath it, rises and directs its course to the poles, where its high electric tension originates the beautiful phenomenon of the aurora. It must now be observed that, on account of the physical condition of the earth's surface, the electromotor activity of the southern hemisphere must be throughout much stronger than in the northern; whence it happens, that not only on both hemispheres between pole and equator, but also between the north and south poles themselves, a continual disturbance of electric equilibrium occurs; and it is this by which the direction of the needle is determined. The narrow belt between the north and south-east trades—called by Dove the zone of calms—may be termed, for present purposes, the meteorological equator. This is known not to coincide with the geographical equator, but to oscillate slowly about a limit of 1 to 1½ degrees north of it. The *experimentum crucis* for the theory—or, as we will only term it at present the hypothesis—here adduced of the trade-winds as the source of terrestrial magnetism, would consist in establishing that the known alterations which the magnetic pole, as well as declination, gradually undergo, are accompanied by parallel changes of our magnetic equator. But work of this description cannot be accomplished by a single private individual, and I must content myself with having brought the subject forward.

Amagat on the law of Mariotte

PROFESSOR E. H. AMAGAT has published the results of some experiments, still in progress, on the influence of temperature on departures from the law of Mariotte. The researches of Regnault have shown that this law is not rigorously obeyed by any gas excepting hydrogen; in all other cases compressibility increases with pressure, that is, when the gas approaches its temperature of ebullition. This phenomenon has received various explanations. It has been considered as resulting from reciprocal molecular attraction; it has also been elucidated by a theory which was first enunciated by Daniel Bernouilli, but has received successive additions at the hands of Joule, Krœnig, and Clausius. The theory in question takes into account not only the movements of translation of molecules, but their rotatory and internal movements, as well as the possible movements of imponderable fluids. If we admit the first explanation, then, as attraction only depends on the mean distance of the molecules, the departure from the law in any single case must be the same at any temperature, provided the initial and final volumes are the same. In other words, let V be a given volume of gas at the temperature t and pressure p. Reduce this volume to V′ by a pressure p', the temperature remaining unchanged. On heating the gas to t, it will expand; let P be the pressure necessary to restore the volume to V, and P′ the corresponding pressure. If the departure be only a function of the volume, it is clear that we must have

$$\frac{pV}{p'V'} = \frac{PV}{P'V'}.$$

As $\frac{V}{V'}$ is common to both sides of this equation, it is only necessary to compare $\frac{p}{p'}$ with $\frac{P}{P'}$. The author has done this in the case of sulphur dioxide, ammonia, and carbon dioxide. In the instance of sulphur dioxide—

$$\left.\begin{array}{l}\text{at } 14^\circ,\ \frac{p}{p'} = 0\cdot50838 \\ \text{at } 98^\circ,\ \frac{P}{P'} = 0\cdot50277\end{array}\right\}\ \text{difference, } 0\cdot00561.$$

(This difference corresponds to an observed height of more than one centimetre of mercury.) For ammonia—

$$\left.\begin{array}{l}\text{at } 13^\circ,\ \frac{p}{p'} = 0\cdot50731 \\ \text{at } 97^\circ,\ \frac{P}{P'} = 0\cdot50402\end{array}\right\}\ \text{difference, } 0\cdot00329.$$

For carbonic dioxide—

$$\left.\begin{array}{l}\text{at } 13^\circ,\ \frac{p}{p'} = 0\cdot50981 \\ \text{at } 97^\circ,\ \frac{P}{P'} = 0\cdot50402^*\end{array}\right\}\ \text{difference, } 0\cdot00210.$$

It appears from the preceding numbers that the departure is not only a function of the volume, but also of the temperature at which the experiment is performed. This result agrees, however, with the second theory. In fact, the *vis viva* of the molecules being greater as the temperature rises, it may be readily conceived that the loss due to their collision is relatively smaller than the augmentation of pressure on the walls of the enclosing vessel, due to the augmentation of *vis viva*, this being true even when, as the rate is accelerated, the molecular collisions become more numerous.

In a new series of experiments, M. Amagat kept the initial and final pressures as nearly as possible the same in each case, thus obtaining the influence of temperature alone. He then arrived at the following general results:—

1. That near 100°, sulphur dioxide and ammonia depart but little from Mariotte's law, yet more so than air at the ordinary temperature.
2. That near 100°, carbon dioxide is almost a perfect gas.
3. That near 100°, air exactly follows the law.

The author is convinced that the higher the temperature of liquefaction of a gas is found to be (under the same pressure), the less does it depart from the law of Mariotte at the same distance from its point of liquefaction.*—[Archives des Sciences physiques et naturelles, 139, p. 169.]

* This number is obviously a misprint.

* With the above results compare those obtained by Andrews (Proceedings of the Royal Society, xviii. 42).

SOCIETIES AND ACADEMIES

London

Linnean Society, March 17.—Mr. Carruthers exhibited a section of a fossil *Osmunda* from the eocene beds of Herne Bay, in which not only the forms of the cells were preserved, but the contents of the cells, and even the starch-granules. Before its conservation it had been attacked by a parasitic fungus, the mycelium of which is preserved, in precisely the same condition as it would be in a recent specimen.—Dr. Hooker read a further communication from Sir Henry Barkly on the Flora and Fauna of Round Island. The highest point of the island is 1,049 feet above the level of the sea; the summit is smooth, with three large and remarkable blocks of granite. It is entirely composed of tufa, mixed with volcanic sand in perfectly preserved strata. The deeper ravines are crowded with lofty palms. Of the twenty-six flowering plants gathered, the greater number belong to the orders *Gramineæ*, *Pandanaceæ*, *Palmaceæ*, *Ebenaceæ*, *Cinchonaceæ*, *Compositæ*, and *Asclepiadeæ*. The proportion of Endogens to Exogens is very large, namely, twelve to fourteen; but this proportion by no means represents the enormous preponderance of the former in individuals, probably amounting to 99 per cent. Some of the Exogens are specifically identical with those of the Mauritius, but few of the Endogens; those of the former class which are common to the two islands have probably been introduced at some remote period. Of the three cryptogamic plants observed, one was a moss, probably a *Sphagnum*, one a *Selaginella*, certainly a new species, and one a widely-spread fern, *Adiantum caudatum*. Of the five grasses the most abundant is identical with the Indian Lemon-grass. The *Cyperaceæ* are represented by one species, *Scirpus maritimus*. The *Pandanaceæ* are very remarkable; *Pandanus utilis* occurs, but in one spot only, rare, and no doubt introduced, whilst the other, an allied species (*P. Vandermeerckii*, is quite peculiar to the islet). Of Palms there are no less than three species, probably all peculiar, the most remarkable being the bottle-stemmed species (a *Hyophorbe*) already described as peculiar to the island. The only other Endogen belongs to the order *Liliaceæ*, and is an aloe, growing on the summit, and probably a new species. Of *Ebenaceæ* there are three species, and two Asclepiads with trailing stems; one species of *Myrsineæ*, new; two *Compositæ*, one of them a *Sonchus*, both probably introduced; one species of *Combretaceæ* and one of *Myrtaceæ*; two *Cinchoneæ*, and a small tree about twelve feet high, resembling the *Blackwellia* of Mauritius. It will be seen that while the genera of the Round Island Flora are Mauritian, the species are mostly peculiar. It is probable that the whole group of islands—Mauritius, Bourbon, Round Island, Ile de Serpents, Rodriguez, with the smaller islets, and probably Madagascar—are fragments of a vast continent. As regards the Fauna, there are no indigenous mammalia, although goats and rabbits have been introduced and have multiplied exceedingly, and no land birds, not even the Mauritian pigeons. The island seems, on the other hand—perhaps from the absence of mammalia and birds—very favourable to reptile life. Of Chelonians, a female land-tortoise had previously been captured on the island. Four distinct Saurians were found, the largest exceeding a foot in length, a native of

Madagascar, but not of Mauritius or Bourbon; one species, at least, is altogether new. The four Ophidians are all undescribed, no authentic evidence being known of any such having at any time been indigenous to the Mauritius group. No Batrachians were discovered, and the time was too short to collect the Fish which abounded in the freshwater pools. Only one Gasteropod was collected, probably *Cyclostoma hemistoma*. Of Arachnida, the spiders are numerous and interesting, of four kinds, belonging to as many different families, two common to Mauritius and two not; also three scorpions, one measuring five inches, none of them Mauritian. Only one Myriapod was captured, a centipede six inches in length, belonging to India, but found also in Rodriguez. There were six Coleoptera, none of them Mauritian, though not very dissimilar. Of Orthoptera, one *Plasma*, peculiar to the island, and a grasshopper, also thought to be new. The Neuroptera included only one specimen of a dragon fly, and the Hymenoptera only a single bee. Generally speaking, the Fauna was of the type of the Malayan archipelago, with greater resemblance to that of Madagascar than of Mauritius or Bourbon. The reptiles have been sent to Mr. Gunther of the British Museum for examination and description, and specimens of the plants to the Kew Herbarium. Thus it will be seen that this little islet, not a mile in diameter, and only thirteen distant from the great island of Mauritius, is unique in respect of the peculiarity of both its animal and vegetable productions. In the matter of Ophidians it is especially so; the absence of them in other oceanic islets throughout the globe being one of the most remarkable features of their history.

Royal Geographical Society, March 14.—Sir R. I. Murchison, Bart., president, in the chair. The following new Fellows were elected:—Charles Ashton; William J. Anderson; Louis Alford; Charles Fairbridge; Charles W. Gray; Edward Gellatly; J. G. Gibson; T. D. Murray; Rev. W. R. Tilson Marsh, M.A.; M. the Chevalier de Overbeck; Robert T. Pigott; Albert Walker; Thomas Watson; Peter T. Wills. The President, Sir Roderick Murchison, read an official letter he had that day received from Lord Clarendon, stating that a severe outbreak of cholera had occurred in East Africa, at Zanzibar, and on the neighbouring mainland, which it was feared would delay the progress of Dr. Livingstone, inasmuch as the native carriers who were taking supplies to him had been attacked by the epidemic. Sir Roderick stated that there was little probability of the disease reaching the remote interior district where Livingstone remained waiting for the Zanzibar caravan. The following paper was read: "On Morrell's Antarctic Voyage, and on the advantages of steam navigation in future Antarctic Explorations," by Captain R. V. Hamilton, R.N. According to the author, a remarkable narrative of a voyage in high southern latitudes by Benjamin Morrell, in the *Wasp* sealing schooner, published at New York in 1832, had been hitherto overlooked by all concerned in Antarctic exploration. Even Morrell's celebrated countryman, Commodore Wilkes, seemed not to have been aware of this publication, which appeared before he sailed on his voyage of discovery. Captain Hamilton had laid down Morrell's route on a South Polar chart, and found that it intersected several times the land said afterwards to have been discovered by Wilkes. The portion of the Antarctic Ocean navigated was between 66° and 70° 14' lat., and between 105° E. long. and the meridian of Greenwich. South of 64° he found less ice, and in 69° 11' there was no field-ice visible. Captain Hamilton concluded that the Antarctic lands seen by Wilkes and others were mostly islands, and that one or other of them would offer a suitable site for the observation of the approaching Transit of Venus. The employment of steam-vessels, he contended, would add very greatly to the safety of the expedition as well as the facility of reaching the high southern latitudes. The great barrier of ice surrounding the South Polar lands, he believed, was not glacier ice, but an enormous floe. In the discussion which followed, Commander J. E. Davis (of Sir James Ross's Expedition) dissected many of Morrell's statements about well-known places in high southern latitudes, and showed that they were almost all pure fiction: he considered his work to be, therefore, of no authority, and denied that it had been overlooked; it had been examined by cartographers and writers, and set aside as unreliable. Mr. Enderby expressed similar opinions, from personal knowledge of Morrell, and Mr. F. Galton also exposed Morrell's inaccuracy with regard to the interior of South-west Africa. Captain Sherard Osborn differed in opinion from Captain Hamilton regarding the formation of the Antarctic icy barrier, and believed it to be the seaward edge of an enormous continental glacier. Admiral Ommaney also took part in the discussion. The meeting was then adjourned to March 28.

Anthropological Society, March 15.—Dr. Charnock, V.P. in the chair. Mr. William Stephens Haywood, Long Wittenham, near Abingdon, Berks; and Mr. P. Henderson, her Majesty's Vice-Consul at Benghazi, North Africa, and No. 1 Stafford Place, Buckingham Gate, were elected Fellows. Dr. Daniel Earl Burdett was elected a local secretary for Belleville, Ontario, Canada. The following paper, by Dr. Isidore Kopernicky and Dr. J. Barnard Davis, F.R.S., was read: "On the strange peculiarities observed by a religious sect of Moscovites, called Scoptsi." This curious Christian sect of a well-defined race was fully described in the paper by Dr. Barnard Davis from data supplied him by Dr. Kopernicky of Bucharest, and it was accompanied by an anatomical preparation which clearly demonstrated the character and amount of mutilation practised by the Scoptsi. That practice is based upon the twelfth verse of the nineteenth chapter of St. Matthew, and it has been carried out with such resolution and to so large an extent, that the Russian Government has been compelled to interfere and to punish with extreme severity all members proved to belong to that community. Hence, the Scoptsi are forced to conduct their worship and to carry out their peculiar rites in the most secret manner: nevertheless, they contrive to amass great wealth, and as a consequence they possess considerable influence in districts in which they reside. Accident alone brought under the notice of Dr. Kopernicky the case of the individual whose body furnished the preparation laid before the society. The paper, after entering at length into the modes of conducting the religious worship of the Scoptsi, their estimated numbers, their physical characteristics and other details, viewed the subject in its psychical aspect. Dr. Kopernicky was of opinion that this aberration in Christianity could not be explained otherwise than by the psychological peculiarity of the race of Moscovites in which it prevails. He endorsed the well-known views of the Rev. Dunbar Heath upon the difference which exists between the Semitic and "Aryan" races in their appreciation of the doctrines of Christianity, and held it to be an anthropological fact that the ideas and religious creeds, sound or absurd, moral or immoral, which are produced, or which develope themselves among a certain race, depend greatly upon the character of the psychological sentiments natural to that race. That was the reason why Christianity was so readily accepted, and has taken such root among the Aryan peoples, and why, on the contrary, the Koran has had most success and most persistence among the Semites. An animated discussion ensued, in which the Rev. Dunbar Heath, Mr. Moncure Conway, Mr. Ralston, Dr. Spencer Cobbold, and others, took part.

Institution of Civil Engineers, February 22.—The following papers were read:—"On the New Mhow-ke-Mullee Viaduct, Great Indian Peninsula Railway," by Mr. A. R. Terry; "On the Pennair Bridge, Madras Railway," by Mr. C. W. Stoney.

March 1.—"The Wolf-Rock Lighthouse." By James N. Douglass.

March 8.—"Description of the Line and Works of the San Paolo Railway in the Empire of Brazil." By D. M. Fox.

March 22.—"On the conditions and the limits which govern the proportions of Rotary Fans." By R. Briggs, of Philadelphia.

DUBLIN

Royal Irish Academy, March 16.—The Rev. John H. Jellett, M.A., was elected president, and the following gentlemen were elected council and officers for the current year:—Dr. W. K. Sullivan, Secretary of Academy; Dr. H. Hennessy, Dr. W. Stokes, Dr. A. Searle Hart, Dr. James Apjohn, Rev. Humphrey Lloyd, D.D., Rev. S. Haughton, M.D., Rev. J. A. Galbraith, Dr. MacDonnell, Dr. E. Perceval Wright, Mr. R. S. Ball; John T. Gilbert, Librarian; William H. Hardinge, Treasurer; Dr. John Kells Ingram, Secretary of Council; Sir W. R. Wilde, Secretary of Foreign Correspondence; Rev. George Longfield, D.D., Dr. Samuel Ferguson, Dr. W. J. O'Donnavan, Dr. Alexander G. Richey, Colonel Meadows Taylor, John R. Garstin. Heinrich Ewald, of Göttingen, was elected an honorary member in the department of Polite Literature. The following grants of money were voted:—20*l*. to Dr. John Barker, in aid of his experiments on "Microscopic Illumination." 15*l*. to Mr. E. Reynolds, to enable him to carry out his researches on the "Spectrum Analysis of Chlorine," &c. 15*l*. to Mr. N. Furlong, to enable him to carry out his experiments on the "Innervation of the Heart." Professor Hennessy read a note on "Two Streams flowing from the same source in opposite directions." The president nominated Henry Hennessy, F.R.S., William Stokes, M.D., Sir William R. Wilde, M.D., and Samuel Ferguson, LL.D., vice-presidents for the current year. The annual report was read and adopted, and then the Academy adjourned.

GLASGOW

Natural History Society, February 22.—Mr. David Robertson, F.G.S., vice-president, in the chair. Mr. Thomas Chapman exhibited specimens of *Venilia Macularia* which he had captured in June last in the Pass of Leny, Perthshire, and the Rev. James E. Somerville stated that he had taken the species in some numbers in Argyleshire, both at Loch Awe and Oban. The secretary exhibited a small collection of star-fishes, which had been forwarded from Girvan by Mr. Thomas Anderson, corresponding member.—Mr. Duncan McLellan exhibited monstrosities of the common ash and hawthorn from the Queen's Park; the former showing the twigs flattened like horns of a reindeer, the latter having its branches tortuous like a cork-screw. Both specimens presented a very unusual appearance. —Mr. Alexander Donaldson exhibited an example of malformation in the bill of a rook, regarding which Mr. Gray observed that it possessed additional interest from the fact of its showing only a partial abrasion at the base of the bill, and that it had been arrested probably in consequence of the malformation. Drawings of other malformations were exhibited by Mr. Gray. Mr. John Gilmour exhibited an unusually dark specimen of the hooded crow (*Corvus cornix*), which had the light space on the breast and shoulders very much clouded, giving the bird the appearance of a variety of the carrion crow (*Corvus corone*). Dr. Stirton exhibited specimens of *Adelanthus Carringtoni*—a Jungermannia new to science, which he had found on Ben Lawers, and other places. This moss had formerly been confounded with *Alicularia compressa*, from which, however, it differs not only in the colour and areolation of the leaves, but also in their mode of attachment to the stem. It approaches much more closely *Alicularia declusa* from Campbell's Island in the South Pacific; and as this last has been proved by Dr. Carrington to be an *Adelanthus*, it has been thought proper to refer this moss also to the same subgenus. The Rev. James E. Somerville than read a paper on *Danais chrysippus* and its food plant, *Asclepias gigantea*, with illustrative specimens from Upper Egypt. The author of this paper gave a very interesting account of this butterfly from personal observations made during a three months' residence in Egypt, and also of the plants on which it is known to feed. He likewise described the peculiar properties of the *Calotropis procera* or *Asclepias gigantea* of Linnæus—a plant better known as the apples of Sodom—a beautiful series of which, in its various stages of growth, was exhibited by Mr. Somerville in illustration of his remarks.

BOSTON

Boston Natural History Society, February 2.—Dr. B. Joy Jeffries states that, as at different times during the past three years he had had occasion to call the attention of the society to the physiology of accommodation in man and other animals, including birds, he would ask to be allowed to make a few remarks on a special part of the eye which is interested in, and may be employed in, accommodation. He illustrated his remarks by a series of pictures and diagrams representing sections of the human eye and a number of different animals, made through the ciliary muscle and the adjacent parts of the sclerotic, cornea and iris. From dissections made by many anatomists, and the special studies of several physiologists, it resulted that the space in the eye hitherto known as the canal of Fontana, who first described it in 1778, is now proved not to be a canal with walls, but rather a triangular space between the ciliary muscle, iris, and sclerotic or cornea, filled by a sort of mesh-work attaching the iris to the last-named membrane. This mesh-work is cut off from the aqueous humour. It constitutes the ligamentum pectinatum iridis, and is quite distinct from the circular venous sinus in the sclerotic just outside of it, which it has apparently sometimes been mistaken for it. Dr. Jeffries discussed the question as to whether it took part in the accommodation of the eye, if not in man where it seemingly could not, in the lower animals where its size increases with the decrease of the ciliary muscle. He remarked that our present knowledge of it is due to the recent researches of Drs. Iwanoff and Rollett.

Section of Microscopy, January 12.—Mr. Stodder referred to a communication of Mr. R. C. Greenleaf, on a specimen of *Aulacodiscus oreganus* Bail., prepared by Mr. Samuels, which in the process of mounting separated into two plates; one being the outer, and the other the inner plate of one valve. A few days since a similar thing happened to Mr. Samuels when mounting another specimen of the same species. The diatom separated into two pieces, the inner and outer plates of one valve as Mr. Samuels supposed. But a careful inspection of the specimen which was exhibited to the section, indicated an entirely different origin. One disc was a perfect *A. oreganus*, with all the characters of that species, having ten rays, and "feet." The other was more hyaline, the umbilicus less distinct, the granules and "feet" imperfectly developed, and having eleven rays and "feet." Mr. Stodder's explanation of the appearances—if Mr. Samuels was not mistaken as to the facts—is that the one disc is the parent, and the other a valve of a new frustule, which was forming in the process of self-division, the growth of which was stopped before it had come to maturity. Ehrenberg and some other naturalists have made the number of rays in such forms a specific character; Bailey and others have rejected this principle of classification, but here for the first time we have positive evidence that a form with eleven rays has been derived directly from one of ten rays. Such a change of characters in one order of plants being authentically established, it is a reasonable inference that all other orders may be liable to similar changes, and therefore great caution should be used in allowing specific value to unimportant characters.

January 26.—*Section of Entomology*.—Mr. E. Burgess in the chair. Twelve persons were present. Mr. F. G. Sanborn exhibited a drawing of the larva of *Callosamia promethea*, made by the late Mr. C. A. Shurteff, together with the specimen after it had spun its cocoon. Dr. H. Hagen read a criticism of the views of Dr. Packard concerning the *Neuroptera*, as given in his recently completed "Guide," and explained that in the manuscript of his own "Synopsis of North American Neuroptera" he had, in accordance with the views of the most prominent entomologists for twenty-eight years, distinctly separated the Pseudoneuroptera and Neuroptera as two different parts of the work. Dr. Hagen also remarked that Mr. Fritz Müller had sent to him some white ants from Itahahy, St. Catharina, Brazil, with the following remarks:—"These nests of white ants are more or less regular cylinders, one span high and two or three inches thick. By horizontal floors they are divided into twelve or fifteen compartments or chambers. The outer surface bulges out so that one can make out the number of chambers by the enlargements of the cylinder. A pillar goes through all the compartments; close to this, or in it, runs an oblique passage from each chamber to the next. Sometimes all these passages together form a somewhat regular winding stair through all the compartments. For the impregnated female these passages are too narrow, and she can therefore not leave her chamber. There are, both in the outer wall and in the horizontal divisions, passages too small to admit the passing of the winged ants; but neither in the outside wall nor in the chambers is there any opening to the outside in nests which have not been injured. In the outside wall the passages run from top to bottom. In the divisions, from circumference to centre without reaching this latter. In the flat compartments they are not to be detected from the outside; in the circumference they appear as flattened ridges. In drying, the outer side of the passages falls off, and then they are to be seen as deep hollows with inflated borders. In undisturbed nests the only entrance seems to be on the upper surface some inches under ground. The nest is not directly connected with the earth, but is surrounded by about a finger's breadth of free space. The nest can, therefore, as soon as the upper end is freed from earth, be easily taken out of the ground. I have never found in one of these nests more than one impregnated female. Besides the winged ants, the eggs, and the larvæ, there are found two kinds of labourers; of these, one kind is distinguished by a truncated nose. Not in the nest, but in the same piece of land, are found, in planting corn, single white ants with disproportionately large heads and long mandibles." The winged ants were stated by Dr. Hagen to belong to *Termes striatus*, or perhaps to *T. similis*; the imago is in too bad a condition for accurate determination. The soldier with truncated nose was figured by him as *T. similis*; the soldier with long mandibles as *T. cingulatus*. No description of white ants' nests like this has ever been given before.—Mr. S. H. Scudder remarked that in a recent examination of the external genital armature of our diurnal *Lepidoptera*, he had noticed the extraordinary fact that in the males of the North-American species of the genus *Nisoniades*, these organs were asymmetrical. The asymmetry is confined to the lower lateral plates, which are unusually developed in this genus, and shows itself in the diverse length of the lower process and in the size, and the entireness or the excision of the lateral flap. The only species in the genus, as generally accepted, which does not come under this rule, is *N. Catullus*, but the structural features of all the appendages of the body of this species show that it is wrongly placed in this relation. Mr. Scudder also stated that the butterfly described by Dr. Harris in his State Report as *Eudamus Bathyllus*,—a name invariably accepted by subsequent writers—was not the species originally described and figured by Abbot and Smith under the same specific name; he therefore proposed to call Harris's species *Eudamus Pylades*. Mr. Sprague referred to an instance related by a friend not versed in entomology, where "flies" were seen, through a hole in the ice in midwinter, to ascend in large numbers from the bottom of a stream to the surface and take flight. Mr. B. P. Mann stated that he had taken a specimen of *Carabus Chamissonis* Fisch., in Labrador. Mr. F. G. Samborn remarked that he had taken ten or twelve specimens of the same species in August, on the sides of Mount Washington, N.H., at a height of from four to five thousand feet above the sea. He also reported the capture in Andover, Mass., on Christmas Day, 1869, of *Capnia* and *Tæniopteryx*, moving actively upon the ice; of several *Staphylinidæ* of the genera *Lathobium*, *Stenus*, *Philonthus* and *Lithocharis*, together with *Photinus corruscus* and larvæ of *Telephorus*, and some undetermined Coleopterous and Geometrideous larvæ, also a species of *Salda* (Hemipterous), and of *Diptera*, *Hydrophorus pirata* Loew, and *Sepsis sp.*, which were struggling in water of about one-eighth inch in depth, covering the surface of the ice in meadows. A great number of *Arachnidæ*, mostly of small size, were noticed under the same circumstances, and appeared to represent many species. He was in pursuit of the aberrant forms, *Boreus* and *Chionea*, but several hours of careful search failed to reveal any specimens of either.

PARIS

Academy of Sciences, March 21.—The following papers relating to various departments of physics were read: A note on the variations of the calorific capacity of water towards the maximum of density, by M. Hirn; on the angle of adjustment of a liquid with a solid wall, by M. Moutier; a description of a vertical galvanometer with a balance, suitable for use before large audiences, by M. Bourbouze.—The chemical papers were rather numerous, and included a note on the analysis and uses of the rock known in the Ardennes under the name of *gaize*, or *pierre-morte*, by MM. H. Sainte-Claire Deville and J. Desnoyers, upon which M. Elie de Beaumont made some remarks.—A note by M. Descloiseaux upon some crystallised derivatives of the coal hydrocarbons; a memoir on the action of sulphuret of carbon and carburetted gases upon wood charcoal, by M. Sidot; a note on cobalt and manganese and their alloys with copper, by M. A. Valenciennes; a note on a new method of preparing hydrobromic acid, by MM. Champion and Pellet; a note on the properties of iodic acid, by M. A. Ditte; one on the hydrogenated derivatives of sulphuret of carbon, by M. A. Girard; a note on the vitality of beer-yeast, by M. Melsens; an important note by M. J. Raulin on the chemical conditions of the life of the lower organisms; a paper on tribromhydrin, by M. L. Henry; and a note on the isomeric xylenes and cumenes in the coal-oils, by M. Rommier. —M. Rosenstiehl also presented a paper on the nature of the motor force which produces the phenomena of endosmose; and M. E. Martin an electro-chemical investigation of ozone.—M. Blanqui forwarded a letter describing an instrument for solving spherical triangles without the aid of tables of logarithms; and M. Bowen a continuation of his communication relating to the distance of the sun, of which the titles only are given.—M. Chasles made known a theorem relating to the theory of surfaces which had been communicated to him by Mr. Spottiswoode.—M. Coumbary's notice of the fall of an aerolite in Barbary (given in our last number) was communicated by M. Le Verrier, who also presented some observations on storms in Norway during the year 1869, by M. Mohn of Christiania.—With the exception of a few medical miscellaneous notes, three botanical papers complete the list of communications at this meeting: these were the continuations of M. Trecul's and M. Chatin's valuable researches upon the tracheæ of ferns, and the causes of the dehiscence of anthers (the latter completed), and a notice of a remarkable case of subdivision of the top of a palm-tree, by M. Ramon de la Sagra.

DIARY

THURSDAY, March 31.

Royal Society, at 8.30.—On the relation between the Sun's Altitude and the chemical intensity of total daylight in a cloudless sky: Prof. Roscoe and Dr. Thorpe.—On the acids contained in Crab-oil: Mr. W. J. Wonfor.
Society of Antiquaries, at 8.30.—On the Crypt of the Chapter-house at Westminster: H. Harrod, F.S.A.
Royal Institution, at 3.—Chemistry of Vegetable Products: Prof. Odling.
London Institution, at 7.30.—Geology: Dr. Cobbold.

FRIDAY, April 1.

Royal Institution, at 8.—Artificial Alizarine: Prof. Roscoe.
Archæological Institution, at 4.

SATURDAY, April 2.

Royal Institution, at 3.—The Sun: J. Norman Lockyer, F.R.S.

MONDAY, April 4.

London Institution, at 4.—Chemistry: Prof. Bloxam.
Royal Institution, at 2.—General Monthly Meeting.
Entomological Society, at 7.
Medical Society, at 8.
Royal Asiatic Society, at 4.
Victoria Institute, at 8.—On Comparative Psychology: E. J. Morshead.

TUESDAY, April 5.

Anthropological Society, at 8.—Phallic Worship: H. M. Westropp:—The Influence of the Phallic Idea in the Religion of Antiquity: C. Staniland Wake.
Royal Institution, at 3.—Nervous System: Prof. Rolleston, M.D., F.R.S.
Institution of Civil Engineers, at 8.—Discussion on St. Pancras Station.—On the Dressing of Lead Ore: Thomas Sopwith, jun., Memb. Inst. C.E.

WEDNESDAY, April 6.

Society of Arts, at 8.

THURSDAY, April 7.

Royal Institution, at 3.—Chemistry: Prof. Odling.
Chemical Society, at 8.—On the Analysis of Deep-sea Water: Dr. John Hunter.—On the refraction equivalents of the aromatic Hydrocarbons and their derivatives: Dr. J. H. Gladstone.—On an acid Feed-water from the Coal-fields of Shellarton, N.S., and the results of its use: Prof. How.
Linnean Society, at 8.—On new species of Annelids, &c.: Dr. Baird.—On Algæ from the North-Atlantic Ocean: Dr. Dickie.

BOOKS RECEIVED

English.—A Poor Man's Photography at the Great Pyramid: Prof. Piazzi Smyth (H. Greenwood).—The Week of Creation: G. Warington (Macmillans).—The Philosophy of the Bath: D. Dunlop (Dublin, Moffat).—The Fuel of the Sun: W. Mattieu Williams (Simpkin, Marshall, and Co).

Foreign.—Grundzüge der Modernen Chemie: Dr. Eugen Zell, Organische Chemie (Berlin, Herschwald).—Grundriss der Physik und Meteorologie: Dr. J. Müller (Brunswick, Vieweg).—L'Année Géographique; révue annuelle: M. Vivien de Saint-Martin (Paris, Hachette).—Reden und Abhandlungen über Gegenstände der Himmelskunde: Dr. J. H. von Mädler (Berlin, Oppenheim).—Jahresbericht über die Fortschritte der Chemie: Adolph Strecker, für 1868, 1tes Heft (Giessen, Ricker).—Charles Darwin und Alfred Russel Wallace: Dr. A. B. Meyer (Erlangen, Befold).—Die Stellung des Menschen in der Natur; 2te Lieferung Wer sind wir: Dr. L. Büchner (Leipzig, Thomas).—Zeitschrift der Gesellschaft für Erdkunde zu Berlin, 4ter. Band: Prof. W. Koner (Berlin, Reimer).—Studien über die Wanderblöcke und die Diluvialgebilde Russlands: C. von Helmersen, 10 Tafeln (St. Petersburgh, Eggers). Through Williams and Norgate.——Cryptogamie Illustrée, ou Histoire des Familles naturelles des Plantes Acotyledonées d'Europe: Casimir Bourneguère (Paris, Baillière).—Gedachtnissrede von Alexander von Humboldt: C. G. Ehrenberg (Berlin, Oppenheim).

CONTENTS

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